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VOLUME 1

EUROPEAN THOUGHT IN THE NINETEENTH CENTURY

Τοιοῦτος οὖν μοι ὁ συγγραφεὺς ἔστω, . . .
ξένος ἐν τοῖς βιβλίοις καὶ ἄπολις.
—LUCIAN.

A HISTORY
OF
EUROPEAN THOUGHT
IN THE
NINETEENTH CENTURY

BY

JOHN THEODORE MERZ

VOL. I

THIRD UNALTERED EDITION

WILLIAM BLACKWOOD AND SONS
EDINBURGH AND LONDON
MCMVII

PREFACE.

As the plan of this work is fully given in the Introduction, only a few points, chiefly of a personal character, remain to be touched on here.

The first refers to the motive which led me to a course of studies, extending over more than thirty years, of which this book is the outcome.

The object of the book is philosophical, in the sense now accepted by many and by divergent schools—*i.e.*, it desires to contribute something towards a unification of thought. When in the beginning of my philosophical studies I became convinced that this is the task of philosophy, I felt the necessity of making myself acquainted, at first hand, with the many trains of reasoning by which, in the separate domains of science, of practical and of individual thought, such a unification has been partially and successfully attempted. Such a survey seemed to me indispensable. The possession of a map showing the many lines of thought which our age has cultivated seemed to me the first requisite, the basis from which a more complete

unification would have to start. The following pages contain the result of this survey. Like every survey, it can claim to be merely an approximation. It gives outlines which closer scrutiny will have to correct and fill up.

My original intention was to complete this survey in three volumes, corresponding to the three divisions of the subject set out in the Introduction.

Some of my friends, who desired that the publication of the book should not be unduly delayed, considered that the Introduction and the earlier chapters of the work would give something intelligible in themselves, and urged the advantage of smaller volumes. I therefore decided to complete the first part of the history, which deals with scientific thought, in two volumes instead of in one.

For the information of my readers, I mention here that the two last chapters of this volume, which treat of the astronomical and of the atomic views of Nature, will be followed in the second volume by similar chapters on the mechanical, the physical, the biological, the statistical, and the psychophysical views of Nature, and that it is my intention to close the first part of my subject by an attempt to trace concisely the development of mathematical thought in this century.

My thanks are due to many friends who have supported me with assistance and encouragement.

I consider myself fortunate in having secured for the revision of the whole volume the invaluable aid of Mr Thomas Whittaker, B.A., whose profound erudition, know-

ledge of ancient and modern literature, and great editorial experience, were well known to my late friend Professor Croom Robertson, during his successful editorship of the first series of 'Mind.'

Mr S. Oliver Roberts, M.A., of the Merchant Taylors' School, has kindly read over the fourth, and Professor Phillips Bedson, of the Durham College of Science of this city, the last, chapter of this volume. The Introduction has greatly benefited by a thorough revision by my brother-in-law, Dr Spence Watson, a master of the English language.

I must also thank him and Dr Thomas Hodgkin for having given me what I value as much as assistance—namely, encouragement.

One indeed to whom I am in this respect more indebted, perhaps, than to any one else—whom to have known has meant, for many, a revelation of the power of mind and the reality of spirit—is no more: Ernst Curtius. While I was writing the last pages of this volume, in which he took a warm interest, the tidings arrived that he had passed away. But she who was nearest and dearest to him is still with us—a true priestess of the higher life, who has kept burning in the soul of many a youthful friend the spiritual fire when it was in danger of being quenched by the growing materialism of our age.

J. THEO. MERZ.

THE QUARRIES,
NEWCASTLE-UPON-TYNE, *November* 1896.

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A HISTORY OF EUROPEAN THOUGHT IN THE NINETEENTH CENTURY.

INTRODUCTION.

I.

BEHIND the panorama of external events and changes which history unfolds before our view there lies the hidden world of desires and motives, of passions and energies, which produced or accompanied them; behind the busy scenes of Life lie the inner regions of Thought. Only when facts and events cease to be unconnected, when they appear to us linked together according to some design and purpose, leading us back to some originating cause or forward to some defined end, can we speak of History in the sense which the word has acquired in modern language; and similarly do the hidden motives, desires, and energies which underlie or accompany the external events require to be somehow connected, to present themselves in some order and continuity, before we are able to grasp and record them.

1. Thought, the hidden world.

That which has made facts and events capable of being chronicled and reviewed, that which underlies and connects them, that which must be reproduced by the historian who unfolds them to us, is the hidden element of Thought. Thought, and thought alone, be it as a principle of action or as the medium of after-contemplation, is capable of arranging and connecting, of combining what is isolated, of moving that which is stagnant, of propelling that which is stationary. Take away thought, and monotony becomes the order.

2. Thought, the only moving principle.

3. History of Nature, how to be understood.

This assertion may seem bold to many, who would look rather to the grand phenomena of Nature than to the narrow limits of man's activity. A few remarks will, however, suffice to show that my proposition is not opposed to the view which they take. It may be urged that, independent of human life altogether, the earth has a history, the planetary system has a development, and that, according to modern theories, evolution is the principle which governs inanimate as well as animated nature; that rest and sameness are nowhere to be found, everywhere change and unrest. But change and unrest do not necessarily constitute history. Motion and change would be as monotonous as absolute rest, were they merely to repeat themselves endlessly, did the whole movement not produce something more, and were this something more not greater or better than the beginning. But greater and better are terms which imply comparison by a thinking beholder, who attaches to one thing a greater value than to another, judging by certain ideal standards, which are not in the objects or process of nature themselves, but are contained only in his own think-

4. Not intelligible without intellect.

ing mind. It may be that a mechanical and mindless series of changes can produce numbers without end, or forms of countless variety: but this process would deserve the name of history only if either the transition from unity to multiplicity, or the production of formal variety, were capable of being understood by a thinking mind,—if the result of the process were a matter of some concern, if an interest were attached to it, if a gain or loss could be recorded. The pendulum which swings backwards and forwards in endless monotony, the planet which moves round the sun in unceasing repetition, the atom of matter which vibrates in the same path, have for us no interest beyond the mathematical formulæ which govern their motions, and which permit us mentally to reproduce, *i.e.*, to think them. A combination of an infinite number of these elementary movements would have as little interest, were it not that out of such a combination there resulted something novel and unforeseen: something that was beautiful to behold or useful to possess, something that was valuable to a thinking mind in a higher or lower meaning of the word.

But if, even in inanimate nature, the processes of change acquire an interest, possess a history, only if referred to a thinking mind which can record, understand, and appreciate them, how much more is this the case when we deal with human affairs, where man is not only the thinking beholder but the principal agent? Here the historic interest would cease, were the succeeding years and ages to produce no valuable change, were the rule of existence and the order of life to repeat themselves in unceasing monotony. The savage tribes of Africa have a history: but

5. History of savage tribes, what is it?

this history is all known when the order of the day, the year, at most of a generation, is known. Even the highly complicated but stagnant life of China would have a short historical record—many thousands of years taking up no more space than as many days of modern European history:

"Better fifty years of Europe than a cycle of Cathay."

6. Two ways in which Thought enters into History.

Thus it is that Thought becomes in two ways a subject of great interest and importance to the historian. Of every change in nature or human life we can ask: What has been its result in the world of thought? What gain or loss, what progress, has it worked in the minds of men, of us the beholders? Has it increased our knowledge, enriched our stock of ideas, deepened our insight, broadened our views and sympathies—in one word, has it added to our interests? has it made larger and fuller our inner life?

And of every change in human affairs we can ask this further question: What part has thought, the inner life, played in this change? These two questions mark the task of the historian of Thought.

7. Definition of Thought impossible.

I do not think it necessary or practicable at this stage to explain minutely the terms with which we have so far been dealing. Many a one might be tempted to ask for a definition of Thought, or for a preciser statement of the actual relation between Nature, Life, and Thought.[1]

[1] In refusing to define what I mean by Thought, I take up the opposite position to that occupied by Prof. Max Müller in his latest work, 'The Science of Thought,' London, 1887, p. 1, where he says: "I mean by Thought the act of thinking, and by thinking I mean no more than combining. I do not pretend that others have not the right of using Thought in any sense which they prefer, provided only that they will clearly define it." So far as definition is at all a part of the work of the historian, I maintain that it is the result and

Such definitions must be left to the reader himself, if in course of the perusal of these volumes he finds it necessary to form abstract theories on these points. Any definition given now would inevitably involve us in controversies, which would be embarrassing and confusing. I rely upon the general and undefined sense of the word Thought, assuming that every one will connect some intelligible meaning with it, some meaning which will enable him to understand the very general proposition with which we started, the existence of an inner or hidden world behind the world of external events and facts, the continually changing nature of this inner world, and the connection and reaction between the two worlds. Whether in time and in importance the outer or the inner world is the first, whether within the latter equal value attaches to the clearer province of Reason, *i.e.*, defined Thought, to the obscurer regions of Feeling and Imagination, and to the unconscious world of Impulse, these are questions which it is not necessary to answer at present. As it was enough to point to the existence of the two worlds of Life and Thought, so it will be enough to notice that thought does not mean merely defined, clear, methodical thought, but likewise the great region of desire, impulse, feeling, and imagination, all of which play, we must admit, a great part in the inner life of the soul as well as in that of the outer world.

8. Relation of outer and inner world undefined.

9. Many meanings of Thought.

outcome of his narrative, the impression which he leaves on the mind of the reader when he has perused the work. History is not mainly a science which proceeds by analysis; it is the attempt to collect and arrange in a living picture an enormous mass of detail. Too rigid definitions, like lines which are too hard and marked, spoil the total effect.

In this sense of the word we have in the following treatise to deal with the History of Thought: not, however, with the history of thought in general, but with that of a defined period, with that of the present age and the age immediately preceding it,—the age, in fact, to which the writer and his readers belong, of which they have a personal knowledge and recollection more or less wide and intimate. It is the latter circumstance which has made me select this special portion of the history of thought; for it is that portion of which, it seems to me, I and my contemporaries should—if we go about it in the right way—know most. As every person is his own best biographer, so it seems to me every age is, in a certain sense, its own best historian.

10. Thought of the present age.

We know that this has been frequently denied so far as external events (that which many persons call history *par excellence*) are concerned. Contemporary writers do not, it is stated, get beyond mere records of events, records at once one-sided, incomplete, and confusing. It is indeed necessary to have the records in great number and variety: because the true and real record can only be given by him who combines all these many records into one, who avoids the errors arising from special points of view, from narrowness of outlook, from individual ignorance, blindness, or prejudice. Still, in spite of such defects, the contemporary records will always remain the most valuable sources for the future historian who may succeed in sifting their various testimonies, combining and utilising them to produce a fuller and more consistent picture of the bygone age. But while his work may be only temporarily valuable, theirs is

11. Contemporary history, to what extent possible and valuable.

lasting. It is hardly doubtful that, after hundreds or thousands of years have passed, the simple, detailed, and perhaps contradictory, narratives of contemporary witnesses will outlive those more elaborate and artistic efforts of the historian which are so largely inspired and coloured by the convictions of another—*viz.*, his own—age. For as Goethe has remarked: "History must from time to time be rewritten, not because many new facts have been discovered, but because new aspects come into view, because the participant in the progress of an age is led to standpoints from which the past can be regarded and judged in a novel manner." [1]

Most of the great historians whom our age has produced will, centuries hence, probably be more interesting as exhibiting special methods of research, special views on political, social, and literary progress, than as faithful and reliable chroniclers of events; and the objectivity on which some of them pride themselves will be looked upon not as freedom from but as unconsciousness on their part of the preconceived notions which have governed them. But where the facts recorded and the mind which records them both belong to the same age, we have a double testimony regarding that age. The events, and the contemplating mind, supplement each other to form a more complete picture, inasmuch as the matter and the medium through which it is viewed belong to the same time. And so it comes to pass that historians like Thucydides, Tacitus, and Machiavelli are looked upon as

12. Supposed Objectivity of historians.

[1] 'Materialien zur Geschichte der Farbenlehre,' Werke, 2te Abtheilung, Band 3, p. 239. I quote from the new edition, brought out by the German Goethe Society.

perfect models in the art of writing history, and the memoirs of many modern statesmen are more lastingly valuable than the more elaborate and connected narratives of remote and secluded scholars.

13. Value of contemporary records, both of Facts and Thought.

But if the contemporary record of facts will always have a peculiar value, however incomplete it may be, still more must this be the case with the contemporary record of thought; especially if thought means the whole of the inner life of an age, not merely that portion which in the form of defined thought has been incorporated in the written literature of the age. For a large portion of this hidden life is known only to those who have taken part in it.

14. Mystery of the Life of Thought.

The vague yearnings of thousands who never succeed either in satisfying or expressing them, the hundreds of failures which never become known, the numberless desires which live only in the hearts of men or are painted only in their living features, the uncounted strivings after solutions of practical problems dictated by ambition or by want, the many hours spent by labourers of science in unsuccessful attempts to solve the riddles of nature,—all these hidden and forgotten efforts form indeed the bulk of a nation's thought, of which only a small fraction comes to the surface, or shows itself in the literature, science, poetry, art, and practical achievements of the age.

15. Latent Thought the material for genius.

Equally important, though not equally prominent, this large body of forgotten thought has nevertheless been that which made the measure full, which heaped the fuel ready for the match to kindle; it constitutes the great propelling force which, stored up, awaits the time and aid of individual talent or genius to set it free. Philosophers tell us of

the wastefulness of organic life, of the thousands of germs which perish, of the huge volume of seed scattered uselessly. A similar fate seems to fall on the larger portion of intellectual and moral effort; but here a deeper conviction tells us that it is not the sacrifice but the co-operation of the many which makes the few succeed, that excellence is the prize of united effort, that many must run so that one may reach a higher goal. What other feeling could console those legions of honest workers who spend their lives in trying to deal with the seemingly unconquerable host of social evils, the apparently growing vice and misery of large towns, who raise a cry for oppressed nationalities, or preach against the curses of war and militarism? Or what higher and unselfish satisfaction could an author derive from spending half a lifetime in producing a work which in the end may fall dead-born from the press, if it were not the conviction that in the cause in which he has failed another after him may succeed, and that his failure may be a portion of the silent and hidden efforts that co-operate towards a useful end?[1] But who in after-ages can write the history of this forgotten and hidden work of a nation? Whose historical sense is delicate enough to feel where the pressure was greatest and the effort longest ere the new life appeared, whose eye penetrating and discerning enough to follow up the dim streaks

[1] "Sehen wir nun während unseres Lebensganges dasjenige von anderen gelestet, wozu wir selbst früher einen Beruf fühlten, ihn aber, mit manchem andern, aufgeben mussten, dann tritt das schöne Gefühl ein, dass die Menschheit zusammen erst der wahre Mensch ist, und dass der Einzelne nur froh und glücklich sein kann, wenn er den Muth hat, sich im Ganzen zu fühlen."—Goethe, 'Wahrheit und Dichtung,' 9th Book; Werke, 27, 277.

of twilight, dazzled as he must be by the blaze of the risen sun? We who live in the expectation of the light which is to come, surrounded by the shadows, difficulties, and obstacles; we who belong to the army, and are not leaders, who live in, not after, the fight,—we claim to be better able to tell the tale of endless hopes and endeavours, of efforts common to many, of the hidden intellectual and moral work of our age.[1]

16. Contemporary record of Thought more faithful.

How far back we who have lived during the second half of the present century may extend the period of which we claim to have a personal knowledge, is a point of further interest. Certain it is that in our parents and immediate forefathers we have known the representatives of a generation which witnessed and laboured in the interests of the great Anti-Slavery, the Reform, and the Anti-Corn-Law movements, who experienced the revolutions worked by the introduction of steam-power and gas, who took part in the great work of national and popular education abroad and in the reform of school-life in England. They themselves went through the enthusiasm of the anti-Napoleonic Revolution in Germany, came under the influence of Goethe's mature manhood, were fascinated by the stories from the pen of the Wizard of the North, par-

17. Events of the immediate past.

[1] Compare what A. de Tocqueville says, 'Œuv. comp.,' vol. viii. p. 170: "Nous sommes encore trop près des évènements pour en connaître les détails. Cela paraît singulier, mais est vrai. Les détails ne s'apprennent que par les révélations posthumes, contenues dans les Mémoires, et sont souvent ignorés des contemporains. Ce qu'ils savent mieux que la posterité, c'est le mouvement des esprits, les passions générales du temps, dont ils sentent encore les derniers frémissements dans leur esprit ou dans leur cœur; c'est le rapport vrai des principaux personnages et des principaux faits entre eux. Voilà ce que les voisins des temps racontés aperçoivent mieux que ne fait la postérité."

took of the spirit of the Romantic School, felt the electrical touch of Lord Byron's verse, listened to the great orators of the third French Revolution, and could tell us of the now forgotten spell which Napoleon I. exercised over millions of reluctant admirers. Most of these fascinations and interests live only in the narratives of contemporaries and surviving witnesses, few of whom have succeeded in perpetuating them with pen or brush, making them intelligible to a future age; most of them die with the generation itself. Not only have we listened to their words and seen in their features the traces of the anxieties they lived through, in their eyes the reflected enthusiasms and aspirations, in their glances and in the trembling of their voices the last quiverings of bygone passion and joy,—we have received from them a still more eloquent testimonial, a more living inheritance. But this we cannot hand down to our children in the form in which it was given to us: it has not passed through our hands unaltered. This inheritance is the language which our parents have taught us. Unknowingly they have themselves altered the tongue, the words and sentences, which they received, depositing in these altered words and modes of speech the spirit, the ideas, the thought of their lifetime. These words and modes of speech they handed to us in our infancy, as the mould wherein to shape our minds, as the shell wherein to envelop our slowly growing thoughts, as the instrument with which to convey our ideas. In their language, in the phrases and catchwords peculiar to them, we learnt to distinguish what was important and interesting from what was trivial or indifferent, the subjects which

18. Changes which Language undergoes from parent to child, a proof of the changing life of Thought.

should occupy our thoughts, the aims we should follow, the principles and methods which we should make use of. The bulk and substance of this they indeed inherited themselves; but the finer distinctions of their reasoning, the delicate shading of their feelings and aspirations, they added and modified for themselves, modelling for their own special use the pliable and elastic medium of the mother tongue. With this finer moulding we have inherited the spirit of the former generation: predisposing us to certain phases of thought and placing in our path a difficulty in acquiring otherwise than by gradual and almost imperceptible degrees the faculty of assimilating new and unexpected opinions, tastes, and feelings. Many of us adhere to the special character and phase of thought acquired in our youth. Some by learning foreign languages, and living in other countries, gain a facility for understanding quite different phases of thought: very few among us develop so much original thought that they burst the shell of conventional speech, coining new words and expressions for themselves, embodying in them the fleeting ideas of their time, the indefinable spirit of their age. Once expressed, these new terms are rapidly circulated, and if we look back on the period of a generation, we note easily the progress and development of opinion and tastes in the altered terms and style of our language.

19. Inadequacy of conventional speech for original thought. Coining of new words.

Thus it is that the writer, and those of his readers whose memory carries them back to the middle of the century, and whose schooling and education embodied the ideas of a generation before that time, can claim to have some personal knowledge of the greater portion of the nineteenth century, of the interests which it created and

the thoughts which stirred it.[1] It is the object of these volumes to fix, if possible, this possession; to rescue from oblivion that which appears to me to be our secret property; in the last and dying hour of a remarkable age to throw the light upon the fading outlines of its mental life; to try to trace them, and with the aid of all possible information, gained from the written testimonies or the records of others, to work them into a coherent picture, which may give those who follow some idea of the peculiar manner in which our age looked upon the world and life, how it intellectualised and spiritualised them. This attempt is therefore not a history of outward political changes or of industrial achievements: the former will probably be better known to our children than they have been to us; the latter will soon be forgotten as such, or incorporated in the still greater results of the future, for which they will be the preparation. Nor is it a history of Knowledge and Science, of Literature and Art, which I purpose to write; though as these are the outcome of the inner life, and contain it, so to say, in a crystallised form, they will always have to be appealed to for the purpose of verifying the conclusions which we may arrive

20. Object of this work to retrace the life of Thought through the dying century.

21. Not a political history, nor a history of Science, Literature, and Art.

[1] On the division of History into centuries see what Du Bois-Reymond says ('Reden,' Leipzig, 1886, vol. i. p. 519), and the fuller discussion of the subject by Prof. O. Lorenz, 'Die Geschichts-wissenschaft' (Berlin, 1886, p. 279 *sqq.*) The latter refers to what the first historian says (Herodotus, ii. 142: *Καίτοι τριηκόσιαι μὲν ἀνδρῶν γενεαὶ δυνέαται μύρια ἔτεα· γενεαὶ γὰρ τρεῖς ανδρῶν ἑκατὸν ἔτεά ἐστι*). A person born in 1840 can claim to have a personal knowledge of the last half, and through his parents and teachers a knowledge of the first half, of the century. In this way it may be said that his personal—direct or indirect—knowledge extends over nearly a century. Lorenz says correctly: "Für jeden einzelnen bildet der Vater und der Sohn eine greifbare Kette von Lebensereignissen und Erfahrungen." And that this applies even more to ideas and opinions, to Thought, than to events and facts, is evident.

22. Where the interest of the book will lie: in all the influences which have a result on our inner life.

at. What will interest us most will be the conscious aims and ends, if such existed, of any political or social movement, and, where they did not exist, at least the results to our inner life which have necessarily followed, the methods by which knowledge was extended or science applied, the principles which underlay literary composition and criticism, and the hidden spiritual treasure which poetry, art, and religious movements aimed at revealing or communicating; in fact the question: What part has the inner world of Thought played in the history of our century,—what development, what progress, what gain has been the result of the external events and changes?

23. The personal knowledge and experience necessary for a true portrayal forms a limitation of the extent of ground to be traversed.

But if personal knowledge and experience are—as it seems to me—of the greatest importance in an attempt like this; if, without having lived the inner life, a record of it would be either a mere string of names or a criticism of opinions, not a living picture,—so it is also the factor which necessarily limits the extent of the ground which I propose to traverse. Thus I feel obliged in the first place to limit myself to European Thought. Such a limitation would hardly have been called for a century ago, because it would have been a matter of course: but the steady growth and peculiar civilisation of a new and vigorous people on the other side of the Atlantic force from me the twofold confession, that there is a large world of growing importance of which I have no personal knowledge, and to estimate which I therefore feel unqualified and unprepared; and further, that I am equally unable to picture to myself the aspect which the whole of our European culture in its present state may assume to an outside and far-removed observer who is placed in the

24. American influence only touched upon.

New World. As this New World grows not only in numbers and national wealth, but also in mental depth, as it becomes more and more intellectualised and spiritualised, so it will no doubt experience the desire of recording its own inner life and culture, emphasising the peculiarities which distinguish it as a whole from our civilisation. But the tendencies of this new culture are to me vague and enigmatical, and I frankly admit that I am unable to say anything definite on this subject. Convinced as I am that in human affairs all outer life is the vessel which contains an inner substance, the shell which envelops a growing kernel, I am, nevertheless, unable in this case to penetrate to either, and must therefore content myself with taking notice of this vast new element of nineteenth-century culture only where it comes into immediate contact with European thought, which has indeed been powerfully influenced by it. And of European thought itself I am forced to select likewise only the central portion, the thought embodied in French, German, and English Literature. I have to admit that Italian, Scandinavian, and Russian influences are all around this centre, sometimes penetrating far into it; but here again languages unknown and interests foreign to me have made it impossible to identify myself ever so superficially with the new life that is contained in them. I must therefore here also confine myself to very imperfect and casual notices, which make no attempt to do justice to the subject.

25. Only French, German, and English thought the subject of the present work.

The subject before us, then, is European Thought—*i.e.*, the thought of France, Germany, and England—during the greater part of the nineteenth century. Circumscribed as

this subject is by the limits of time and space which I have mentioned, it is, nevertheless, still vast, intricate, and bewildering. And yet it is my intention, throughout the inquiries which I have to institute and in the various outlines and sketches which I have to draw, never to lose sight of the unity of the whole. This unity, I maintain, the progress of our age has more and more forced upon us. It is itself a result of the work of the century. A hundred years—even fifty years—ago, it would have been impossible to speak of European Thought in the manner in which I do now. For the seventeenth and eighteenth centuries mark the period in which, owing to the use of the several vernacular languages of Europe in the place of the mediæval Latin, thought became nationalised, in which there grew up first the separate literature and then the separate thought of the different civilised countries of Western Europe. Thus it was that in the last century, and at the beginning of this, people could make journeys of exploration in the region of thought from one country to another, bringing home with them new and fresh ideas. Such journeys of discovery, followed by importation of new ideas, were those of Voltaire[1] to England in 1726, where he found the philosophy of Newton and Locke, at that time not known and therefore not popularly appreciated in France; the journey of Adam Smith in 1765 to France, where he became acquainted with the economic system of Quesnay and the opinions of the so-called "physiocrats," which formed the starting-point of his own great work,

26. Unity of Thought a product of this century.

27. Voltaire.

28. Adam Smith.

[1] For a most complete collection of data referring to this subject see Du Bois-Reymond's address in the Berlin Academy, 30th January 1868, reprinted in the collection of his 'Reden,' Leipzig, 1886, vol. i.

'The Wealth of Nations.' During the last quarter of the eighteenth century A. G. Werner raised the Mining Academy at Freiberg, which had been founded in 1766, from a mere provincial institution to be one of the great centres of scientific light in Europe, to which students from all parts of the world flocked to listen to his eloquent teaching. Towards the end of the century Wordsworth and Coleridge went on a trip to Germany, whence the latter brought to England the new philosophy of Kant and Schelling. Madame de Staël, in an age when tidings of a new literary life in Germany had reached French Society through some of the emigrants of the Revolution, set herself reluctantly to learn German,[1] convinced that a new phase of thought had appeared there; and then with Benjamin Constant visited the country itself at the end of 1803, and again in 1807. The result of these journeys of exploration was her work 'De L'Allemagne.' Whilst Coleridge and Madame de Staël drew inspiration from the new life which centred in the Weimar of Goethe and Schiller, the scientific students of the whole Continent directed their gaze to Paris, where alone for many decades the modern methods could be learnt, where the new scientific ideas were, so to speak, collected in a focus. For more than half a century Paris remained the centre of scientific thought,[2] and even English philosophers, who

29. German thought brought to England by Coleridge and Wordsworth.

30. German thought imported into France by Madame de Staël.

31 Paris the focus of scientific ideas.

[1] See Lady Blennerhasset's interesting work on Madame de Staël, German ed., vol. ii. p. 461 *sqq.*; especially the remarkable passage quoted there, p. 465, in her letter to the Baron de Gérando, October 1802: "Ich glaube wie Sie, dass der menschliche Geist, der zu wandern scheint, jetzt bei Deutschland angelangt ist."

[2] See Bruhns, 'Life of A. v. Humboldt,' translated by Lassell, vol. i. p. 232: "Notwithstanding the sardonic expression of the frantic judge, 'Nous n'avons pas besoin de savans,' Paris was yet at the close

since Bacon and Newton had followed their own independent line of research, had to discover in the second decade of the century that Newton's great name was not a guarantee for the efficiency of his methods, which had been greatly developed and improved in the hands of Continental mathematicians. These improved methods were imported into England by three Cambridge graduates, Herschel, Babbage, and Peacock, who translated Lacroix's Treatise, and by doing so gave a great impetus to mathematical research in this country. Fifteen years later, students from all parts of the world flocked to the small University town of Giessen in Germany, thence to take home with them a knowledge of the new science and methods of Chemistry, taught in the laboratory of Liebig—methods previously used only in the private and inaccessible laboratories of learned investigators.[1] It will be in the memory of many how the philosophy of Auguste Comte, published between the years 1830 and 1840, remained without much influence in his own country, whereas, mainly through the writings of J. S. Mill and

32. Continental mathematical methods introduced into England by Babbage, Herschel, and Peacock.

33. Liebig's Laboratory.

34. Comte's philosophy shown to his own country by an Englishman.

of the eighteenth century the metropolis of the exact sciences. Lalande, in writing to von Zach on January 26, 1798, remarks: 'The love of mathematics is daily on the increase, not only with us but in the army. The result of this was unmistakably apparent in our last campaigns. Bonaparte himself has a mathematical head, and though all who study this science may not become geometricians like Laplace and Lagrange, or heroes like Bonaparte, there is yet left an influence upon the mind which enables them to accomplish more than they could possibly have achieved without this training. Our mathematical schools are good, and successfully accomplish their main object in the diffusion of mathematical knowledge.'" Compare also vol. i. p. 342, referring to 1804. Also vol. ii. p. 92, referring to the period 1820 to 1830. "Humboldt continued to regard Paris as the true metropolis of Science" (p. 70), and many other passages. See also Steffens, "Was ich erlebte," vol. x. p. 233, and what Goethe said to Eckermann on the contrast of Germany and Paris in the year 1827.

[1] See A. W. Hoffmann, 'The Life-Work of Liebig,' Faraday Lecture for 1875, p. 8.

his school, it became, as it were, a centre of thought, an embodiment of a circle of modern ideas in this country, whence it was reimported into France nearly a generation after its first appearance. Something similar happened to a once neglected but now renowned English landscape-painter, Constable, whose pictures when exhibited in France in 1824 created a profound sensation, and had such an influence on the artists of that country that they are said to mark an era in landscape-painting there.[1]

35. Constable's influence in France.

Such journeys of discovery in the realm of thought and ideas have now become almost impossible. In the course of our century Science at least has become international: isolated and secluded centres of thought have become more and more rare. Intercourse, periodicals, and learned societies with their meetings and reports, proclaim to the whole world the minutest discoveries and the most recent developments. National peculiarities still exist, but are mainly to be sought in those remoter and more hidden recesses of thought, where the finer shades, the untranslatable idioms, of language suggest, rather than clearly express, a struggling but undefined idea. Thought has its dawn and twilight, its chiaroscuro as well as its open day; but the daylight has grown wider and clearer and more dif-

36. Science become international.

[1] See Walter Armstrong in the 'Nineteenth Century' for April 1887; Julius Meyer, 'Geschichte der modernen französischen Malerei,' Leipzig, 1867, Book 7, chap. 2; A. Rosenberg, 'Geschichte der modernen Kunst,' vol. i. p. 63. Rosenberg thinks the influence of Constable on French Art is exaggerated, and mentions Paul Huet, whose early pictures date from 1822. But an Englishman, Bonington, who, however, is claimed as of the French School, was even before Huet and Constable. See also what Delacroix wrote to Th. Sylvestre in 1858: "Constable est une des gloires anglaises. C'est un véritable réformateur, sorti de l'ornière des paysagistes anciens. Notre école a grandement profité de ses examples et Géricault était revenu tout étourdi de l'un des grands paysages qu'il nous avait envoyés" (quoted by Emile Michel in 'Grande Encyclopédie,' art. "Constable").

fused in the course of our century, and so far as the greater volume of ideas is concerned, we can speak now of European thought, when at one time we should have had to distinguish between French, German, and English thought. Reserving, therefore, in the meantime the task of investigating what still, within the bounds of this larger international life, remains peculiar to the thought of each nation, it is the great body of common European thought with which I propose at first to deal. How has it grown to be what it is now, what special contributions have the several nations made to the general stock, what is at present our inventory of it, how has it been changed in course of the century? But how, it may be asked, are we to take stock? how is this inventory to be drawn up? There is indeed one very obvious method which presents itself, though it is not the one which I propose to use exclusively, or even largely. And yet it seems to me well worthy of special attention.

37. The light which Etymology throws on history of Thought,

Already I have remarked how the changes of thought are deposited in the altered language and style of the age. A closer study of the changes which, in the course of this century, have taken place in the vocabularies as well as in the styles of the three principal European languages would no doubt reveal to a great extent when and how new ideas have presented themselves, how they have become fixed and defined in special words or terms. It would allow us to trace to a very large extent not only the growth of the general stock of European thought, but also the migration of single ideas from one nation to

38. and on the migration of ideas.

another. And, lastly, it would exhibit to a great extent in what peculiar phrases, in what secluded corners, the

individual thought of each of the three nations has found refuge.[1] Any one who has attempted to translate from one of these languages into another, be it prose or be it lyrical, philosophical, or descriptive poetry, will have experienced the necessity of studying minutely the meaning or hidden thought which a word or a phrase may signify: he will have been led to notice what is common and what is peculiar to different languages,

[1] The only books which treat of words in the sense mentioned above, and which have come under my notice, are Horne Tooke's 'Diversions of Purley' and Archbishop Trench's little volumes on 'The Study of Words' and 'English Past and Present.' So far as the use of merely philosophical terms is concerned, I may refer to R. Eucken, 'Geschichte der philosophischen Terminologie,' Leipzig, 1879. A great deal of material for a research of this kind may be found in the large Dictionaries of Grimm, Littré, and Murray, though I do not feel sure that the great change which has come over language, through the expansion, deepening, and differentiation of ideas and of thought in our age, has been specially taken note of. The plan of Grimm's Dictionary, which aims at embracing the German language in its development during three centuries, beginning with Luther and ending with Goethe (see Wilh. Grimm's 'Kleinere Schriften,' vol. i. p. 508), almost excludes the period which I am reviewing.

It is interesting to remember that Diderot, the first writer who attempted to collect the great body of modern Thought and Learning into an encyclopædic whole, referred to Language very much in the same manner as we do now, a hundred and fifty years later. See the article "Encyclopédie," where Diderot says that a Dictionary is only an exact collection of titles, to be filled in by the Encyclopædia; and further on, p. 639: "Si l'on compte les hommes de génie, et qu'on les répande sur toute la durée des siècles écoulés, il est évident qu'ils seront en petit nombre dans chaque nation et pour chaque siècle, et qu'on n'en trouvera presqu'aucun qui n'ait perfectionné la langue. Les hommes créateurs portent ce caractère particulier. Comme ce n'est pas seulement en feuilletant les productions de leur contemporains qu'ils rencontrent les idées qu'ils ont à employer dans leurs écrits, mais que c'est tantôt en descendant profondément en eux-mêmes, tantôt en s'élançant au dehors, et portant des regards plus attentifs et plus pénétrans sur les natures qu'ils environnent, ils sont obligés, surtout à l'origine des langues, d'inventer des signes pour rendre avec exactitude et avec force ce qu'ils y decouvrent les premiers. C'est la chaleur de l'imagination et la méditation profonde qui enrichissent une langue d'expressions nouvelles: c'est la justesse de l'esprit et la sévérité de la dialectique qui en perfectionnent la syntaxe; c'est la commodité des organes de la parole qui l'adoucit; c'est la sensibilité de l'oreille qui la rend harmonieuse."

39. Goethe. and the thought which they express. Of Goethe it may be said that he created to a large extent the language and style of that which is best in the modern literature of his country. No such supreme influence belonging to a single individual can probably be found in any other German, French, or English writer in our century, for reasons which are obvious: but the great French novelists, the German metaphysicians, and the original poetical minds of modern England have enlarged and enriched the vocabulary of their respective languages, and have added a number of useful and novel modes of expression (*tournures, Wendungen*). Carlyle's influence has been great in introducing novel epithets, borrowed or imported frequently from the German. Matthew Arnold has laboured in a similar direction, his models being, besides Goethe and Heine, mostly French authors, such as Sainte-Beuve and the introspective school. Germany has been less fortunate

40. Peculiarity of the German Language. in extending her vernacular vocabulary: the facility which her language possesses of assimilating foreign words and using them almost without any alteration has done much to complicate German style, destroying its simplicity, its graces, the poetical element. It will, however, probably be found that by far the greatest accession to the vocabularies—though not to the finer modelling—of the modern languages has come from the influence of the

41. Growth in the meanings of words. sciences on general culture and literature. Well-known words, long in use, have at the same time through this influence acquired altered or more specific meanings.

The vaguer word "development" has been supplanted by "evolution." "Differentiation" has a definite philosophic—not only a mathematical—meaning. The word "positive" has, besides the logical signification, acquired at least two meanings which are very specific, and which it did not possess formerly. "Energy" has, besides the general meaning, and the philosophical one which Aristotle assigned to it, acquired a special meaning, having first in England and then abroad taken the place of "force" as a more correct and definable term. In connection with it, "correlation" and "conservation" are terms of very specific value. The word "fittest" and the phrase "struggle for existence" mean something different from what they meant fifty years ago. Then there are the terms "exact" and "science" themselves, which mean 42. New thought has found new words.
something different now from what they meant formerly. And coming out of the more recent doctrines of the limits of human and conscious individual knowledge, there are the words "unconscious," "unknowable," and "agnostic," which indicate whole trains of novel thought. It would indeed be an interesting and useful investigation to follow up to their origin the many new words and phrases, or the altered meanings of well-known and familiar words, in which the three principal European languages abound. It would be a methodical study of the changes which thought has undergone.

Nor need such an undertaking be based upon any particular or one-sided theory as to the connection of Civilisation, Thought, and Language. This century has not been wanting in such, from the extreme theory of 43. De Bonald's theory of revealed Language and Max Müller's Science of Language.
De Bonald,[1] who saw in Language an immediate Divine revelation, to the most recent and more scientific view

[1] De Bonald (1754-1840), 'Législation primitive,' Paris, 1802.

of Max Müller, who would absorb philosophy in the science of Language[1] in the same way as Astronomy has to many become merely "une question d'analyse." In a certain sense we can agree with both of these thinkers. Without discussing the vexed question of the origin of Language and Thought, to us as individuals, born in a civilised and intellectual age, words certainly came earlier than clear and conscious thought. The easy manner also in which, through the use of our parents' tongue, we became introduced into a complex and bewildering labyrinth of highly abstract reasoning is little short of a miraculous revelation. But, as I mentioned above, it is not my intention to study the development of European thought during this century by means of a close analysis of the changes and growth of the three principal languages. Such an enterprise would demand an amount of lexicographical knowledge possessed only by the authors of dictionaries like those of Grimm, Littré, and Murray. But though I am not qualified for such a task, there is one special point on which I cannot avoid being drawn into a grammatical discussion. It refers to the word Thought itself. How is the meaning which I and my readers connect with this word to be expressed in French and German? How are we to translate the word? The subject we deal with does not belong to England alone, but as much to France and to Germany: it must thus have a name in each of their languages. Now I believe that the word *pensée* expresses in French very nearly the same thing which we mean in English by thought. It is some-

44. Thought, how expressed in French and German.

[1] See his 'Science of Thought,' London, 1887, especially pp. 292 and 550.

what more difficult to find a corresponding word in German. I have for some time hesitated whether to use the word *Geist* or *Weltanschauung*, two terms frequently used to express the aggregate of the inner life of an age: but have finally resolved to use the word *Denken*, as this word lends itself to the same contrasts of Life and Action (*Leben und Handeln*), denoting the inner world, whereas the opposite of *Geist* is *Stoff* (matter), and *Weltanschauung*, though an expressive and untranslatable word, denotes rather the outcome, the result, of thought than thought itself. Passing from the word to the subject itself, I find that the greater definiteness of the term in the English language is accompanied also by a more abundant literature of the subject. The larger idea of a Philosophy of History is indeed due mainly to Continental thinkers, especially to Herder, Hegel, Comte, and Guizot, and Voltaire's 'Siècle de Louis XIV.' will always be the model of the historical picture of a period. Still it is—in my opinion—mainly the writings of Carlyle, Buckle, Draper, Lecky, Leslie Stephen, and, considering its size, perhaps more than all, Mark Pattison's 'Essay,'[1] which have fixed in our minds the meaning of the word Thought as the most suitable and comprehensive term to denote the whole of the inner or hidden Life and Activity of a period or a nation. I therefore put in a claim to start with the use of the English word, as sufficiently familiar to most of my readers, and request those who may object to the vagueness of the French

45. Philosophy of History due to Continental thinkers.

[1] See 'Essays and Reviews,' 'Tendencies of Religious Thought in England, 1688-1750,' by Mark Pattison; also Leslie Stephen's remarks on it in the Preface to his 'History of English Thought in the Eighteenth Century.'

and German equivalents to look for a definition of my intention in the English word "Thought." I am not aware that French literature possesses any "histoire de la pensée," either of a longer or shorter period; I know of innumerable works in German which cover a similar field, but they have mostly used the word *Weltanschauung*, or expanded the meaning of Thought into the wider sense of a history of Civilisation (*Kulturgeschichte*) or narrowed it to that of Literature, proving—as it seems to me—

46. Want of precise term in German and French.

the real want of a concise term such as the English language now supplies. And yet, I think I am right in saying that the conception of Thought, in the sense in which I am using it, is truly an outcome of interna-

47. Conception of Thought nevertheless not specifically English.

tional, not of specifically English progress, and belongs mainly to the period of which I am treating,—a period characterised, as I have already remarked, by the great interchange of ideas, by the breaking down of intellectual barriers, between the principal European nationalities.

48. Carlyle the first to give a special meaning to the word Thought.

It was above all in the mind of Thomas Carlyle, who first among Englishmen made a profound study of the intellectual agencies which brought about the great change in modern Europe, that the conception formed itself of an intellectual and spiritual organism, underlying and moving external events. He first gave the peculiar sense to the word Thought, in which we here employ it, and made it an object of special study for those who came after him; an object, indeed, definable in various ways and to be contemplated from differing points of view, but yet a something, a power recognised by every one, and for which no better word could be invented. No other language has a word so comprehensive, denoting at once the process and

the result, the parts and the ideal whole, of what is felt and meant: it commits us to no preconceived theory, can be used equally by thinkers of the most opposite views, and lends itself to any specialisation which may become necessary.

II.

1. The two factors of intellectual progress.

Two processes have helped to determine the intellectual progress of mankind. These two processes have often been apparently opposed to each other in their operations; but in reality neither of them can proceed very far without calling the other into existence. They are the extension and the condensation of knowledge. Curiosity, the demands of practical life, the experiences of every day, all tend to an enlargement, to an accumulation of knowledge. Such growing knowledge is, however, of little avail if it be not readily grasped: the command of knowledge is as important as its accumulation. The more extensive the country which we wish to explore, the more we look out for elevated and commanding points of view, which permit us at a glance to overlook a wide landscape measuring the distance behind or the prospect before us. But, however enticing, these elevated views are frequently seductive and misleading. They permit us not only to look backward on the land which we have explored, giving us a clearer picture of its many features, of its winding paths, of the position of its separate objects —these elevated views present to us likewise the regions which we have not yet explored, and suggest the attempt to supersede the laborious process of further exploration

by the more delightful venture of filling up the dim outlines which we see before us, with analogies of past experience or creations of our imagination. And even if we do descend into the plains and continue the minuter and more laborious search, we cannot rid ourselves of certain preconceived but frequently misleading ideas which the superficial glance has impressed on our minds.

The condensation may become an idealisation of knowledge. History affords numerous examples of these different stages of progress; centuries of dull accumulation, of unmethodical and ill-arranged learning, have been followed by short periods of enlightenment, by the triumphant shout of sudden discovery or the confident hope of invention. Patient work and real progress have for a long time been repressed by the allurements of seductive phantoms, which have had to be abandoned after an immense waste of labour. New prospects have suddenly opened the view into vast unexplored regions, heights have been gained from which the whole of human knowledge appeared for the moment condensed into a single truth or idealised into a vision, and again these delightful achievements have for a time appeared lost in an all-pervading discouragement and dismay.

2. Object of the book.

Whether our century has been characterised by any one or by a succession of several of these varying moods, is a question which I hope to answer in the sequel. For the present it is sufficient to note that in both directions—in that of accumulating and in that of condensing and idealising knowledge—the efforts of the nineteenth century have been many and conspicuous. In the former it is altogether unparalleled, whereas in the latter it has

3. Nineteenth century unequalled in accumulation of knowledge.

probably not equalled the ideal greatness of Greece in the Periclean age, the brilliancy of the Renaissance in Italy, or the great discoveries of the sixteenth and seventeenth centuries in France and England. But what our century has done is this: it has worked out and deposited in special terms of language a clearer view of the correct methods for extending knowledge, and a peculiar conception of its possible unity. At one time—and that not very long ago—the word truth seemed to indicate to the seeker not only the right method and road for attaining knowledge, but also the end, the crown of knowledge. "Truth, and nothing but truth," seems still to the popular mind the right maxim for seeking knowledge—the whole truth stands before it as the unity of all knowledge, were it found. I think it is now sufficiently clear to the scientific inquirer, as well as to the philosopher, that love of truth, while it does indeed denote the moral attitude of the inquiring mind, is insufficient to define either the path or the end of knowledge. "What is truth?" is still the unsolved question. The criteria of truth are still unsettled. It would, indeed, be a sorrowful experience, a calamity of unparalleled magnitude, if ever the moral ideas of truth and faith should disappear out of the soul of either the active worker or the inquiring thinker; but it is with these as with other treasures of our moral nature, such as goodness and holiness, beauty and poetry—our knowledge of them does not begin, nor does it increase, by definition; and though in the unthinking years of our childhood we acquire and appropriate these moral possessions through the words of our mother-tongue, they rarely gain in depth or meaning by logical distinctions which we may learn,

4. Nineteenth century, what it has achieved: *a.* Method of knowledge; *b.* unity of knowledge.

5. Search after truth not the end of knowledge, only the attitude of the inquiring mind.

or to which we have to submit, in later life. These do not touch the essence, though very frequently they may succeed in destroying the depth, of our convictions.

In the place, then, of the high-sounding but indefinable search after truth, modern science has put an elaborate method of inquiry: this method has to be learnt by patient practice, and not by listening to a description of it. It is laid down in the works of those modern heroes of science, from Galileo and Newton onward, who have practised it successfully, and from whose writings philosophers from Bacon to Comte and Mill have—not without misunderstanding and error—tried to extract the *rationale*. These methods will take up a large portion of our attention. For the moment it is important to note that the result or aim of scientific inquiry does not dictate the methods,—the purely scientific inquirer does not know where the path will lead him: it is sufficient that it be clearly marked. Modern science defines the method, not the aim, of its work. It is based upon numbering and calculating—in short, upon mathematical processes; and the progress of science depends as much upon introducing mathematical notions into subjects which are apparently not mathematical, as upon the extension of mathematical methods and conceptions themselves. The terms "exact" and "positive" are current in the Continental and English languages to denote these methods and their application. Now to any one who does not stand in the midst of the scientific work of the age, it might appear as if by merely following a defined method which is capable of numerous modifications,—by treading a clear path which in its course leads us to endless equally defined ramifications,—the scientific

6. Method of scientific inquiry. Practised first by Galileo, Newton, &c., defined by Bacon, Comte, Mill, &c.

7. Disintegration of learning only apparent.

inquirer is losing daily more and more those elevated views, those points of condensation, those unifying and idealising aspects on which, as it seems to us, the command and grasp of knowledge depends. This is indeed almost inevitable so far as the older ideas are concerned. Unity of knowledge, order and harmony, even completeness and symmetry, truth and beauty, are indeed no longer of direct use as canons for the scientific inquirer, any more than the mysteries once supposed to be inherent in certain numbers. Though we still live under the charm of such entities, however much we may try to get rid of them, it must nevertheless be admitted that the poetical, philosophical, and religious aspects of things seem to recede into an increasing distance from the scientific; they do not guide scientific search; it does not receive from them much support. Have both sides been losers by this change? So far as science is concerned, it can claim to have attained by it not only a greater formal completeness and certainty of progress, but also another very important advantage which was unknown to ancient and mediæval research.

8. Apparent distance between science and poetry.

This advantage consists in the closer connection between science and practical life. The same mathematical spirit which governs scientific methods rules also in trade, commerce, and industry, and is gradually penetrating into the professions, such as medicine, law, and administration. For all these pursuits have either directly to do with numbers, measures, and weights, with distances of space and time, or they have found it necessary to introduce an elaborate system of statistics and averages through which the irregularity and captiousness of subjective and individual influences are practically eliminated. The

9. Closer connection between science and life.

problems of scientific research have thus enormously increased; each advance in science increases our command of certain measurable phenomena in practical life; each new development in the latter prepares a new field for scientific inquiry. The contact between science and life has become more intimate in the course of our century. This to a great extent has counterbalanced the tendency of modern scientific method, which, operating alone, would have led to endless specialisation; for it is the peculiarity

10. Solidarity of all practical problems.

of all practical problems that they cannot be isolated in the same way as scientific experiments—that they, in fact, force upon us the necessity of looking at a large number of surrounding and extraneous circumstances, at the totality of life and its interests.[1]

If our century can claim to have firmly established exact or positive methods in science and life, and to have furthered in this way the interests of both, the question

11. What has the nineteenth century done for the ideals of life?

remains, Has nothing been done to uphold those older, those time-hallowed ideals of truth, beauty, and wisdom which to former ages seemed to denote the unifying and harmonising principles of science and life? What has become of philosophy, art, and religion, which were once intrusted with the special care of those ideals, charged with preventing the falling asunder of the many branches of knowledge and practice, and expected to save us from a loss of the belief in the integrity, interdependence, and co-operation of all human interests?

[1] Science deals with things in the abstract, in their isolation, *in vacuo.* Practical life deals with the same things in their position in the real world, surrounded by other things. In this distinction lies the value of Lotze's definition of the reality of a thing as "a standing in relation," *viz.*, to other things, to all things. See 'Microcosmus,' book ix.

12. Deeper conception of the unity of human interests.

Unless I believed that our age was elaborating a deeper and more significant conception of this unity of all human interests, of the inner mental life of man and mankind, I do not think I should have deemed it worth while to write the following volumes: for it is really their main end and principal object to trace the co-operation of many agencies in the higher work of our century; the growing conviction that all mental efforts combine together to produce and uphold the ideal possessions of our race; that it is not in one special direction nor under one specific term that this treasure can be cultivated, but that individuals and peoples in their combined international life exhibit and perpetuate it.

13. Different terms for expressing this unity.

A number of words have during this century been introduced by various systems of philosophy to denote this unity of the inner life of mankind: Hegel's *Geist*, Comte's Humanity, Lotze's Microcosm, Spencer's Social Organism, all refer to special sides and aspects of the same subject. And it is interesting to note how the great schools of Idealism in Germany, of Positivism in France, of Evolution—physical and mental—in England, and—in spite of their apparently disintegrating tendencies—how the social changes of the Revolution and the specialisations of science have all combined to emphasise this unity of human life and interests. To show this in detail is the object I have in view. So far we have not committed ourselves to any

14. Definition of Thought.

of the many existing theories: the word Thought seems to me to be capable of the widest application, and to denote in the most catholic spirit whatever of truth and value may be contained in the combined aim and endeavour of

all these modern aspirations. A history of this thought will be a definition of Thought itself.

Much has been done in the course of this century to prepare for an undertaking such as the one before me. It will be well to review shortly this special side of modern literature. We have indeed passed out of what may be

15. 1750 to 1850. The age of encyclopædic treatment of learning.

called the age of encyclopædic treatment of learning—the hundred years from the middle of the last to the middle of the present century.[1] The plan of such an arrangement of knowledge belongs to an earlier period, the period immediately succeeding the birth of modern science. Lord Bacon was the father of it, but neither he nor the most encyclopædic intellect of modern times, Leibniz, did much to realise the idea, and it was reserved for the genius and the labours of Diderot and d'Alembert[2] in France, in the

[1] "Encyclopædia nomen hodie frequentius auditur quam alias."—Gessner in Göttinger Lections-Katalog for 1756.

[2] Diderot's "Prospectus" to the 'Encyclopédie' appeared 1750; the first volume appeared 1751 with the celebrated "Discours préliminaire" of d'Alembert and a reprint of the "Prospectus." The complete title was 'Encyclopédie ou dictionnaire raisonné des sciences, des arts et métiers, par une société de gens de lettres, mis en ordre et publié par Diderot et d'Alembert.' The principles which guided the editors, and the object of the work, are explained, with repeated references to Lord Bacon, in this introduction, as well as in the article "Encyclopédie," in the fifth volume (1755), which was written by Diderot, and occupied 28 pages. See also Diderot's 'Pensées sur l'interprétation de la Nature,' published anonymously in 1754.

Copious details about the history, the reception, and the influence of the 'Encyclopédie' are to be found in the correspondence and memoirs of Grimm, d'Alembert, and Voltaire, Madame d'Epinay, the Abbé Morellet, and many others. They are combined into a concise narrative, giving all the important facts, in Rosenkranz's 'Leben und Werke Diderots,' 2 vols., Leipzig, 1866, and in John Morley's 'Diderot.'

It is interesting to note how the idea of the unifying and life-giving influence of thought was as familiar to Diderot as it is to us: "Si l'on bannit l'homme ou l'être pensant et contemplateur de dessus la surface de la terre; ce spectacle pathétique et sublime de la nature n'est plus qu'une scène triste et muette. L'univers se taît; le silence et la nuit s'en emparent. Tout se change en une vaste solitude, où les phénomènes inobservés se passent d'une manière obscure et sourde. . . .

middle of the eighteenth century, to carry out the plan, foreshadowed in the 'Novum Organum,' of collecting all knowledge, which had been accumulated ever since science had been liberated from the fetters of theology, into one comprehensive whole. It must, however, be admitted that whilst the practical end of these laborious undertakings, the diffusion of knowledge, has certainly been greatly furthered, the original idea, that the sum of human knowledge is an organic whole, has in the execution been by degrees entirely lost sight of. The unity of thought and knowledge was indeed referred to in Diderot's "Prospectus" and d'Alembert's "Discours préliminaire," and in the introduction to Ersch and Gruber's great Encyclopædia,[1] as also in Coleridge's celebrated essay

16. Unity of knowledge gradually lost sight of in encyclopædic works.

Voilà ce qui nous a déterminé à chercher dans les facultés principales de l'homme la division générale à laquelle nous avons subordonné notre travail."—Article "Encyclopédie,' p. 641.

[1] Ersch und Gruber's 'Allgemeine Encyclopädie der Wissenschaften und Künste,' Leipzig, 1818 to 1875, unfinished, 151 vols. It was founded by Professor Johann Samuel Ersch, librarian at Halle in 1813, assisted by Hufeland, Gruber, Meier, and Brockhaus, and contained contributions by the most learned and eminent Germans of the century. It is interesting to compare the plan and principles which guided the editors, as expounded in the introductions to the first and second volumes, with the corresponding dissertations prefixed to the 'Encyclopédie' in France and the 'Encyclopædia Metropolitana' in England. The unity aimed at by Bacon was either purely formal, securing only uniformity and completeness of treatment, or it was that of practical usefulness—the philosophy of fruit and progress. The plan adopted by Diderot and d'Alembert could hardly attain anything more than this. Coleridge, nursed in German philosophy, and deeply impressed with the fact that there is a higher view than that of Lord Bacon, and that such is to be found rather in writers like Plato and Shakespeare, uses the word method in a much wider sense. He was deeply affected by the spirit of the idealistic philosophy, which was foreign to Bacon and unduly despised by him.

In the idealistic systems of the Continent, beginning with Kant, the opinion was current that the methods and treatment of science alone were insufficient to close the circle of knowledge. The truly encyclopædic view was only possible in a scientific investigation specially carried on for that purpose, and this was considered to be one of the main objects of philosophy. Thus Kant in many passages of his works, notably vol. ii. pp. 377, 378,

on the science of method prefixed to the 'Encyclopædia Metropolitana'; but the result has shown, what was not evident to Lord Bacon, that neither a systematic division of learning according to some logical principle, nor the historical identity of the beginnings of all branches of knowledge, can in the end preserve the real unity and integrity of thought. The work of the advancement of learning, if it be once handed over to different sciences and intrusted to separate labourers, does not proceed in a cycle which runs back into itself, but rather in the rings of an ever-increasing spiral, receding more and more from the common origin. Such is the impression we get if we contemplate the unfinished[1] rows of Ersch and Gruber's

613; vol. iii. pp. 188, 212; vol. v. p. 312 (Rosenkranz's edition), especially the two following: "Philosophy is the only science which can procure for us inner satisfaction, for she closes the scientific cycle, and through her only do the sciences receive order and connection." And: "Mere 'πολυιστορία' is a cyclopean learning which wants one eye—the eye of Philosophy—and a cyclops among mathematicians, historians, naturalists, philologists, and linguists, is a scholar who is great in all these lines, but having these considers all philosophy as superfluous." Still, with Kant Philosophy is not an "instrument for the extension," but merely a study of "the limits of knowledge"; she does not "discover truth," but only "prevents error." This modest definition was given up in the systems of Fichte, Schelling, and Hegel, who maintained that a certain kind of—and this the highest—knowledge could be attained by starting from one highest principle deductively: the all-embracing, encyclopædic character of philosophical, speculative knowledge was increasingly emphasised, and this not only in special lectures on the subject, as in Fichte's lectures on "The Nature of the Scholar," in Schelling's on "The Method of Academic Study," in Hegel's 'Encyclopædia of Philosophy,' but also in the regeneration and reform of many older and in the foundation of new universities and academies throughout Germany. The great 'Encyclopædia' of Ersch and Gruber was planned in a similar spirit, as the reform of university teaching and of academic learning. This reform has been of the greatest importance to the German nation and to the interests of science and knowledge. The Encyclopædia, on the other hand, has remained incomplete, a huge but abortive attempt to combine not only the principles of knowledge, but also the colossal and growing volume of it, into a systematic whole.

[1] The promoters of it were evidently not sufficiently impressed with the two very essential conditions which make a work of this

volumes, or if we recognise the fact that the more useful and popular publications of our day have abandoned the philosophical introductions and preliminary discourses[1] by which the earlier works preserved a semblance of unity and method, and are contented to be merely useful dictionaries of reference. The encyclopædic treatment of knowledge, the execution of Lord Bacon's scheme, has shown that the extension and application of learning leads to the disintegration, not to the unification, of knowledge and thought. A conviction of this sort is no doubt the reason why in German universities lectures on "Encyclopädie" have been abandoned.[2] They were very general and popular in the earlier years of the century, when, under the influence of Kant, Fichte, and

17. Lectures on "Encyclopädie" abandoned in German universities.

kind useful—*viz.*, that it must be finished, however imperfect it may be, and that it must be completed within a limited time, on account of the revolutions and smaller changes in thought and knowledge. These essential conditions were always before the mind of Diderot. See his article "Encyclopédie," pp. 636-644.

[1] The object of the philosophical introductions has in course of this century been much more completely attained by such works as Mill's 'Logic' and Jevons's 'Principles of Science'; whilst the "preliminary dissertations," such as were contained in the older editions of the 'Encyclopædia Britannica,' have been partially superseded by works like Whewell's 'History' and his 'Philosophy of the Inductive Sciences,' in which the common origin, the genesis, the continuous development and interdependence of the different sciences, are traced. The value in this respect of an undertaking like that of the Royal Bavarian Academy ('Geschichte der Wissenschaften in Deutschland,' vol. i., 1864: it has now reached 22 vols., the science of War significantly filling three large volumes, that of Mathematics one small one) is much diminished by the title suggesting that science is a national, not a cosmopolitan or international concern. Fortunately many of the contributors to this important and highly useful publication have not limited their narratives to purely German science, but have largely taken notice of non-German research. Special reports on the state of any science or branch of science in a nation have, of course, quite a different meaning and value.

[2] The term is still in use for courses of lectures giving a general and comprehensive view of special sciences: thus, "Encyclopädie des Rechts, der Medicin, der Philologie, der Philosophie, der Theologie."

Schleiermacher, university teaching and learning entered on a new era, in which the idea prevailed that completeness, universality, and unity of knowledge could be secured by one and the same arrangement of study.[1] It was the age when philosophy for the last time had got a firm hold of all departments of knowledge, and permeated all scientific pursuits;[2] when, favoured by political events,

[1] On this subject the literature connected with the foundation of the University of Berlin in the year 1809 is of special interest. It was essentially the creation of Wilhelm von Humboldt, though prepared by Wolf and Beyme in 1807. See Seeley, 'Life of Stein,' vol. ii. p. 430 *sqq.*; Haym, 'Leben W. v. Humboldts,' p. 270 *sqq.* The foundation of this university in the year of Prussia's greatest misery, when the first gleams of liberty in the rising of Spain and the success of Aspern had been extinguished by the defeat of Wagram, the voting of £22,500 per annum for the purposes of the new University and the Academy of Science and Arts, when a crushing war-tax hung over the country, when land was depreciated, the necessaries of life at famine prices, the currency of the country at a large discount, when every one, from the king to the lowest subject, was forced into sacrifices and economies of every kind, was an act as heroic as the great deeds on the battle-field, and as far-seeing as the measures of Stein and Scharnhorst. Interesting from our point of view are the ideas of Fichte on university teaching and academic learning, laid down in his 'Deducirter Plan einer zu Berlin zu errichtenden höheren Lehranstalt,' written at the request of the minister Beyme in 1807. In it a great deal is said about encyclopædic treatment. The question of the position of philosophy in the encyclopædic or academic treatment of knowledge was easily solved in the Kantian school, to which most of the above-mentioned writers belonged. Later on in the school of Schelling it became more difficult. It was frequently discussed by Schelling himself, who was one of those that initiated the new era in the Academy of Munich, which was remodelled in the year 1807. See, *inter alia*, Schelling's essay, "Suggestions concerning the Occupation of the Philologico-Philosophical Class" of the Academy, and especially the following remarkable passage ('Werke,' vol. viii. p. 464): "If, indeed, Philosophy were denied living contact with real things, if she were obliged to soar in transcendent regions without end and measure, and to rise a hungry guest from the well-appointed table of Nature and Art, of History and Life; then it would be incomprehensible how she could still find so much support as to be received in an academy, and it would be much better if we also followed the path of other nations, who have lately said good-bye to all philosophy, and have thrown themselves, with the most glowing ardour, upon the exploration of Nature and Reality in every direction."

[2] The principal representatives of the encyclopædic teaching at the German universities were Eschenburg, Krug, and Gruber. The latter, in his introduction to the second volume of Ersch and Gruber's 'Encyclopädie,' gives a definition and history of encyclopædic study, which, according to him, was introduced into the modern (German) universities together with the philosophical faculty. In the beginning this was subservient to the three higher faculties (theology, law, and medicine), but gradually took the lead. He argues that only since university studies have become encyclopædic can they be considered as furthering true humanity. He refers to the great crisis through which in the beginning of the century literature, science, and arts were passing (p. li), and mentions the conflicting principles in the treatment of mathematics, physics, history, philosophy, and philology. See also the 'Vorbericht,' vol. i. p. vii.

ideal aims, a generous spirit of self-sacrifice, and a feeling of one common duty pervaded the German nation, and foremost in it the teachers and students of the German universities.[1] This spirit, as it produced co-operation and unity of action, also favoured unity of thought, and contributed much to the popularity of several philosophical systems which promised more than they could give. Encyclopædic surveys were then supposed to be more than the empty shell, the mere skeleton of learning which they have since proved to be; they were looked upon as being able to grasp and convey the living spirit of knowledge. This phase of thought, which in the sequel will largely command our attention, has dis-

18. Encyclopædias did not fulfil what they appeared to promise.

[1] Among the mass of literature dealing with this subject, the 'Memoirs of Frederick Perthes,' by his son (English translation, vol. i. chap. xi. *sqq.*), and Steffens's 'Autobiography' ('Was ich erlebte,' Breslau, 1840-44, 10 vols.), give the most vivid and exhaustive accounts. Neither Stein, the great statesman, nor Goethe, the great poet and thinker of the age, took part in this alliance of the patriotic and intellectual interests of the German nation. Stein's attitude to the idealism of the age is defined by Seeley, 'Life of Stein' (vol. i. p. 30, "It is desirable to mark that between him and the literature and philosophy of his time and country there was no connection at all"), and is expressed in a remarkable conversation which he had with Steffens, March 1813, at Breslau (quoted by Seeley, vol. iii. p. 119; Steffens, vol. vii. p. 120 *sqq.*) Goethe's position is defined by his reply to the invitation to contribute to the 'Deutsches Museum,' a periodical planned by the bookseller Perthes. It was to be a scientific alliance of all the intellect of Germany, and was in time "to be transformed into a political one possessing the strength and union necessary for vigorous action" (Perthes' Memoirs, vol. i. p. 167).

appeared; the second half of our century does not expect to find the essence of knowledge condensed in any philosophical formula, any more than it expects to find the real unity and integrity of thought preserved in the fragmentary articles of an alphabetical dictionary. The purpose of the latter is purely practical; it is a popular and handy instrument for the diffusion of knowledge, whilst philosophical divisions are merely formal, and at best are applicable only to a narrow and limited sphere of research.[1]

The age of encyclopædic representation of learning and the short period of philosophical formalism seem both to belong to the past; but the desire of bringing together what is scattered, of focussing knowledge and learning, and of realising the organic continuity and unity of thought and progress, is as great as, perhaps greater than ever. Neither the shapelessness of a huge dictionary nor the barrenness of a concise formula will satisfy the

[1] It is interesting to observe the development and spread of encyclopædic learning in the three countries. Encyclopædias in the modern sense have their origin, like so many other modern institutions and ideas, in England. They were there compiled mainly for practical purposes. France took up the scheme in a philosophical spirit, and carried it as far as it is capable of being carried under this aspect. Attempts to improve and amplify the plan proved impracticable; and when subjected to the vast erudition of Germany, it became evident that unity, depth, and breadth of view could not be maintained. In course of this century the country which produced the classical era of encyclopædism has done least for encyclopædic learning. This has now its home in Germany, where encyclopædic labours have been specialised, and where every science is represented by some compilation or annual register aiming at collecting and systematically arranging the scattered contributions of the whole world. But it would be ungrateful not to mention the Royal Society's catalogue of scientific papers, and the services which America has rendered in summarising the literary productions of the English-speaking nations in such works as Poole's 'Index to Periodical Literature.' Without the aid of such laborious compilations the present work could not have been undertaken.

deeper conviction that all mental work is living, individual, and of endless variety. To stimulate individual thought, to bring about life and change, is nowadays felt to be quite as necessary as to insist on method, system, and order. Prompted by this conviction, the last fifty years have done much to facilitate intellectual interchange, and to record the historical development of all branches of science.

19. French were the masters in science at the beginning of the century.

This object has been promoted in three different ways. The French, who in the beginning of the period were the masters in science, led the way by founding a series of periodicals devoted to the development of separate sciences. Germany followed, and still later England.[1] A living

[1] The oldest scientific periodical is the 'Journal des Savants,' which was started in 1665 in Paris; next to it comes probably Rozier's 'Observations sur la Physique' (1771), continued under the title 'Journal de Physique' (1778, continued with interruptions from 1794 - 95 till 1823). In opposition to this journal, which defended the older phlogistic theories in chemistry, the 'Annales de Chimie' were started in 1789 by Berthollet, Guyton de Morveau, and Fourcroy, as an organ of Lavoisier's ideas. In 1788 the Société Philomatique started its 'Bulletin,' and in 1795 the 'Journal de l'Ecole Polytechnique' started its influential career. No such periodicals existed for special sciences at that time in any other country, if we perhaps except the 'Transactions of the Royal Linnæan Society,' which started in 1791. 'Nicholson's Journal' started in 1797; the 'London, Edinburgh, and Dublin Philosophical Magazine and Journal of Sciences' had its origin in Tilloch's 'Philosophical Magazine'; but the first journal devoted specially to mathematical sciences in England was probably the 'Cambridge Mathematical Journal,' started in 1839. In the meantime the number of scientific journals in France had grown enormously. In Germany we have Crell's 'Chemische Annalen' (1778), Gehlen's 'Allgemeines Journal für Chemie' (1803), Gren's 'Journal der Physik' (1790), Gilbert's 'Annalen der Physik' (1799), Zach's 'Monatliche Correspondenz' (1800), Crelle's 'Journal für die reine und angewandte Mathematik' (1826), and many others, all periodicals of the first importance. The 'Transactions of the Royal Society,' which of course contain many of the valuable scientific contributions of this country, can nevertheless hardly be looked upon as a repository of the work of English mathematicians and physicists of the period in question,—not even as much as the Memoirs of the Paris Academy in France. In Great Britain a new centre of scientific and literary work existed during the latter part of the last century

intercourse between men of science was greatly promoted by the British Association for the Advancement of Science, which held its first meeting at York in 1831. Associations and meetings of this kind had their origin ten years earlier in Germany through Oken;[1] but the line in which Germany has done most is the establishing of and continuing annual Reports[2] of the progress of the different

in Edinburgh ('Transactions of the Royal Society of Edinburgh,' started in 1788), and somewhat later likewise in Dublin ('Transactions of the Royal Society of Dublin,' started 1799), and Manchester ('Memoirs of the Manchester Philosophical Society,' started in 1789). Many of the first scientific writers of the age published in these provincial papers or in separate pamphlets—the want of a common collecting centre being very obvious.

[1] Alexander v. Humboldt supported them, and was instrumental in giving to the Assembly at Berlin in 1828—which he called "The invasion of philosophers"—a special importance. It was, as he says, "a noble manifestation of scientific union in Germany; it presents the spectacle of a nation divided in politics and religion, revealing its nationality in the realm of intellectual progress."—Bruhns, 'Life of A. v. Humboldt,' vol. ii. p. 130. The British Association for the Advancement of Science was (as Prof. Owen informs us) at the outset avowedly organised after the Okenian model.—'Encyclopædia Britannica,' art. "Oken."

[2] The first reports aiming at giving a statement of the position of Science were those drawn up by Delambre and Cuvier at the request of the Emperor Napoleon I., and presented in the year 1808 under the title 'Discours sur les Progrès des Sciences, Lettres, et Arts depuis 1789 jusqu'à ce jour' (1808). They were imitated on a larger scale by the Emperor Napoleon III., on the occasion of the great Paris Exhibition 1867, and have been continued under the Republic. Of the report of 1808 Cuvier says, "Ce tableau historique nous servira désormais de point de départ et nos rapports annuels en seront autant de continuations." He also adds significantly, "Dans les relations actives où nous nous trouvons avec la plupart de ceux qui cultivent les sciences, il est bien difficile qu'ils se fassent en Europe quelques découvertes importantes sans que le bruit en retentisse promptement dans cette enceinte, et nous excite à des travaux qui s'y rapportent plus ou moins directement."

By far the most important work of reporting and summarising the results of scientific labour has been done by Germany. The first publication of this kind, however, originated with Berzelius, who from the year 1821 reported regularly to the Academy of Stockholm on the progress of the physical sciences. Of Berzelius's periodical Kopp says ('Geschichte der Chemie,' vol. i. p. 403), that it "summarises with the greatest completeness all that had been done in chemistry since 1820." This work, which regularly appeared in German translation, was continued in Liebig's 'Jahresbericht der Chemie' (1847). In Berlin the 'Physikalische Gesellschaft' has

sciences, in which all scientific researches are—without regard to nationality—reviewed, classified, and arranged in the most complete manner, according to the place which they occupy in the general development. Invaluable service has also been done in England by special Reports or Addresses, prepared by men of the greatest eminence—frequently at the request of the British Association—in which the position of special branches of science is explained, the work of the past summed up, the leading principles clearly brought out, and the unsolved problems placed prominently before the minds of young and aspiring workers.

20. Reaction in Germany against metaphysical treatment of scientific subjects.

In Germany during the first half of the century a reaction set in against the metaphysical treatment of scientific subjects, which had been exaggerated in the schools of Schelling and Hegel. Experimental research, following mainly the great French and English models, was next favoured, and through the establishment of laboratories and observatories, through voyages of discovery and the application of science to the industries, an enormous amount of detailed and minute knowledge was accumulated.[1] For a time—even within the limits

continued to issue regularly since 1845 annual Reports under the title 'Fortschritte der Physik.' But it was only in 1868 that a similar annual was started in Berlin having reference to mathematics, under the title 'Fortschritte der Mathematik.' A 'Jahresbericht' on Zoology has appeared ever since 1879, and one on Botany since 1873.

[1] It was the age which compiled the great repositories of chemical knowledge. Such were Gmelin's 'Handbuch der Chemie' (1st ed., 1817. Translated into English by the Cavendish Society, 1848), and the 'Handwörterbuch der reinen und angewandten Chemie' (edited by Liebig jointly with Poggendorf and Wöhler, 1837). The same age also set going and filled the volumes of Liebig's 'Annalen' (started by Hänle in 1823 under the title 'Magazin der Pharmacie,' it finally assumed the title of 'Annalen der Chemie und Pharmacie' under Liebig's editorship), of Poggendorf's 'Annalen der Physik und Chemie' (1824), and the 'Annales de Chimie et de Physique.'

of exact reasoning—attempts to condense and unify knowledge were discredited. The result—especially in Germany—was that in many sciences information became buried in periodicals and in the memoirs of learned societies: text-books were chiefly written by men of secondary importance, translated from the French and English, and frequently on somewhat antiquated lines.[1] The new spirit which began to leaven scientific research in the middle of the century was confined to a few master minds, who—frequently almost unknown—marched in advance of their age. In the course of the last thirty years this has been entirely changed. The means of intercourse and communication, referred to above, make scientific isolation almost impossible; the necessity has

21. Reform in school literature.

been felt of remodelling the whole of the popular school literature on more modern lines: some of the first intellects in science have condescended to write text-books of their subjects, by which a great reform has been brought about in the higher scientific literature.[1] At the same time—after fifty years of experimental research and accumulation of material—it has become necessary to review the fundamental principles on which scientific reasoning rests: a more philosophical, not to say metaphysical, spirit is manifesting itself within the limits of science.[2] In the abstract, and especially the mathematical, sciences, real progress depends now mainly upon the discovery of methods of simplification, on conciseness and elegance of treatment, and on the discovery of unifying principles and generalising aspects.[3]

22. Scientific reasoning more philosophical.

[1] The greater part of the higher German school literature in mathematics and physics was supplied by the French or modelled on French ideas—Legendre and Monge in elementary and descriptive geometry, Lacroix in the higher branches. Francœur's course of mathematics was introduced in England as well as Germany; Poisson, and later Lagrange and Duhamel, became the models in mechanics, Biot and Pouillet in experimental physics, Regnault in chemistry. The only great popular authorities which did not belong to France were Berzelius and Graham in chemistry, and Euler in mathematics. As late as 1860 hardly any text-book existed in Germany on the theoretical and mathematical portions of physics. The second volume of 'Baumgartner' was a miserable compilation. Beer's 'Höhere Optik' was the first important work of this kind. Germany had indeed not been wanting in original research, but the new ideas of Möbius, Steiner, Staudt, Plücker, and Grassmann in geometry found no adherents till, mainly through the translation of Salmon's text-books by Fiedler, a new spirit came over geometrical teaching. In the meantime Lejeune Dirichlet, and Neumann the elder, cultivated in their academical lectures the higher branches of mathematical physics, and educated a whole generation of mathematicians and physicists. Through them the original researches of Gauss and Jacobi became better known, and an independent school of German mathematical thought was established. In England the influence of French science was much more limited, and to the present day Euclid is preferred to Legendre's more elegant methods.

[1] This remark refers mainly to England and Germany. In France, as a result of giving lectures at the École Polytechnique, the Bureau des Longitudes, the Faculté des Sciences, &c., the great mathematicians and physicists of the century have frequently worked up their researches in connected treatises. For such we are indebted to Lamé, Cauchy, Poncelet, and many others. But the two works which in England and Germany created probably the greatest reform in the teaching of the principles of natural philosophy were Thomson and Tait's 'Natural Philosophy' (first sketch, 1863, 1st ed., 1867) and Kirchhoff's 'Vorlesungen über Mechanik' (Leipzig, 1877).

[2] I refer principally to the various writings of Helmholtz, following those of Riemann, and the many hints thrown out in Gauss's published papers, and in his correspondence with Schumacher. Helmholtz has—of all purely scientific writers—paid most attention to the metaphysical foundations of geometry and dynamics, and has critically examined the earlier theories of Kant, published a century ago. It is interesting in this respect to note what Kant is reported to have said to Stägemann in 1797: "I have come with my writings a century too soon; after a hundred years people will begin to understand me rightly, and will then study my books anew and appreciate them." (See 'Tagebücher,' von Varnhagen von Ense, Leipzig, 1861, vol. i. p. 46.) Next to Helmholtz we are most indebted to Emil du Bois-Reymond and his brother Paul. See Emil's 'Reden' (Leipzig, 1886-87, 2 vols.), and the posthumous work of his brother: 'Ueber die Grundlagen der Erkenntniss in den exacten Wissenschaften' (Tübingen, 1890).

[3] An authority on this subject says: "Generality of aspects and methods, precision and elegance of exposition, have, since the time of Lagrange, become the common property of those who claim to be scientific mathematicians. This

All these are merely external signs of the new life, indications of progress and change: the inner reason and result, the altered ways of thinking which underlie or are produced by these external changes, will be the object of closer study hereafter; they constitute the real substance of this work. What I draw attention to here, by way of introduction, are merely fingers on the dial-plate of a complicated clock-work: their motion and position are patent to every one. Later on I shall invite the reader to remove the outer case, and try with me to understand the delicate working parts and the principle of the mechanism, the prime mover and the mode of transmission of motion within. The general curiosity that exists to follow the internal and hidden workings of thought is manifested especially in that country which in modern history has frequently taken the lead in philosophical reasoning.

23. Germany has taken the lead in studying the life of thought.

It is manifested by the huge and increasing historical literature of Germany, which is devoted to tracing out the growth and development of modern science and thought. In that country history seems for the moment to have taken the place of metaphysical speculation. A similar transition from the logical to the historical view can be traced in English literature in the last century, the

generality is sometimes exaggerated at the expense of simplicity and usefulness, and then leads to abstruseness and to the enunciation of theorems which have no special application; precision may degenerate into an affected brevity which renders a dissertation more difficult to read than to write; elegance of form has in our days almost become the test of the value of a theorem. Yet in spite of all drawbacks these conditions of efficient progress are of the greatest importance, inasmuch as they keep the scientific matter within those limits which are intrinsically necessary if mathematical research is not to lose itself in minutiæ or be drowned in over-abundance." — Hankel, 'Die Entwickelung der Mathematik in den letzten Jahrhunderten' (Tübingen, 1869).

typical representative of that change being David Hume, who, starting with the metaphysical problems involved in Locke's and Berkeley's writings, was from them led on to the study of moral, political, and economic questions, and ended by devoting himself to the study of history.[1] At the end of his career political and historical writings were as frequent in English literature as metaphysical and theological writings had been at the beginning.

24. Causes of transition from metaphysical to historical method.

The causes which have effected the same transition from the metaphysical to the historical mode of treatment in Germany during the present century are similar to those existing in England in the last century; but the whole movement has taken place on a larger scale, penetrates deeper into the mental life and work of the nation, and cannot be so easily studied in the writings of any great representative.

Whilst in Germany historical studies are now foremost,

[1] I am quite aware that generalisations of this kind must be made and used with great caution. I therefore refer my readers to Leslie Stephen's 'History of English Thought in the Eighteenth Century,' especially to the Introduction, where the typical position of Hume is fully discussed, and also to the last chapter of the second volume, where he says of Hume (vol. ii. p. 381, 1st ed.): "Hume was, in one sense, far in advance of his time, and indeed of the average opinion of the present time. But the change may in many respects be described as a revolt from Hume's opinions, much more than a development of them. . . . The history of philosophical and of theological opinion in England is a history of gradual decay down to the revolutionary era." And p. 444: "The last half of the century was pre-eminently historical. As civilisation progresses, as records are better preserved, and a greater permanence in social organisation makes men more disposed to look beyond their immediate surroundings, a tendency to historical inquiry is naturally awakened. This cause alone, without the more philosophical considerations which might lead a Hume or a Gibbon to turn from abstract investigations to historical inquiries, may account for the growth of antiquarianism in the latter years." But the mere statistics of English literature in the eighteenth century suffice to prove the decline of argumentative and the growth of realistic literature.

and have almost dislodged systematic philosophy, England has for the first time in her history produced a system of philosophy—that of Mr Herbert Spencer; and this with the distinct understanding that the object of philosophy is the unification of knowledge.[1] It is a remarkable fact, which will occupy our close attention hereafter, that the unifying principle in this system is historical,—a process of development now specially known under the term Evolution. This system forms in a certain way a contrast to the last great system in German philosophy, that of Hermann Lotze. Whereas in all systems of evolution the unity of things is historical, and has to be sought in their common origin, Lotze emphasised the truth that unity must be a living presence, a principle which exists in individual things, not merely a link which connects them by proximity in time or space. His object is to answer the question, How can the human mind represent to itself such a living unity, in what ideas

25. Herbert Spencer the first Englishman who has produced a system of philosophy.

26. Definition of Lotze's system.

[1] See G. H. Lewes ('Problems of Life and Mind,' 1st ed., vol. i. p. 84), who says: "The absence of a philosophy in England during the last two hundred years has been a serious defect in her culture. Science she has had, and poetry and literature, rivalling when not surpassing those of other nations. But a philosophy she has not had, in spite of philosophic thinkers of epoch-making power. Hobbes, Locke, Berkeley, Hume, have produced essays, not systems. There has been no noteworthy attempt to give a conception of the world, of man, and of society, wrought out with systematic harmonising of principles. There has not been an effort to systematise the scattered labours of isolated thinkers. Mr Herbert Spencer is now for the first time deliberately making the attempt to found a philosophy." And in his 'History of Philosophy' (3rd ed., vol. ii. p. 653) the same author says: "Mr Spencer alone of British thinkers has organised a system of philosophy." Croom Robertson would take exception to this in favour of Hobbes, "who attempted a task which no other adherent of the 'mechanical philosophy' conceived—nothing less than such a universal construction of human knowledge as would bring Society and Man within the same principles of scientific explanation as were found applicable to the world of Nature" (Ency. Brit., 9th ed., vol. xii. p. 39).

belonging to human thought can this unity be grasped, by what words of human speech can it be expressed?

Both Mr Herbert Spencer's 'System' and Lotze's 'Microcosmus' are written with the object of establishing the unity of thought, of preserving the conviction that things exist and that events happen in some intelligible connection, and especially that the religious and the scientific views of the world and life are reconcilable. But whereas Mr Spencer is content to point to the underlying unity as the Unknowable, and then betakes himself to the study and exposition of the manner in which events follow and things develop, Lotze considers the whole of this part of philosophy as merely an introduction to the solution of the real problem. To him a process of development is merely the outer form in which some real substance presents itself, a mechanical method by which something of higher value is accomplished. He admits the all-pervading rule of such a mechanism, but he urges the necessity of finding the substance itself, and of gaining a view of the end and aim which is to be attained by this array of processes, by this parade of mechanical means, of the interest that attaches to them, and the result which is to be secured.[1] Knowing the mechanism by which a certain object is accomplished, we may be able to calculate phenomena and events, but to understand[2] them requires a

[1] The earliest passage in which Lotze gives us a pretty complete idea of his philosophical methods and aims is to be found in his polemical pamphlet against Fichte the younger ('Streitschriften,' Leipzig, 1857, p. 52 *sqq.*) He there also reviews his attitude to the idealistic school of German Philosophy and to Herbart, whose follower he refuses to be called (*ibid.*, p. 5 *sq.*) It is evident that at that time his system was not yet definitely settled in his mind (p. 58).

[2] The difference between calculating and understanding phenomena is probably to be traced to Leibniz. Lotze emphasises this difference.

further knowledge of the worth of the object which is accomplished, of the result which is gained by the calculation. It is one thing to be able to trace the mechanical conditions upon which the accuracy of a clock depends; it is another to mark the hour which the clock strikes, and to note the time which it measures out to us for our work. Curiosity will lead a child to pry into the former; but the latter depends on our appreciation of the objects of life and the seriousness of our duties.

27. Lotze's relation to Herder's 'Ideen.'

When Lotze undertook to write the 'Microcosmus,' he referred to two great works of a kindred tendency. Both attempted, yet in very different ways, to give a comprehensive view of a large field of scattered phenomena, to take in at a glance the entire scheme of a great world of facts. The earlier of the two belonged to the last century and was concerned with history, with the uniting bond of all human development. For this Herder, in his 'Ideen zur Philosophie der Geschichte der Menschheit,' had, if not invented, yet endowed the term Humanity with a specific pregnancy, meaning by it the unity of all human interests in their social and historical development—an idea which since Leibniz has governed German literature.[1] The other

See, *inter alia*, the closing paragraph of the first volume of the 'System der Philosophie' (1st ed., Leipzig, 1874). I cannot omit to notice here the extraordinary and misleading misprint in Erdmann's quotation of this passage: see his valuable 'Geschichte der Philosophie' (3rd ed., Berlin, 1878, vol. ii. p. 861), where instead of *berechnen*, to calculate, we read *bezeichnen*, to designate!

[1] The history of this idea has been written by Hettner in the last two volumes of his 'Literaturgeschichte des 18ten Jahrhunderts.' I quote from the 2nd edition, Braunschweig, 1872. Herder had inherited the spirit of Leibniz (see, *inter alia*, the concluding chapter of my essay on Leibniz, in Blackwood's Philosophical Classics, Edinburgh, 1884). Herder formed a kind of centre of thought, inasmuch as he gathered up in his own mind and writings the influences of Leibniz, Rousseau, and the English writers of the eighteenth century, together with classical influences and new inspirations drawn from the popular song-literature of all nations. Hettner says (see last volume but one, p. 7): "Herder applied Rousseau's gospel of Nature to the demands of poetical sense and creation. Thus he has become essentially the forerunner of the new school of poets: the last fetters of the moralising style by which even Lessing was still hampered fell, and through the scientific study of the beginnings and development of human culture he became the founder of a new science of Language, Religion, and History, in the lines of which we are still advancing." And p. 101: "Herder does not belong to the classics of the style of Winckelmann, Lessing, Kant, Goethe, and Schiller; he is everywhere only suggestive, hardly anywhere conclusive and final. For this reason his writings are to some extent antiquated. Nevertheless Herder is one of our most important and influential spiritual heroes. Herder made so deep an impression on his age that the great poetry of Goethe and Schiller, the so-called Romantic School, the philosophies of Schelling and Hegel, cannot be imagined without Herder as the precursor." The fourth volume of Gervinus, 'Geschichte der deutschen Dichtung,' contains likewise a very important chapter on Herder. But the great authority on Herder is R. Haym, 'Herder nach seinem Leben und seinen Werken' (Berlin, 2 vols., 1880 and 1885). From the unpublished literary notes, correspondences, and diaries of Herder, which Haym inspected, it is evident that the great idea of writing a History of Humanity originated in Herder's mind as far back as the year 1769, on a voyage from Riga to Nantes (on the way to Paris). His diary closes thus: "History of the progress and of the powers of the human mind in the concurrence of whole ages and nations—a spirit, a good demon, has exhorted me to do this. Be that my life's work, History, work!"

The first attempt to carry out his great idea was published by Herder in the year 1774, with the title: 'Auch eine Philosophie der Geschichte zur Bildung der Menschheit.' Herder was then in his thirtieth year. His chief work appeared ten years later (1784), with the title 'Ideen zur Geschichte der Menschheit.' Herder died in 1803. Goethe's 'Faust,' which is an attempt to deal with the highest problems of human interest, the problems of knowledge, evil, sin, and redemption, as they appear in the history of a great individual, not of the race, had its first beginnings about the same time as Herder's 'History of Mankind.' But the work was not finished till a year before Goethe's death in 1831.

great work was that of A. v. Humboldt, who in the course of a long career, peculiarly favoured by opportunities for studying Nature on an extensive scale, and for appreciating the detail of modern research, of which he was an illustrious representative, had never lost sight of the all-pervading unity.[1] In an elevated style, in which poetry and science

28. Lotze's relation to A. v. Humboldt's 'Kosmos.'

[1] Alex. v. Humboldt, 'Kosmos. Entwurf einer physischen Weltbeschreibung,' 1845. Like Herder's great work on the 'History of Humanity' and Goethe's 'Faust,' Humboldt's 'Kosmos' occupied a

are happily blended, he essayed in the evening of life to unroll before the gaze of his readers a picture of the grand features of nature as his mind had viewed them from the elevated regions of scientific study, and his eyes from the heights of Chimborazo.

In the great picture of the world, in the vast changes of the universe, where is man with his life and his interests? In the huge Kosmos where is the Microcosmus? This question naturally presented itself to the mind of

29. Lotze's 'Microcosmus.' Lotze. "It is not," he tells us, "the all-embracing 'kosmos' of the universe which we wish to describe again on the model which has been given to our nation. As the features of that great world-portrait sink deeper into general consciousness, so much more vividly will they lead us back to our own selves, suggesting anew the question, What significance belongs to man and human life with its lasting characteristics and the changing

long period in the life of its author. Goethe's 'Faust' deals with the individual problem, Herder's 'Ideen' with the problem of the race or mankind, Humboldt's 'Kosmos' with the same problem as referring to the world, the universe. In the preface Humboldt confesses "that the image of his work had stood before his mind's eye in undefined outlines for nearly half a century": *cf.* what Goethe says in the dedication to 'Faust' (written probably after 1797):—

> "Again ye come, ye hovering forms; I find ye
> As early to my clouded sight ye shone," &c.
>
> —Transl. B. Taylor.

The view of the universe which was given in Humboldt's 'Kosmos' was prepared by his own publication, 'Die Ansichten der Natur' (1808); also by Georg Forster (1754-1794), who wrote an account of the second voyage of Captain Cook round the world, whom he accompanied with his father. "He conceived of nature as a living whole; his account is almost the first example of the glowing yet faithful description of natural phenomena, which has since made the knowledge of them the common property of the educated world" (R. Garnett in 'Ency. Brit.,' art. "Forster"). Humboldt confesses to have received from him "die lebhafteste Anregung zu weiten Unternehmungen" ('Kosmos,' vol. i. p. 345, also vol. ii. p. 65, and especially vol. ii. p. 72, where incidentally also Darwin's narrative of the "Adventure" and "Beagle" is mentioned).

course of its history in the great totality of nature?"[1] And in collecting the answers to this question which suggest themselves both in and outside of the study, Lotze professes only to renew the enterprise brilliantly begun by Herder in his 'Ideen zur Geschichte der Menschheit.' Both Herder's 'Ideen' and Humboldt's 'Kosmos' belong to the age in which philosophy and poetry largely influenced science and history. Many may now think it premature or altogether impossible to try to combine the detailed studies of modern science and modern history with the comprehensive view demanded by philosophers and poets, or to grope through the labyrinth of external phenomena and events to their underlying significance and unity. They may, whilst fully maintaining the existence of an all-pervading power, nevertheless relegate it with Mr Spencer to the region of the Unknowable.[2] Without desiring at present to

[1] Microcosmus, 1st ed., Leipzig, 1856, Preface. Hermann Lotze was born in 1817, and died in 1881. His first philosophical essay of importance was the 'Metaphysik' (Leipzig, 1841).

[2] Herbert Spencer's Philosophy of the "Unknowable" is laid down in his Introduction to 'First Principles.' I believe the first appearance of the first part of this book was in 1860, and the first collected publication in the year 1867. In defining the region of the Knowable an opposite course has been adopted by Emil du Bois-Reymond, who in a series of addresses and articles, now collected in two volumes with the title 'Reden' (Berlin, 1886 and 1887), tried to lead up to the limits which are fixed around scientific knowledge. The purport of his teaching on the highest "World-problem" is contained in the four words, *ignoramus, ignorabimus, dubitemus, laboremus.* The first of these addresses, which are full of brilliant suggestions and vivid illustrations, furnishing in the notes especially an invaluable store of historical references on the subject of the philosophy of the sciences, was delivered at the forty-fifth meeting of the German "Naturforscher und Aertze," and published at Leipzig, August 1872, with the title 'Die Grenzen des Naturerkennens.' It made a great sensation, and was translated into several languages. It was followed some years later by an address delivered in the Berlin Academy, 1880, and published with the title 'Die sieben Welträthsel.' If H. Spencer's philosophy is termed the philosophy of the Unknowable, Du Bois-Rey

criticise the weighty considerations which have led them to a view so modest and resigned, I propose in the sequel to test within narrower limits, and by what seems to me a novel method, the validity of the conviction that a true understanding of phenomena and events can be attained only by viewing them in their interdependence and collective effect. If anything in the wide expanse of physical and mental life deserves to be considered as one and indivisible, it is surely human thought in its various branches and manifestations. The attempt to trace its origin in the early ages of civilisation, or to foreshadow the end which it is slowly approaching, may indeed be impossible; but of the age to which we belong, and the literature of which we have witnessed the growth, we may claim to possess a deeper knowledge. Astronomers have succeeded in gaining a view of immense and distant orbits by minutely observing and tracing merely an insignificant portion[1] which came within their view. Comparative anatomy teaches how from a few surviving links to construct the whole framework of an organism. I propose to apply a similar method to the small portion

mond's may be termed the philosophy of the Limits of the Knowable. Both views form a contrast to Lotze's philosophy.

[1] The most brilliant example of this is the discovery of the planet Ceres by Piazzi at Palermo in the New Year's night of 1801; the invention of special methods for calculating the orbit of this planet, which had been lost, by Gauss in the course of 1801; and the rediscovery of it by Olbers, aided by Gauss's [...]emeris, in the New Year's night of 1802. After the discovery of this first of the small planets, but before it was known in Germany, Hegel published his 'Dissertatio philosophica de orbitis planetarum,' in which he ridiculed the search for new planets, but which Duke Ernest of Gotha sent to the astronomer Zach with the superscription, "Monumentum insaniæ sæculi decimi noni." See R. Wolf, Geschichte der Astronomie, München, 1877, p. 684 *sqq.*

of mental progress of which I have been able to take personal notice and of which I have felt the immediate personal influence. A tracing as concisely as possible of this comparatively small portion of the course of European thought may be the first approximation to more accurate delineations, which themselves will be the means of gradually gaining a truer idea of the purport and significance that belong to the larger dimensions of the mental life of mankind.

This life does not consist in the accumulated knowledge of our century, not in the results of scientific inquiry deposited in libraries and museums, not in the many schools for learning and study, not in educational and social reforms, least of all in political and economic institutions. These are all external objects, which are capable of being described or photographed like the external objects of nature. The mental life of mankind consists in the inner processes of reflection, by which these external objects have been produced, by which man has been able to add to the physical creation of nature a new creation of his own, by which he has been able to change the face of the earth, and endow the objects of nature with an ideal meaning. To this end he is always inventing and using methods which change, suggesting and applying principles which turn out to be half true or totally fallacious, guessing at results and aims which have to be abandoned, inventing theories which are short-lived—in fact, erecting scaffoldings with the help of which he raises the structures of Society, Art, and Science: these remain as the historical testimonies of his activity; the scaffoldings are removed as of merely transient and temporary value; and yet they

30. What the mental life of mankind consists of.

alone constitute the mental life which interests us. Only so far as we have taken part in building the scaffolding, only in so far as we have witnessed the many contrivances which have been used, only in so far as we have seen the growth of any structure from small beginnings, from the first sketch of the architect, can we say that we know something of the mental life which lies hidden in and behind those external signs and documents. A closer study of what we ourselves have witnessed is thus the only way of attaining some insight into the workings of the mind—the spiritual life of mankind. We shall presently find that in science as well as in philosophy every period starts from certain assumptions and proceeds according to certain methods, that certain habits of thought become general, and certain views become accepted; but in the course of one or two generations we find those assumptions questioned, those methods criticised, a new habit of thought introduced, and those general views which seemed so natural and convenient giving way to new and altered ones. The whole fabric of society, the whole structure of science and knowledge, all the applications of art, have to be remodelled on new principles, and to meet our changed demands. Few indeed, very few, of the old creations remain. One or two so-called laws of science that survive, a few dozen books that are re-edited, half-a-dozen works of art and one or two great poems,—this is about all that our century will at its close have preserved as the living inheritance of its early years: all the others will be relegated to the growing bulk of historical records. Possessed of merely monumental interest as documents of a bygone life, these creations had to be left aside as incap-

31. Methods, the most approved, have their day, and cease to be.

32. One century does not inherit all of the past; it discards much.

able of marking or guiding any longer our onward career. A few centuries lapse, and posterity will look upon them as we do on the huge monuments of early Eastern civilisation, on the Sphinx in the desert or the Pyramids of Egypt, wondering by what ingenious contrivances they were raised, what amount of human work and suffering they represent, or what idea lived in the minds of those who planned and placed them where they still remain.

III.

1. Necessity of choosing a road.

It is the privilege of art to represent at a glance the whole of its object, and thus to produce at once a total effect on the mind of the beholder. Closer scrutiny may follow and may show how the various parts support the whole, how the uniting idea is revealed in all the manifold detail of the component elements: still the impression of the whole remains and supplies the key for the comprehension of every part. Literature, science, and history are denied this privilege of presenting their objects in their entirety, and thus giving from the outset a commanding view, a leading and abiding impression of the whole. We have to ask the student to follow us patiently by an isolated path to the summit: many ways lead to it, and we may err in the choice of the right and convenient one. Even if we succeed in reaching the central position, we may have fatigued the reader on the road or produced sensations which prevent the unbiassed contemplation of the whole view when it is presented. With us the whole is only the sum of its many parts, whereas with the artist the parts are merely fractions of a united whole. In

treating of the thought of the century, even within the narrow limits which have been prescribed, I am met with similar difficulties. In the large circumference of the domain of thought I have to choose a starting-point and to construct a road which may lead to the central position, hoping there to gain a comprehensive view of the whole.

2. Some periods of history take their name from some great event or movement.

Some periods of history are characterised by one great and central movement which absorbs all active forces and all intellectual and imaginative power, making them either subservient to one end and purpose, and helpful in the elaboration of one idea; or else forcing them into opposition, where they testify equally to the importance of this central movement. Such periods were, for instance, the long centuries of Jewish history, the early age of the Christian Church, the period of the culmination of Papal power, the Reformation, the French Revolution. In studying the thought of such ages, we are not at a loss where to find the leading idea,—we easily fix the centre of the vortex which draws into its motion all the existing forces, all genius and all talent. In an age like that of the Reformation we can speak of the Politics of the Reformation, the Religion of the Reformation, the Philosophy, Literature, and Art of the Reformation, and we are pretty sure to embrace under these various heads an account of all the mental progress and to trace all the thought of that age, be it friendly or antagonistic.

3. No central event in our age.

It is evident that no such central event, no such all-absorbing vortex of motion, exists in the period which we have lived through. The uniting bond, if it exists, lies much deeper; the problem we have been engaged in solving, the prize we are fighting for, does not present itself on

the surface; it is not explicitly stated, it must be implied rather than defined. The great object of our life and labour has not been clear to us, as it seemed clear to those who lived during the Reformation or the Revolution, otherwise we should not have philosophies of the Unconscious and of the Unknowable, and the century would not end in asking, Is life worth living?

Then, again, we find in history long periods of quiet development, where men's minds seemingly run very much in the same direction, exhibiting a general tendency of ideas, the spreading of a defined habit of thought and of simple methods, the application of a few principles: such a period was that preceding the French Revolution, the greater part of the eighteenth century. It has therefore been easy to characterise that century: it has been termed the philosophical century, the century of the *Aufklärung*, the century of Voltaire.[1] No such one

[1] The first who reviewed the literature of the eighteenth century from an international point of view was Villemain, who as early as 1820 was engaged in lecturing at the Sorbonne before the *élite* of the rising literary generation of France on the literature of the eighteenth century, taking France as the centre, and showing the influence of foreign literature, especially English, as likewise the reaction of French ideas abroad. He was too early to recognise the true meaning of the new spirit which had then already gone forth from Germany. In this respect his 'Cours de Littérature française,' published in 1828 and republished in 1864, remains incomplete. Schlosser next attempted to present in his 'Geschichte des achtzehnten Jahrhunderts,' after the manner of Gibbon, a picture of the combined political and literary work of the last century. The first draft of it appeared in 1824, after Schlosser had passed two years in Paris, where no doubt he must have come under the influence of Villemain. The work itself began to appear in 1826, and was finished in 1848. It is considered to be Schlosser's greatest work, and had a large circulation. The connection of political and literary history was studied by Gervinus, who with Häusser is usually counted as a pupil of Schlosser. But the great work which Villemain had begun and Schlosser taken up was adequately carried out by Hettner, who in his 'Literaturgeschichte des achtzehnten Jahrhunderts' conceived the whole intellectual movement of that age as a battle for enlightenment (*Kampf der Aufklärung*). The

term can be applied to our age, no one name can be found which carries with it the recognition of all the many interests which surround us.

4. Is history of thought history of philosophy?

It has been suggested by some that the history of thought is equivalent to the history of philosophy; that the different philosophical systems and theories exhibit in the abstract the course which ideas have taken in an age.[1] A history of thought in the nineteenth century would thus mean a history of nineteenth century philosophy. There have indeed been plenty of philosophies and systems during our period, but in spite of their great number and variety—ranging from the extreme idealism of Fichte to the equally extreme materialism of Büchner[2]—we feel that they do not cover the whole area of thought. The period in our century which in England was most barren in philosophy, the first forty years, produced an entirely new literature and a novel conception of art, both containing new sources of mental life, though they have hardly yet found expression in any philosophical system. Equally barren in speculation was France during the Restoration; yet there, too, was a

latter part of his work deals with the reaction against *Aufklärung* and "Rationalism" as it began in England, and was represented on the Continent by Rousseau and the earlier ideals of the French Revolution. Through Rousseau and the Revolution the growing influence of the new spirit of English literature was overpowered and lost for the Continent. And, as we have to regret in Villemain his neglect of the new life of Germany, so we have to deplore that Hettner followed the developments of Rationalism and *Aufklärung* only in the form they assumed in Germany, neglecting to notice the contemporary growth of the new life in English Literature and Art, to which, in fact, no German historian has as yet done justice.

[1] See especially Hegel's Lectures on the History of Philosophy in his collected works, vol. xiii. p. 68 *sqq.* (Complete edition, Berlin, 1832.)

[2] The principal publications of this school are Vogt, 'Physiologische Briefe,' 1845-47; Moleschott, 'Der Kreislauf des Lebens,' 1852; Büchner, 'Kraft und Stoff,' 1855.

brilliant era of literature, and the whole of Europe was illuminated by the light of science which emanated from Paris during the first third of this century. History of philosophy has little to say about Goethe, though his work embodies for us probably the deepest thought of modern times. Again, the only great and novel system of philosophy which France has produced during this century is that of Comte, but it has had only small influence in its own country; and who would say that it reflects French thought of the period as Voltaire and Montesquieu reflected the thought of the last century? Hegel himself, who was intent upon tracing the working of the human mind in the systems of philosophy, declared that philosophy is the latest fruit of civilisation,—that the special idea which governs any period is already dying out when it appears in a system.[1]

5. Goethe's work involves the deepest thought of the century.

[1] The principal passage expounding this idea of Hegel's is to be found in the introduction to the course of lectures which he delivered at Berlin repeatedly during the years 1816 to 1830. See his collected works, vol. xiii. p. 66: "Philosophy makes its appearance at the time when the mind of a nation has worked itself out of the indifferent dulness of the early life of nature, as well as out of the period of passionate interest; inasmuch as the direction towards detail has spent itself, the mind transcends its natural form—it passes on from practical morals, from the force of real life to reflection and comprehension. The consequence is, that it attacks this actual form of existence, these morals, this faith, and disturbs them; and with this comes the period of decay. The further stage is, that thought tries to collect itself. One may say, that where a people has come out of its concrete forms of life, where distinction and separation of classes has set in, where the nation approaches its fall, where a rupture has taken place between the inner desires and the external reality, where the ruling form of religion, &c., &c., does not satisfy, where the mind shows indifference towards its living existence or lingers discontentedly in it, where moral life is in dissolution—then only does one philosophise. The soul takes refuge in the realms of thought, and in opposition to the real world it creates a world of ideas. Philosophy is then the reparation of the mischief which thought has begun. Philosophy begins with the decline of a real world: when she appears with her abstractions, painting grey in grey, then the freshness of youth and life is already gone; and her reconciliation is not one in reality, but in an ideal world."

6. Philosophy retrospective. This means that philosophy is retrospective: it sums up, it criticises, it does not prefigure the future. The correctness of this proposition may be doubted. We shall have to deal with it in another place. At present it reminds us that thought, in the sense in which we take it, cannot be identified with philosophy, and hence a history of philosophy in the nineteenth century is not identical with a history of its thought. There is indeed a sense in which the word philosophy is sometimes used, when it approaches more nearly to the meaning of the word thought, as we intend to use it. Whewell has in this sense written the philosophy of the inductive sciences, meaning to trace in that work the processes of thought which are consciously or unconsciously employed in scientific research and reasoning, and which lead to

7. When does thought mean philosophy? progress in science. Something similar might be attempted in regard to art, commerce, politics, government, religion, and literature generally. In every case philosophy would simply mean the peculiar way of thinking and reasoning which is adopted in these various branches of practical or intellectual life. This is, however, not the sense in which the word philosophy is generally used. It generally denotes something more than a statement of method or a *rationale* of ideas and reflections; it denotes a definite theory, an explanation of a larger or smaller circle of phenomena. As such it certainly forms a part of the thought of the century, probably the most interesting and fascinating part; but it is also that which is most liable to change, most subject to discussion; whereas the other more hidden thoughts and reasonings form, as it were, the ground upon which all the

intellectual, artistic, and practical achievements of the age rest.

8. Inquiry into thought of the century divided into two questions. It would thus appear as if an account of the thought of the century might naturally divide itself into two separate investigations. In the first place, we should regard thought merely as a means to an end, as the method adopted to attain a certain purpose, be it practical or theoretical. It would mean the peculiar kind of reasoning which has been employed in the search for knowledge or in its useful application. As all reasoning starts from certain assumptions, called premisses, or principles, or axioms, and progresses from these by certain methods, this portion of our task would divide itself again into a statement of the principles which underlie, and an account of the methods which have guided, theoretical and practical reasoning. But thought does not exist merely for the sake of increasing our knowledge of things and of applying this to practical purposes. Occupied in this way merely, it remains fragmentary, incomplete, and not infrequently it reveals contradictions. Even those who devote themselves purely to detailed research or to practical work are again and again compelled to take a wider and deeper view of things than their special occupation affords. One may find that the methods which he is using daily become useless for certain practical purposes he has in view, and may thus be forced to question the principles which during half his lifetime he has applied with unquestioning faith in their validity and usefulness. Another may have met with such success in the use of a special method of research, that he wishes to apply it to subjects which were previously handled in a different manner, or elevate it to

the dignity of a general rule of thought. A third may, accidentally, be interested in two or more pursuits which are seemingly unconnected, but which—being brought side by side in his mind—he feels the wish to unite and harmonise. A fourth may, at a certain time of life, grow tired of the drudgery of petty pursuits which never carry him beyond a very limited sphere of interests: he is tempted to look beyond this narrow range, and gain some wider view of other pursuits and interests. Allowing that ignorance or indifference prevents even the majority of those whose powers are not exhausted in the struggle for mere existence from looking much beyond their narrow circle, allowing also that many of us live—like children—in a blessed trust that the great and important interests of mankind are under higher and better guidance than we can understand or control, there still remain a considerable number of persons who are always on the look-out for something higher, wider, and better, who are driven by an undying thirst after real wisdom, or by an
9. Speculation. inherent restlessness of disposition to inquire into the deepest foundations and the ultimate ends of the world and life. Language has coined a word which denotes the whole of these occupations and endeavours, how various so ever they may be, and for whatsoever purpose they may be undertaken. It calls them speculations. The word also indicates the venturesome and risky nature of these undertakings. They have existed in all ages and countries and languages wherever literature has existed, and have been carried on by the powers of reason or imagination, in prose, verse, or symbol, sometimes in defined and clear terms, more often in mystic allegory. Philosophy may be

said to have grown out of these vague and scattered beginnings by the attempt to conduct them according to some method, and to unite them into a complete and consistent whole. Philosophy may thus be defined as speculation carried on according to some clear method, and aim- 10. Philosophy defined.
ing at systematic unity.[1] Both science and philosophy may be called methodical thought, but the word system is applicable only to the higher and more advanced forms of philosophic thought which aim at unity and completeness.

We have thus arrived at a second division of our subject. In the first we have to consider thought merely as a means to an end; in the second we have to consider it as its own object, as a reflection on itself, carried on with the object of knowing its own origin, its laws, its validity, of testing its powers, and with the end and aim of gaining certainty, completeness, and unity. The whole of this great division of thought I shall comprise under the 11. Division of the book.

[1] This view of the nature and object of Philosophy agrees with Lotze's definition (see 'Grundzüge der Logik,' Leipzig, 1883, § 88): "The *common culture of life* and the *separate sciences* contain a number of suppositions the origin of which is obscure to us, because they have been very gradually formed within us through the comparison of many experiences, or because they have first become conscious by means of such experiences, have then received definite names and become habitual without having been subjected by us to any examination as to the reason, the sense, and the extent of their validity. In this way science and life make use of the notions of *cause* and *effect*, of *matter* and *force*, of *means* and *end*, of *freedom* and *necessity*, of *matter* and *mind*, and they frequently entangle themselves, owing to the above-mentioned defect, in contradictions, inasmuch as they are unable to fix the limits of validity of these to some extent contradictory assumptions.

"Now we may formally define the task of Philosophy as follows: that it is an endeavour to import unity and connectedness into the scattered directions of cultured thought, to follow each of these directions into its assumptions and into its consequences, to combine them all together, to remove their contradictions, and to form out of them a comprehensive view of the world; mainly, however, to subject those ideas which science and life regard as *principles* to a special scrutiny, in order to determine the limits of their validity."

term Philosophy; and as the first part will deal with the scientific, so will the second deal with the philosophical thought of our century.

Science has gradually risen out of the mass of accumulated but inaccurate and disorderly knowledge by the desire of making it accurate, orderly, and useful. Philosophy has similarly emerged from the great world of speculative thought by the desire of carrying it on methodically and for a defined end and purpose. Nevertheless neither the one nor the other, nor both together, really exhaust the whole meaning of the word "Thought"; neither science nor philosophy covers the whole region of thought. Both are comprised under the term methodical thought; but there remains the great body of immethodical, undefined thought. This is buried in general literature, in poetry, fiction, and art; it shows its practical influence in the artistic, moral, and religious life of our age. It is a reflection of the knowledge of science or the light of philosophy, but, like all reflected light, it not only follows, it also precedes the real and full light: it is not only the dusk that comes after, it is also the dawn that comes before the day, it is the twilight of thought. In it lie hidden the germs of future thought, the undeveloped beginnings of art, philosophy, and science yet unknown and undreamt of; it encloses and surrounds the innermost recesses of the mind, where all thought had its origin, and whence it ever and again draws fresh life and inspiration.[1]

12. Neither science nor philosophy comprises the whole meaning of the word thought.

13. Thought also hidden in the literature and art of the age.

[1] This is originally a Leibnizian idea. It is laid down in the doctrine of the *petites perceptions*, as given in the introduction to the 'Nouveaux Essais,' and referred to in many passages of Leibniz's various

No account of the thought of our century would be complete or satisfactory which took no notice of this great volume of immethodical and unsystematic thought which lies buried in the general literature and in the art of the age. Both have shown a vitality, originality, and versatility which exceed that of any except the few favoured periods—those of Athens under Pericles, Italy during the Renaissance, and England under Elizabeth. In one of the arts, in music, our age has, according to the opinion of many competent judges, exceeded in originality and certainly in productiveness all former ages. In poetry Goethe and Wordsworth have raised our tastes and demands to a higher level, in fiction France and England have almost created a new branch of literature, whilst the peculiar features of modern English landscape-painting were unknown to previous centuries. All this, though produced under no scientific or philosophical rule

14. Goethe and Wordsworth raised our tastes.

writings. See 'Nouv. Ess.,' Preface, Leibniz, Philosophische Werke, ed. Gerhardt, vol. v. p. 48:—

"Ces petites perceptions sont donc de plus grande efficace par leur suites qu'on ne pense. Ce sont elles qui forment ce je ne sçay quoy, ces gouts, ces images des qualités des sens, claires dans l'assemblage, mais confuses dans les parties, ces impressions que des corps environnans font sur nous, qui enveloppent l'infini, cette liaison que chaque estre a avec tout le reste de l'univers. On peut même dire qu'en consequence de ces petites perceptions le present est gros de l'avenir et chargé du passé, que tout est conspirant (*σύμπνοια πάντα*, comme disoit Hippocrate) et que dans la moindre des substances, des yeux aussi perçans que ceux de Dieu pourraient lire toute la suite des choses de l'univers.

"Quæ sint, quæ fuerint, quæ mox futura trahantur. . . . C'est aussi par les perceptions insensibles que s'explique cette admirable harmonie préestablie de l'âme et du corps, et même de toutes les Monades ou substances simples, qui supplée à l'influence insoûtenable des uns sur les autres, et qui au jugement de l'auteur du plus beau des Dictionnaires exalte la grandeur des perfections divines au delà de ce qu'on eu jamais conçu."

The importance of this idea of Leibniz has been dwelt on at length by Kuno Fischer in his 'Geschichte der neueren Philosophie,' where he also traces its influence in the development of philosophy and literature in Germany after Leibniz.

and very frequently outside of any school, points to novel modes of mental conception, to a fund of ideas yet undeveloped or only partially developed into clear thought. The whole of this productiveness indicates a vast amount of mental work which, though not yet absorbed by science or philosophy, belongs nevertheless, according to our original conception, to the world of thought. The meaning of it may be enigmatical, and the clear expression which it will some day produce in philosophical and scientific reasoning may be far distant and unintelligible

15. Unmethodical thought.

to us now. Still there it is, this great body of undefined thought, this volume of diffused light, the focus and centre of which is still hidden from us. We feel that in discussing the thought of the century we cannot pass it by or neglect it.

It is difficult to find any one term under which we could comprise this great body of unmethodical, scattered, and fragmentary thought,—any one word, similar to science and philosophy, in which we could sum up and characterise its general meaning and tendency. So far we have only stated what it is not, what to a large extent it perhaps never will be—*viz.*, methodical. And yet we feel that it contains that kind and portion of thought which touches our deepest interests, our most intimate concerns, our noblest aspirations. Science becomes more and more a mere calculation, *une question d'analyse*, an occupation for the laboratory, the workshop, the manufactory, and the market; philosophy savours at its best too much of the school and lecture-room, runs too much into systems and categories, it fatigues us with definitions

and abstractions. But neither calculation and measurement, nor definition and abstraction, suffice to exhaust what is to us, in the quiet and serious moments of life, of the deepest concern—*viz.*, our religion. I use the word here in its original sense, and I propose to sum up in the term religious thought the whole of the thought contained in that large volume of literature which does not submit to scientific and philosophical treatment, but which nevertheless forms so important an outcome of the mental life of the century.

16. Summed up in the term religious thought.

There are other words more or less current in modern literature that may serve to throw some light on the distinction that I am here drawing for the purpose of affording a preliminary view of the course to be pursued in the following treatise.

17. Science is exact, positive, and objective.

Science is said to be exact, positive, and objective, and it is opposed to such other thought as is inexact, vague, and subjective. Science is said to convey its results or ideas in defined, direct, and general terms, whereas there is a large department of literature and thought which moves in undefined, symbolical, and indirect expressions. Science professes to rest on clear and precise knowledge, and is thus opposed to such other realms of thought as rest on opinion, belief, and faith. It may be well to note here that these different terms refer either to the method of treatment or to the matter which is under treatment. Science alone professes to have a rigid and undisputed method. Other branches of thought either borrow their methods from science, or they have fluctuating, not generally recognised methods, or they refuse to submit to method

altogether. But so far as the matter under treatment is concerned, a clearer division is possible. Science deals with all such things or objects of thought as are common to a great many persons and—under certain circumstances—are accessible to everybody: it thus claims that its observations and reasonings can be checked and submitted to repeated examination and verification; so that a large portion of them can always be regarded as settled and agreed upon, and can be taken for granted and used as a secure foundation by those persons who are themselves unable or unwilling to go through the process of verification. But

18. Some interests or objects of thought are personal or subjective.

there are a great many things and interests which centre in the individual mind of each person—which are, in fact, personal, individual, or subjective. They are to all of us just as important as the others. They form the real subject-matter of all that thought which is separated from science, and in its very nature and aspect opposed to it. In this great province of thought one person cannot do the work for many in the same way as is possible in

19. Agreement on these matters impossible.

science. Proof is almost impossible, and agreement refers always only to a certain number of persons. Doctrines or theories in this region of thought cannot be accepted and taken for granted as they are in science, but every person must go over the same ground for himself before he has any right to accept or make use of what is given to him. The real and true character of all this thought is that it is individual and personal, whereas all scientific thought—whatever its origin may be—must be general and impersonal. At the extreme end of thought in one direction are placed the mathematical sciences, at the extreme end in the other lies religion. Disagreement in the former is

almost as unknown[1] as agreement in the latter. There we have an almost universal unity of thought; here unity of thought probably never existed; it is unknown. Popularly we can say that at the one extreme lie knowledge and certainty, at the other faith and belief. There is, however, a very large extent of ground between these two extremes. This is covered by all such intermediate thought as rests partly on knowledge, partly on faith, where certainty is largely mingled with belief. This large intermediate region, where changes and fluctuations are frequent and rapid, is the proper home of philosophy, which occupies itself with the grounds of certainty and belief, the origin of knowledge and faith, and the relations in which both stand to each other. Were all our thoughts either purely mathematical—*i.e.*, referring to number, measurement, and calculation, or purely religious—*i.e.*, referring to our individual concerns and personal convictions,—the need of a continued compromise or mediation would be unnecessary, the question as to the grounds of certainty or belief would never arise. But no sooner do we wish either to apply our strict mathematical notions and processes, or to bring our personal convictions into practical use, than the two kinds of thought come into contact, not to say into conflict, and there is need of some theory according to which this contact may be regulated, this conflict settled. And as the occasions for such contact change with the demands of practical life, or

20. Philosophy intermediate between mathematical science and religion.

[1] It may be doubted whether this is quite correct, looking at the controversies which have been connected with many mathematical theories—such as the theory of parallel lines, the meaning of infinitesimals, the correct measure of force. These controversies, however, referred really to applied, not to pure mathematics, and were settled by introducing correcter and more stringent definitions.

the progress of applied science, these theories must themselves change and develop. Now it may be generally stated that it is the task of philosophy to take note of these different ways by which the strict methods of science are applied and made useful, or by which personal and individual convictions are brought to bear upon practical questions which are not only of personal but of general interest and importance. It does not follow that philosophy must necessarily construct a complete system; but it is a natural and frequent occurrence that the occupation with a great number of detached theories or aspects of thought generates the desire to bring them into harmony and to unite them in a connected whole. Thus the enterprise which was originally purely critical and preparatory, and undertaken merely as a means to an end, may lead to the formation of a general and all-embracing view of things —*i.e.*, to a philosophical system.

21. Threefold consideration of thought: scientific, philosophical, individual.

From whichever side we approach the matter, we are thus always led to a threefold consideration of thought, as scientific, as individual, and as philosophical. An attempt in which any of these three aspects were neglected could have no value in an account of the thought of our age. There have indeed been schools of thought which identified science with philosophy, or which maintained that no independence belonged to religious, personal, or individual thought, inasmuch as this was merely of a derived character. Though such theories may have exerted considerable influence, they have as a whole failed,[1]

[1] This can be said of Hegelianism as well as of Comtism. In the former it was a favourite doctrine that philosophy was the higher wisdom compared with religion and art. See Hegel, 'Geschichte der Philosophie' (Werke, vol. xv. p. 684): "The highest aim and interest of philosophy is to reconcile thought, the idea, with reality.

and we find ourselves at the end of a long and critical period unable to say that any one of the three realms of thought has gained an undisputed victory over the others. Science is more than ever that kind of thought which gives knowledge and certainty. Religion is still the generally recognised abode for those convictions which refer to our deepest personal interests. And more than ever do we feel the need of a reconciliation of both in some theory of life which is neither purely scientific nor purely individualistic; and this means that philosophy is as much needed as ever. Our century has witnessed a great development of scientific thought, a great revival in religious interest, religious feeling, and religious activity, and it is probably richer than any preceding age in philosophical theories and systems.

22. Philosophy the mediator between science and religion.

I must repeat here what I said above, that it is a misfortune that in dealing with a complicated subject we are obliged to divide it,—that we are forced to give preference to some one aspect, and to choose a special

Philosophy is the veritable theodicy, compared with art and religion and their sentiments—this reconciliation of the mind, indeed of that mind which has grasped itself in the freedom and wealth of its reality. It is easy otherwise to find satisfaction in subordinate regions of intuition and feeling," &c., &c. Although it is an exaggeration to say that Hegel desired to absorb or evaporate religious belief in philosophical knowledge, as his lengthy explanation (Introduction to the 'History of Philosophy,' Works, vol. xiii. p. 77 *sqq.*) sufficiently proves, there is no doubt that the sentiment expressed in the above passage indicates that philosophy was coming to the rescue of true religious belief, which threatened to be lost in the rationalistic and mystical schools of the day. And this had the further consequence that a scientific occupation with or interest in religious subjects—be it metaphysical or historical—took the place of a purely religious interest, and that many eminent German theologians became either pure metaphysicians or merely critics, the practical side being lost sight of.

It is probably just as incorrect to accuse Comte of an intention to destroy true religion because he preached the well-known doctrine of the three stages of human thought—the theological, the metaphysical, and the scientific or positive.

point from which to set out. In dealing with the thought of our age, I have been obliged to divide what is in reality connected and coherent; and I am further forced, in examining more closely its different aspects, to select one as the most prominent with which to make a beginning. In reality such a preference does not exist in my plan. I recognise all the aspects of thought as equally important, and I feel that I might begin with any one of the three, and that I should in due course be led on to a consideration of the other two. They are in their actual historical appearance in the course of our period so interwoven that they cannot practically be separated. And it is indeed not difficult to assume various positions in contemplating the whole subject from which either one or the other of the three forms of nineteenth-century thought assumes as it were the ascendancy. Thus it would be undeniable that from a German point of view the great movement of ideas centred in the first third of the century in what I have called philosophy. The number of systems which succeeded each other was astonishing, the influence they had on literature, science, and practical life was without precedent, the enthusiasm with which students from all parts gathered in the lecture-rooms of the great metaphysicians was quite extraordinary, and probably equalled only in the schools of Athens in antiquity, or in the lecture-room of Abelard in the middle ages. From this point of view an account of this great movement—how it grew, flourished, and died away—would no doubt afford a suitable introduction to the history of thought in our century. If after this we were to turn to France and try to fix upon the

23. Difficult to separate the three aspects of thought.

most striking intellectual feature of the century, it would be the equally great and remarkable array of scientific names of the first magnitude. In France during the early part of the century the foundation of nearly all the modern sciences was laid; many of them were brought under the rule of a strict mathematical treatment. It was there that scientific subjects were made so popular, and clothed with a garment of such elegant diction, that they have since that time greatly entered into general consciousness, and have promoted in literature and art an independent school — the naturalistic. Compared with this mathematical and naturalistic spirit, philosophy proper has found but a meagre development and culture in France: the constructive tendency of idealism has found nourishment for the most part only in leanings to the older systems of Descartes, Plato, and Aristotle, or to the foreign ones of Hegel and other German metaphysicians. Compared with Germany in philosophy, and with France in science, England during the early part of the century appears remarkably unproductive. English science and English philosophy had flourished in the seventeenth and eighteenth centuries, and leavened the whole of European thought, but in the beginning of our period we find neither represented by any great schools. The great discoveries in science belonged to individual names, who frequently stood isolated; the organisation and protection which science could boast of in France was then unknown in England; into popular thought it hardly entered as an element at all. Metaphysics had not recovered from the blow which David Hume had struck, and speculation was confined

24. French thought centred in science during the first part of the century.

25. State of philosophy in the early part of the nineteenth century in England.

almost entirely to the novel field of social and economic problems. But against this there was a young growth of ideas springing up in the poetic literature of the nation. It is the freshness of individual thought as clothed in the poetic language of Shelley and Wordsworth, maturing and deepening in the works of Tennyson and Browning, which strikes us as the most original phase of English thought in this century, whether we compare it with Continental thought of the same period, or with English thought of the previous age.

26. Goethe's 'Faust' representative of the thought of the century.

And lastly, we might be tempted to make the great work of the greatest mind of the early part of our period, Goethe's 'Faust,' the centre and beginning of our survey, singling it out as a comprehensive embodiment, as the classical expression of nineteenth-century doubts and aspirations, leading us—if we try to understand it—now into the bewildering labyrinth of philosophy, now into the cheerful expanse of natural science, and again into the hidden depths of individual life, of religious faith with its mysteries of sin and redemption.

But from whatsoever point we may start on our journey, from whatsoever easily reached eminence we may cast a first eager glance across the wide country which we wish to explore, there is one feature which impresses itself alike upon our minds from the very beginning. It is not a country of repose and restfulness, of healthy industry and quiet work, of gradual development, of ripening crops, of sowing or ingathering; it does not present the aspect of a happy division of labour, of successful co-operation, of peaceful regulation of employment. It looks more like a land which has lately been disturbed by

27. A period not of repose but of ferment.

great elemental forces, heaved up by an earthquake or visited by a destructive storm. We see some persons employed in filling up great breaches and recently made rents, others trying to lay new foundations; others again are fighting for their possession or trying to divide a disputed territory; even the peaceful workers are called out to help in the battle, or disturbed by the complaints of their neighbours, on whose ground they are trespassing unawares, whose foundations they are unconsciously undermining. If we inquire into the cause of this unrest and anxiety, which seems to be a feature common to nearly all the phases of nineteenth-century thought, we must look back to the age which immediately preceded it. It is the storm of the revolution which passed over Europe, and shook to the foundation all political and social institutions, that has likewise affected our ideas and thoughts in every direction. The period we refer to has thus not incorrectly been termed a century of revolution. If in spite of this I decline to consider nineteenth-century thought as essentially revolutionary, it is because the work of destruction belongs in its earlier and more drastic episodes to the preceding age. The beginning of our period witnesses everywhere the desire to reconstruct, either by laying new foundations or by reverting to older forms of thought and life which it tries to support by new arguments or to enliven by a fresh interest and meaning. We may say that the thought of the century in its practical bearings is partly radical, partly reactionary,—meaning by the former all those constructive attempts which try to go to the root of things and to build up on newly prepared ground; by

28. Cause of it seen in the century of revolution preceding it.

29. Nineteenth-century thought not revolutionary.

30. Thought of this century partly radical, partly reactionary.

the latter all those endeavours which, clinging to historical institutions and beliefs, aim at finding the truth and value which are in them, and the peculiar importance which they may have for the present day. The work of destruction is indeed still going on; in the midst of this constructive or reconstructive work we still witness the workings of the revolutionary spirit.

31. The thought of Burns, Wordsworth, and Coleridge disturbed by the Byronic school.

The healthy new life which Burns, Wordsworth, and Coleridge infused into English poetry at the beginning of our period was disturbed in its quiet growth by the revolutionary spirit of the Byronic school. The new thought, which grew up in Kant's philosophy and the idealistic school, degenerated in its further development into a shallow materialism and a hopeless scepticism. But none of these destructive influences, however passingly interesting they may have been, seem to have struck out any new line of thought.

32. Destructive spirit in writings of eighteenth century.

Whoever wishes to study the arguments by which social order was subverted and cherished beliefs destroyed will find them brilliantly and consistently expounded in the writers of the eighteenth century, from which many nihilists of our age have drawn their inspiration. This is not the task which I have in view. It has been performed in our time by many writers of great eminence. Nor do I intend to describe the courses which governments and politicians have taken in dealing with the legitimate demands of the people, such as a hundred years ago found a memorable expression in the American Declaration of Independence, and an exaggerated one in the cry of the French Revolution. Only to a small extent has the ideal of that great movement, as it lives in the mind of many a democratic leader, been realised in our century. In

most European countries the work of national unification and consolidation, and the struggle for political independence, have retarded internal reforms; nor have theorists been able to agree in what form of social organisation liberty and equality could consistently live side by side. Their teaching must indeed command special attention

33. Revolutionary theories not practical.

as one of the many forms of the philosophic thought of the age; but a wide gap separates theory from practical politics, which have been largely occupied with wars and diplomatic feats, or, when they really dealt with social problems, have had to be content with awkward compromises between prejudices and institutions of bygone ages on the one side, and legitimate demands for freedom on the other.

Though much practical thought and much labour have been spent in achieving even these moderate results, I feel that they really fall outside of my programme. Wherever either science or philosophy steps out of the quiet regions of the study, the lecture-room, and the laboratory, or wherever religious faith leaves the secret recesses of the believing soul to solve the problems of life or to perform the work of the day, the line is crossed which I have felt obliged to draw around the following sketch. Not that I do not recognise this borderland, where the spirit subdues matter, where thought becomes useful, where the idea attains reality, this field of strife and endeavour, of patient toil and slow victory, as by far the most important subject of history, and as that in which our age has probably excelled every earlier period. But an account of this side of nineteenth-century life could ill afford to limit its view to the three principal countries of the Old World. For where are discovery and invention at this moment more at home

than in America; where have political theories, the original rights of man, the ideas of liberty, equality, and brotherhood, been more widely put to the test; where have religious beliefs entered into closer contact with the work of the day; or where in our age has the simple rule of early Christianity been more successfully put into practice? An account of the application of thought taken merely from our European experience, where half our endeavour must always be spent in clearing away obstacles, in removing the *débris* of antiquated institutions, in overcoming prejudice, or battling with evils which have grown to uncontrollable magnitude, would give us but a poor notion of the influence of thought over material circumstances, and a very exaggerated one of the inertia of the mechanism of older societies. With the work of the inventor, the practical statesman, or the lawgiver, I have thus nothing to do at present; only in cases where practical problems have immediately reacted upon scientific research, or where social questions have given rise to special theories, shall we be compelled to cast a glance outside of the inner world of thought into which I invite my readers to retire.

34. This is not a history of invention or of practical politics.

This inner world has, indeed, not been all rest and peace and quiet development. No age has been so rich in rival theories, so subversive of old ideas, so destructive of principles which stood firm for many ages, as ours. It is not my intention to emphasise this critical or radical tendency more than is necessary. True to the original view which I have already expressed, I intend to look upon thought as a constructive, not a destructive agency; on the world of ideas as a positive acquisition, not as a mere counterpart

35. Thought to be considered in its constructive, not in its destructive attitude.

or shadow of material existence. Though demanding for its growth an outer stimulus, and unable to proceed very far without external correctives, I nevertheless maintain that the human mind in its individual and collective life encloses an independent source of reality which contact with outer things and thought in all its various forms has to reveal, to preserve, and to develop. To what extent this has been done in our century is the question I propose to answer. With this object in view I shall try to gather my observations and my narrative around the prominent and novel constructive ideas which have sprung up in the course of the century, not omitting the great development which the purely formal side of thought, the method of research, has undergone. Such constructive ideas are those of energy, its conservation and dissipation; the doctrine of averages, statistics, and probabilities; Darwin's and Spencer's ideas of evolution in science and philosophy; the doctrines of individualism and personality, and Lotze's peculiar view of the world of "values" or "worths." Around these centres of thought cluster the many critical oppositions, the great controversies of radical or conservative opponents. As regards these, I shall welcome all radicalism which lays bare the roots of our ideas, which delves deep into the ground of our opinions and principles, or which points out new methods by which we may test the correctness and consistency of our axioms. As such I consider the spirit infused by Kant into all modern thought. That other radicalism, which merely roots up, which destroys without building, which fails to find any ground of certainty, simply because human thought and observation may after

36. Darwin's, Spencer's, and Lotze's constructive ideas.

37. The right sort of radicalism.

all be a delusion,—this kind of radicalism I shall try to pass over as meaningless. And equally meaningless appear to me those opposite conservative tendencies which merely annul progress, which shut out the daylight, and preach the doctrine of inertia. But this, again, will not prevent me from recognising the real gain and interest which belong to some reactionary movements, such as lay at the bottom of Romanticism, with its love of the past, its artistic idealisation of the childhood of mankind, of aspects of life in their infancy and primitiveness, with its study of mediævalism and its more sober historical tastes. I shall endeavour always to ask what addition to the great stock of human ideas has resulted; what gain we have to register; convinced that everything that lives must grow, increase, and multiply: and what can be more living than Thought?

38. Reactionary movement of Romanticism.

But although the school of Critical Thought in Kant, and the Romantic school as centred in Walter Scott and the German Romanticists, are in time almost the first intellectual phases of the century, they will not in the beginning command my special attention.[1]

[1] In order to give some idea of the complexity of the different currents of thought in the first years of the century, I place here a carefully selected list of dates. They refer to events or publications which mark epochs or important stages in the history of thought. Of specifically *scientific* importance are—

1796. Laplace's 'Exposition du Système du Monde.'
1799. (2 vols.)—1825. Laplace's 'Mécanique céleste.'
1799. Legendre's 'Théorie des Nombres.'
1801. Gauss's 'Disquisitiones Arithmeticæ.'
1801. Piazzi discovers and
1802 Olbers rediscovers the first of the minor planets, "Ceres," being assisted by Gauss's new methods of calculation, which were published *in extenso* in
1809. Gauss's 'Theoria motus corporum cœlestium.'
1798. Cuvier's 'Tableau élémentaire d'Histoire naturelle.'
1800-5. Cuvier's 'Leçons d'Anatomie comparée.'

Though somewhat later in point of time than they, the school of exact research seems to have become the more generally recognised agent in nineteenth-century progress.

1809. Lamarck's 'Philosophie zoologique.'
1799. Volta constructs his first electric pile, and announces this in
1800 to Sir Joseph Banks.

In chemistry the early years of the century brought many of Gay-Lussac's important Memoirs, in
1801 Humphry Davy publishes the first of his electro-chemical discoveries, and
1802-3 Berzelius publishes his own.
1803. Berthollet's 'Essai de Statique chimique.'
1810. John Dalton's 'New System of Chemical Philosophy.'
1801. Thomas Young announces to the Royal Society his belief in the undulatory theory of light, which during
1802, 3, and 4 he substantiates further in his papers, and fully expounds during
1802 and following years in his lectures to the Royal Institution.
1808. Malus announces his discovery of the polarisation of light through reflection.
1802. Chladni's 'Akustik.'

Count Rumford's papers, which laid the foundation of the mechanical explanation of heat, belong to the end of the last century, and in
1799 H. Davy publishes his equally important 'Essay on Heat, Light, &c.'
1800. Bichat's 'Recherches physiologiques.'
1801. Bichat's 'Anatomie générale.'
1799-1804. Alexander von Humboldt travels in America, and lays by his observations the foundation of the sciences of physical geography and meteorology.

For the history of the *philosophical* movement of thought the years
1793-1806 witnessed in Germany the great development, expansion, and criticism of Kant's ideas in the writings of
1793 Schiller, 'Briefe über ästhetische Erziehung.'
1796. Schiller, 'Über naive und sentimentalische Dichtung.'
1797. Fichte, 'Wissenschaftslehre.'
1797. Schelling, 'Naturphilosophie.'
1803. Schelling, 'Transcendentaler Idealismus.'
1799. Schleiermacher, 'Reden über die Religion.'
1800. Schleiermacher, 'Monologen.'
1799. Herder, 'Metakritik.'
1799. Jacobi, 'Offener Brief an Fichte.'
1806. Hegel, 'Phänomenologie des Geistes.'

In France—
1804. Destutt de Tracy's 'Idéologie' represents the reigning philosophy, and
1803 Maine de Biran's 'Mémoire sur l'Habitude' the beginning of the later reaction against it.

In England—
1792-1827. Dugald Stewart's 'Elements of the Human Mind' and his

To it are due the great changes in every department of science, of life, and probably also of literature and art, the great inventions and the great conflicts of our age. Science has not only very largely influenced our ideas, it has also by its applications altered the external face of the world we live in. It is therefore simply a tribute to the popular view, and a desire to start from some striking and generally conceded position, if I select the scientific movement of ideas as the first with which

39. Scientific progress to be considered first.

I have to deal. How has it spread in the course of the century? From what beginnings and through what influences? What are its principles and methods? How have they themselves changed and developed? What has it led to? These are some of the questions which

1803 'Life and Writings of Thomas Reid' represent the predominant Scottish philosophy, and
1804 Thomas Brown, 'Inquiry into the Relation of Cause and Effect,' the beginnings of the later associationalist school. At the same period Jeremy Bentham's influence, which cannot be reduced to special dates, had already acquired European if not world-wide importance. His long life (1748-1832) was contemporary with Goethe's (1749-1832), whose 'Faust' was given to the world in successive stages between the years 1790 and 1832.
1794. Thomas Paine's 'Age of Reason.'
1798. Malthus's 'Principles of Population.'
Literary criticism started on a new era and extended its influence in
1802 through the 'Edinburgh Review,' and
1808 the 'Quarterly Review'; in Germany somewhat earlier in
1794 Schiller's 'Horen.'
1797. Schiller and Goethe's "Xenien" in the 'Musenalmanach.'
1798. Schlegel's 'Athenæum.'
1802. A. W. v. Schlegel's Berlin lectures.

The *Romantic school* of fiction dates in Germany from 1798, when Frederick Schlegel uses the term for the first time as characteristic of a new departure in his review of Goethe's 'Wilhelm-Meister' ('Athenæum,' vol. i.) A literary movement with frequently similar aims and characteristics is represented in this country by Walter Scott ("Lay of the Last Minstrel," 1805), Southey ("Thalaba," 1802), and Coleridge ("Christabel," 1806), and spreads later into France. As the great source of the new and original *poetic* inspiration of nineteenth-century poetry we have the "Lyrical Ballads," 1798, and besides 'Faust,' the other principal works of Goethe and Schiller (died 1805).

I shall try to answer as concisely as possible. This selection does not commit me to any theory on the value of the scientific view as compared with other aspects. Such theories will have to be dealt with in a later portion of the work. They have sprung up in the course of the last hundred years, partly as the inevitable outcome of scientific progress itself, partly in the educational world, where a reaction has set in against the undue importance which former generations attached to classical learning and training. I need not at present do more than note these opinions, nor need I define my position with regard to Comte's celebrated positivist theory on the advancing stages of the human intellect. Curiosity and the consensus of popular opinion suffice for the moment to make me take up the scientific side of the thought of the age. As we proceed, other directions and movements will present themselves, and the interdependence of all human interests will reveal and explain what truth attaches to Hegel's celebrated doctrine of the inherent dialectic of ideas, the spontaneous development of thought.

40. Hegel's doctrine of the spontaneous development of thought.

A HISTORY OF EUROPEAN THOUGHT IN THE NINETEENTH CENTURY

PART I.

SCIENTIFIC THOUGHT

CHAPTER I.

THE SCIENTIFIC SPIRIT IN FRANCE.

It will be generally admitted that the scientific spirit is a prominent feature of the thought of our century as compared with other ages. Some may indeed be inclined to look upon science as the main characteristic of this age. The century may thus be called with some propriety the scientific century, as the last was called the philosophical century, or as the sixteenth was termed the century of the Reformation and the fifteenth the century of the Renaissance. It is therefore natural that we should begin our study of the thought of the age with an examination of this side of modern culture. 1. Our century the scientific century.

It is not necessary to *define* what I mean by science.[1]

[1] The use of the word science and its adjective scientific has varied considerably in the English language. We must wait for Dr Murray's great work to give us a history of the word. I venture to assert that it acquired its present definite meaning about the time of the formation of the British Association for the Advancement of Science (1831). The two other great organisations which professedly started for the culture of what we now call science—*viz.*, the Royal Society for the Improvement of Natural Knowledge, and the Royal Institution—did not use the word officially in their charter or title, although it is used frequently in the documents and correspondence connected with the foundation of the younger, and occasionally in those referring to the older Society. The Royal So-

Schools and colleges of science, triposes, examinations, and degrees in science, have established a popular meaning which did not exist a hundred years ago, but which is now well understood. For my purpose it is of some interest to note that the meaning of the word in French is somewhat different, and that the word *Wissenschaft*,[1]

ciety, and sometimes the Royal Institution, use the word "philosophy" in formal and official statements of their object. This is in accordance with older English usage. What we now universally call science was not infrequently termed in the seventeenth century natural knowledge, and Bacon himself translates *scientiæ* by "knowledge," by "learning," and sometimes by "sciences." In France, on the other hand, the word "science" seems to have acquired its present meaning as far back as the middle of the seventeenth century. At the time of the foundation of the "Académie des Sciences," in 1666, the word was used almost in the same sense — embracing the same separate departments of knowledge—as the word "science" is now used in this country when we speak of a college of science. In France, so far as I am aware, a cultivator of science has never been called a philosopher. Science and philosophy have there never been synonymous. But science in France has been made to cover a larger field of knowledge by such adjectives as "moral," "social," "political," and has been narrowed by such other adjectives as "exact" and "natural," in the same way as the word philosophy has been more strictly defined in the English language by the adjectives "natural," "experimental," "moral," "mental," &c. At the head of the sciences in France stood "mathematics," at the base of the new philosophy in England stood "experiment" and "observation."

[1] The word *Wissenschaft* has a much wider meaning than science in the modern sense, and is the literal translation of the Latin *scientia*. It means knowledge in a systematic form and connected by some method. What the French call *science*, the Germans call *exacte Wissenschaft*. This includes mathematics and *Naturwissenschaft*, which covers the ground covered by the word "sciences" in English. The word *Wissenschaft* plays an important part in German culture, as we shall see later on. The modern term "scientist" is about synonymous with the word *Naturforscher* in German. The word *savant* in French has no synonym in English, but is about equivalent to the term *Gelehrter* in German; and this, again, is partially translated by "scholar" in English. I suppose "man of science" and "scholar" together would be about covered by either *savant* or *Gelehrter*. Those who desire to study the older and modern, the English and foreign, uses of the word science and other kindred terms, should read Bacon's English writings; Weld's 'History of the Royal Society' (1848, vol. i.); Bence Jones's 'The Royal Institution' (1871); Léon Aucoc's 'L'Institut de France' (Paris, 1889); Alfred Maury, 'Les Académies d'autrefois' (vol. i., Paris, 1864); and the correspondence in connection with the foundation of the British Association in Dr Whewell's 'Writings and Correspondence' by Todhunter (2 vols., London, 1876). I believe the word philosophy has lost the specific meaning which it acquired in the Baconian school, as much through the influence of French science on the one side as through that of metaphysics on the other. The latter emanated from Scotland, and from Germany through Coleridge. It reinstated the word philosophy in its original sense.

by which science is translated into German, requires a qualification in order to cover approximately the same ground. These verbal differences point to differences of thought. Only since Continental ideas and influences have gained ground in this country has the word science gradually taken the place of that which used to be termed natural philosophy or simply philosophy. One reason why science forms such a prominent feature in the culture of this age is the fact that only within the last hundred years has scientific research approached the more intricate phenomena and the more hidden forces and conditions which make up and govern our everyday life. The great inventions of the sixteenth, seventeenth, and eighteenth centuries were made without special scientific knowledge, and frequently by persons who possessed skill rather than learning. They greatly influenced science and promoted knowledge, but they were brought about more by accident or by the practical requirements of the age than by the power of an unusual insight acquired by study.[1] But in the course of the last

2. Difference of English and Continental notions of science.

[1] Examples are plentiful. Not to speak of gunpowder and printing, which came earlier, we have later nearly all the great improvements connected with the manufacture of textiles, the fly-shuttle, the self-acting mule, the power-loom, the spinning-roller, invented by men of little or no scientific education. The same is the case with the older metallurgical processes, the refining of copper and the introduction of cast-iron. Watt was one of the first who brought a trained intellect to his mechanical work. The Royal Society was started with the distinct purpose of cultivating such knowledge as has "a tendency to use"; the Royal Institution still more so. It is, however, still doubtful, view-

hundred years the scientific investigation of *chemical* and *electric* phenomena has taught us to disentangle the intricate web of the elementary forces of nature, to lay bare the many interwoven threads, to break up the equilibrium of actual existence, and to bring within our power and under our control forces of undreamed-of magnitude.

3. Relation of science and practical life.

The great inventions of former ages were made in countries where practical life, industry, and commerce were most advanced; but the great inventions of the last fifty years in chemistry and electricity and the science of heat have been made in the scientific laboratory: the former were stimulated by practical wants; the latter themselves produced new practical requirements, and created new spheres of labour, industry, and commerce. Science and knowledge have in the course of this century overtaken the march of practical life in many directions.[1] A confused

ing the history of the learned societies as well as the rare cases in which highest scientific genius is allied with practical skill in the same person, whether the cultivation of research for its own sake should not preferably be kept distinct from its hasty application. This is the view held by many great thinkers abroad. In England the opposite view has frequently impeded the progress of pure science.

[1] A few examples may suffice. The discovery by Oersted and Ampère of Electromagnetism (1819, 1820) led at once to the idea of electrical telegraphy: the first telegraph over considerable distances was constructed by Gauss and Weber (see 'Wilhelm Weber,' Breslau, 1893, p. 26, &c.) The artificial preparation of an organic substance by Wöhler in 1828 led at once to many attempts at preparing expensive organic compounds—especially medical substances—by chemical synthesis. The occupation with this problem under A. W. Hofmann's instructions led Perkin in 1856 to the discovery of the first anilin colour (Mauvein, see 'Berichte der deutschen chemischen Gesellschaft,' No. 17, p. 3391). Leblanc's discovery how to make carbonate of soda from salt, for which a prize had been offered by the Paris Academy under Napoleon, led to the enormous development of the sulphuric acid industry in England and on the Continent. Liebig foretold in 1840 the recovery of sulphur from the waste of chemical works and the effect on the sulphur mines of Sicily, fifty years before this process was satisfactorily carried out (see Liebig's familiar 'Letters on Chemistry,' 1st ed., 1843, pp. 22, 31, &c.) But the greatest of all industries created in the laboratory was probably that of

4. Foreseen by Lord Bacon.

picture of this latest stage of culture lived in the prophetic but essentially unscientific mind of Lord Bacon. But he did not sufficiently allow for the amount of patient scientific toil that was needed, nor for the time which the preparation of the instruments of research would require, nor for the necessity of destroying existing superstition and accumulated errors. All that has since been done by Newton and the great Continental mathematicians in the former, and by Bayle and Voltaire in the latter sense, Bacon had hoped to achieve at once by the new philosophy of fruit and progress. Such expectations were inevitably doomed to disappointment, though posterity has made amends by all but universally referring to him as the pioneer of modern thought,—as the herald of a new era of human civilisation.[1]

making artificially the fertilising compounds required in common agriculture which followed on the publication of Liebig's famous work on 'Chemistry in its applications to Agriculture and Physiology' in 1840 (see Hofmann's Faraday Lecture of 1875, 'The Lifework of Liebig,' p. 15, &c.) Liebig also discovered and described in 1832 the properties of chloroform and chloral, fifteen years before Simpson introduced the first as an anæsthetic and twenty years before Oscar Liebreich discovered the physiological action of chloral (ibid., p. 101, &c.) Sir Lowthian Bell calculated, many years before the invention of the so-called basic process of making steel, the fertilising value of the phosphorus which was contained in the ironstone of Cleveland, and which then made it useless for the manufacture of high-class iron and steel. The great revolution in the theory of the steam-engine embodied in the work of Macquorn Rankine is to be traced back to the patient measurements by Joule of the mechanical equivalent of heat.

[1] A great controversy arose on this subject through the publication of Liebig's pamphlet in 1862, entitled, 'Francis Bacon von Verulam und die Methode der Naturforschung.' It was directed mostly against the exaggerated view taken by Macaulay in his celebrated essay. The fact is that Bacon, like Voltaire after him, was much more of an essayist and a man of the world than a patient labourer in any special field of research; he was more of a philosopher in a worldly sense (what the Germans call "ein Weltweiser") than a profound thinker. He misunderstood many of the great discoveries of his age, though he prophetically foresaw the great change in the spirit of inquiry. He did not appreciate

5. Defect in Bacon's philosophy.

Our age has in many ways inherited the spirit of Bacon's philosophy; but it would be a mistake to attribute its great scientific achievements to the exclusive working of this spirit. Bacon was neither a retired and patient nor an accurate thinker—the desire to apply and make his learning useful led him away from the "sapientum templa serena" into the forum of life: in his own experience, as well as in his writings, he anticipated many of the dangers which beset modern culture—the love of premature application, and the haste for practical results and achievements. Science, which in the hands of patient and diligent observers[1] had just been rescued from the sway of empty metaphysical and theological reasoning,

the enormous part which mathematics would play in the development of science. In this respect Descartes was a genius of much greater originality—his actual contributions to scientific progress, as well as those of Pascal, being far beyond those of Bacon; but they both retained the metaphysical habit of thought which has characterised many, if not all, among the greatest mathematicians. In modern culture the popularisation of novel views and ideas has become so important a factor that writers like Bacon and Voltaire, who combine the scientific and literary taste, are of the greatest importance in the diffusion of new ideas, though none of their works need be looked upon as great repositories of research and knowledge. Before Liebig wrote his pamphlet, a very impartial and temperate estimate of Bacon's philosophy and its relations to actual science was published by Robert Leslie Ellis in his introduction to the philosophical works of Lord Bacon (London, 1857). As the literature of the subject is so large, I cannot but recommend this essay as containing one of the best discussions of it.

[1] A very good and concise account of the achievements of these contemporaries and forerunners of Bacon—of Tycho (1546-1601), Kepler (1571-1630), Galileo (1564-1642), Gilbert (1540-1603), Harriot (1560-1621), Napier (1550-1617), Harvey (1578-1656)—is given by John Nichol in the second volume of his 'Francis Bacon, his Life and Philosophy' (Edinb., 1889), pp. 86, 254. In the same volume (p. 193) there is also a useful summary of Bacon's real claims to a place among physicists, of his ignorances (p. 196), and of the reception which his works met with in England and abroad (p. 233 to end). Not quite so readable, but more complete, is the little volume of Hans Heussler, 'F. Bacon und seine geschichtliche Stellung' (Breslau, 1889), with its flood of references—which exhaust the subject. See especially p. 160, &c., on Bacon's anticipations.

was in danger of falling a prey to hasty generalisation for the purpose of practical ends. Practical demands threatened then, as they frequently still do, to stifle or to force into premature growth the patient thought which had just begun to germinate in the new light and freedom of reason. The narrow view had indeed been widened, and the breadth of the land had been surveyed, but there was little inclination to deepen the view, or to do more than search on the surface. The spirit of Bacon's philosophy required a corrective. For a long time to come the hope of practical application had to be postponed; the thinker and student had to retire into solitude, and there to lay the more permanent foundations of the new research. This was done by Newton for all time. His reputation spread more slowly than that of the great High Chancellor; but it rests on a surer foundation, which baffles every attempt to shake it, and will outlast all coming changes of thought.

6. Corrected by Newton.

The beginnings of modern scientific thought are thus to be found in this country. Lord Bacon foretold prophetically the great change which the new philosophy was destined to work. Newton more patiently drew up the first simple rules and gave the first brilliant application. More than the unfinished and wearisome pages of Bacon's 'Novum Organum' does the 'Principia' deserve to be placed on a line with Aristotle and Euclid as a model work of scientific inquiry.

7. Bacon's and Newton's ideas taken up by French philosophers.

For a real recognition of the greatness of Newton, as well as for a partial realisation of Bacon's plans, we are, however, mainly indebted to the French philosophers of the second half of the eighteenth century. Bacon's plan of promoting

knowledge and research by the co-operation of many was more thoroughly realised in the old French Academy than in the Royal Society of London: his desire to unite all knowledge in a collective work underlies the great productions of Bayle, and still more those of the Encyclopædists. The many problems contained in Newton's 'Principia' were first treated singly by Clairault and Maupertuis; a general knowledge of his view of the universe was introduced into popular literature by Voltaire,[1] who made use of it as a powerful weapon wherewith to combat error and superstition, or, as he termed it, "pour écraser l'infâme"; but for a full announcement of its scientific value and its hidden resources we are indebted to Laplace, whose 'Mécanique céleste' was the first comprehensive elaboration of Newton's ideas, and whose 'Système du Monde' became the scientific gospel of a whole generation of Continental thinkers.

8. Bacon and Newton compared.

We may look upon Lord Bacon as one who inspects a large and newly discovered land,[2] laying plans for the

[1] I believe Voltaire was the author of the term *Newtonianisme.* The modesty and truly scientific spirit of Newton would not have allowed him to apply such a term to his work, and it is doubtful whether Voltaire did not extract from Newton's 'Philosophia Naturalis' a general philosophy which was not conceived in his spirit.

[2] Cowley in his Ode to the Royal Society:—

"Bacon at last, a mighty man, arose, . . .
And boldly undertook the injur'd pupil's cause.
. . . led us forth at last,
The barren wilderness he past;
Did on the very border stand
Of the blest promis'd land;
And, from the mountain's top of his exalted wit,
Saw it himself, and shew'd us it."

On this Mr Ellis remarks (Bacon's Works, vol. i. p. 63): "Bacon has been likened to the prophet who, from Mount Pisgah, surveyed the Promised Land, but left it for others to take possession of. Of this happy image, perhaps part of the felicity was not perceived by its author. For though Pisgah was a place of large prospect, yet still the Promised Land was a land of definite extent and known boundaries, and, moreover, it was certain that after no long time the chosen people would be in possession of it all. And this agrees with what Bacon promised to himself and to mankind from the instauration of the sciences. . . . In this respect, as in others, the hopes of Francis Bacon were not destined to be fulfilled. It is neither to the technical part of his method, nor to the details of his view of the nature and progress of science, that his great fame is justly owing. His merits are of another kind. They belong to the spirit rather than to the positive precepts of his philosophy."

development of its resources and the gathering of its riches. But the wealth lies deep down, and is only indicated by the first labours of the early pioneers. Newton, following these, unites their beginnings into a systematic exploration, and sinks the main shaft which reaches the lode of rich ore. He opens out the wealth of the mine and marks out the work for his followers. But many difficulties had to be overcome, much united effort and a vast organisation of labour were required, in order to develop to the full Newton's scheme, and to raise the great treasure which he had reached. This was not done until the end of the last century, when Laplace collected, 9. Laplace's work.
arranged, and condensed the work of French and English mathematicians and observers into a picture of the universe. A variety of circumstances had combined to make the French capital the place above all others where the means and materials for the development of the great work could be most easily procured. Let us glance for a moment at the different factors in operation during the eighteenth century which contributed to the great achievement.

Whilst Newton was labouring privately and almost unassisted[1] at the greatest scientific work produced in

[1] It has been stated that Newton, not knowing of Norwood's approximately correct determination of the length of a degree in 1635 (published in his 'Seaman's Practice' in 1637), but relying on the old figure of sixty miles for a degree of latitude (confirmed by Ed. Wright, Cambridge, 1610), was led away from the right supposition, which he entertained as far back as 1665, regarding the moon's orbit, and had to wait for Picard's figures (ascertained about 1669, published in France about 1672, and in the

modern times by any single mind,[1] the penetrating and far-seeing genius of Colbert had already recognised the important part which science would one day play in the government of the world, and had secured the approval of his royal master to the constitution of an Aca-

Philos. Transactions in 1675), by applying which he determined that "the moon appeared to be kept in her orbit purely by the power of gravity." See Brewster's 'Life of Newton,' vol. i. p. 290, &c.; Todhunter's 'History of the Theories of Attraction,' vol. i. p. 38, &c. This account is, however, now discredited (see *infra*, chap. iv.) For the part which Dr Hooke and Halley took in the discovery of the "reciprocal duplicate" ratio, see also Brewster, *loc. cit.*, vol. i. p. 291, &c. During the writing of the 'Principia' Newton carried on a useful correspondence with Flamsteed, who was then Astronomer-Royal. How this happy co-operation ceased ten years later can be read at length in Brewster (*loc. cit.*, vol. i. p. 312; vol. ii. p. 164, &c.) The greatest material assistance which Newton received was from Halley, who defrayed the expenses of publishing the 'Principia,' after the Royal Society, to which it was dedicated, had reversed its resolution to defray them (Brewster, vol. i. p. 305, &c.) Nevertheless Weld, in his 'History of the Royal Society,' says: "Fortunate indeed was it for science that such a body as the Royal Society existed, to whom Newton could make his scientific communications; otherwise it is very possible that the 'Principia' would never have seen the light." Though one must lament the differences between Flamsteed and Newton, which prevented the latter from bringing his investigations of the lunar and planetary theories to a close (Brewster, vol. i. p. 312), a word of deep gratitude is due to Flamsteed's own exertions in the cause of astronomy. After Charles II. had built the Observatory in order to have the places of the fixed stars "anew observed, examined, and corrected for the use of his seamen" (Flamsteed, History of his own Life), and after he had appointed Flamsteed Astronomer-Royal at a salary of £100 per annum, the Observatory, "hurriedly established, was left for a period of nearly fifteen years without a single instrument being furnished by the Government" (Weld, vol. i. p. 255). The instruments were mostly supplied by Flamsteed himself or lent by others, and besides, "the king had ordered that Flamsteed should instruct monthly two boys from Christ Church Hospital, which was a great annoyance to him, and interfered with his proper avocations" (Baily, 'Account of the Rev. J. Flamsteed'). "Any other man would probably have succumbed under the amount of drudgery appertaining to the office (earning his salary by labour *harder than thrashing*), if indeed, in the absence of encouragement, he would have continued in it at all, and particularly when the reward was so insignificant" (Weld, vol. i. p. 256).

[1] "And it may be justly said, that so many and so valuable Philosophical Truths, as are herein discovered and put past dispute, were never yet owing to the Capacity and Industry of any one Man" (Words of Halley, Philos. Transactions, vol. xvi., 1687).

demy, which was based upon the endowment of research, and which prompted the co-operation of its members in organised[1] scientific work. Whilst the Royal Society of London only received a charter, and existed by the entrance payments and contributions of its own members, augmented by private donations, the Paris Academy had, as far back as 1671, received the funds with which to commence its labours in connection with the survey of the kingdom and its extensive dependencies. It was these labours which led to the measurements of the length of the seconds pendulum, and of the variation of gravity in different latitudes; to the explanation of this variation by Huygens; to the controversy regarding the figure of the earth; to the direct measurements of the arcs of the meridian in Peru and Lapland; and, finally, to Clairault's celebrated work on this subject.[2] It was almost exclusively by these observations that the data were found with which to substantiate Newton's mathematical reasonings: in his own country that fruitful co-operation which

10. French Academy of Sciences.

[1] "Le roi assurait l'existence des Académiciens par des pensions et mettait libéralement à leur disposition un fonds destiné à pourvoir aux frais de leurs expériences et de leurs instruments" (Maury, 'Les Académies d'autrefois,' vol. i. p. 13). Organisation and co-operation are difficult to obtain in societies founded on private and voluntary contributions. In England they scarcely existed before the foundation of the British Association, with perhaps one illustrious exception pointed out by Struve ('Description de l'Observatoire de Pulkowa,' 4to, Pétersbourg, p. 5): "Il y a, dans l'histoire de l'observatoire de Greenwich, un point très remarquable, savoir que les astronomes ont travaillé sur un même plan, depuis l'origine de l'établissement jusqu'à l'époque actuelle." Organisation and co-operation were the order in the Paris Academy from the beginning. "On y travaillait de concert"; and, "Dès les premiers mois de 1667, Perrault proposa un plan de travail pour la physique, c'est à dire pour l'ensemble de l'histoire naturelle" (Maury, *loc. cit.*, p. 15).

[2] A full account of these is given in Todhunter ('Hist. of Theories of Attraction, &c.,' vol. i.) Clairault's book was published in 1743, and had the title, 'Théorie de la Figure de la Terre, tirée des Principes de l'Hydrostatique, par Clairault.'

can only be secured by an academic organisation and by endowment of research was wanting. No one since the time of Bacon had been more impressed with this necessary condition of modern progress than Newton's great rival, Leibniz,[1] much of whose time was spent in promoting academies all over Europe—in Berlin, St Petersburg, Dresden, and Vienna—and who had himself been early attracted to Paris and London by the scientific fame of their learned societies, though he significantly pointed out the want of activity and efficiency in the early history of the Royal Society.

11. Continental methods in mathematics.

There was, moreover, another and independent line of scientific thought which had centred in France, the development of which came greatly to the aid of the students of Newton's work. This was the purely mathematical elaboration of the various infinitesimal methods of the French and English mathematicians, by which they were all brought together, simplified, and united into a calculus with strict rules, a practical notation, and an easy algorithm. Newton himself had for the purposes of his great work invented a new and powerful

[1] A collection of Leibniz's writings on this subject will be found in the 7th volume of M. Foucher de Careil's edition of Leibniz's Works, Paris, 1875. Of the projects of Leibniz, only the Academy of Berlin came into existence during his lifetime (1700 and 1701); the others were discussed at great length with the Elector of Saxony, with the Emperor, and with Peter the Great. The Academy of St Petersburg was founded in 1724, eight years after the death of Leibniz. The Academy of Vienna did not come into life till 1846, and in the same year that of Saxony was founded, which has its seat at Leipsic. Leibniz had the largest views on academic life and work: they were to embrace the historical and philosophical studies as well as the purely scientific, and were to stand in relation with the higher and lower educational institutions. His ideas are best realised at Berlin. See Jacob Grimm's interesting discourse, entitled 'Ueber Schule Universität Akademie' (Kleine Schriften, vol. i. p. 211, &c.)

instrument, afterwards called "the method of fluxions"; but he had not made it generally known before the invention of Leibniz was published.[1] This, though much later in time, had been perfected and applied by his friends and followers in a most extensive manner, and had, in fact, become the recognised mathematical language of the Continent. No learned body did more than the Paris Academicians to perfect (with purely scientific

[1] Leibniz seems to have been in possession of his method as early as 1675, and communicated it to Collins in 1677. It was, however, not published before 1684 in the 'Acta Eruditorum,' and then probably only on account of some writings of Tschirnhausen trenching on the same subject. Newton seems to have been in possession of his methods as early as 1665, fully ten years before Leibniz made use of his. Immediately after the publication of Leibniz's paper in 1684, the differential calculus was taken up by the Continental mathematicians, especially by James Bernoulli (1654-1705) and John Bernoulli (1667-1748), and the Marquis de l'Hopital, who published the first treatise on the new calculus in 1696. Newton did not publish any account of his method, though he must have used it extensively in arriving at the results contained in the 'Principia.' Different views have been expressed on the reasons which induced Newton to withhold from publication his new methods, and the question to what extent Leibniz owed the first suggestions of his method to Newton remains also undecided. Those who take an interest in the personal question should refer to the original documents, the 'Commercium Epistolicum,' published by the Royal Society in 1715; the pamphlet of Gerhardt, 'Die Erfindung der Differentialrechnung' (Halle, 1848). An extreme view, unfavourable to Leibniz's originality, is taken by Sloman, 'Leibnitzens Anspruch auf die Erfindung der Differentialrechnung' (Leipzig, 1857); but it has not been generally adopted by those who have examined into the subject. As to the superiority of the Continental notation for practical purposes, this seems to have been generally admitted at the beginning of this century, when it was introduced into English mathematical works. In the school of W. R. Hamilton of Dublin the notation used by Newton acquired a peculiar importance, and it is still occasionally used in some important works like Tait and Steele's 'Dynamics of a Particle,' and Thomson and Tait's 'Natural Philosophy.' See on this Tait's article on Hamilton in the 'North British Review' (Sept. 1866). The importance of the labours of the Continental school, headed by Leibniz, for the diffusion of the new methods, is well described by Remont de Montmort in a letter to Brook Taylor, dated 18th December 1718, and given in the appendix to Brewster's 'Life of Newton' (vol. ii. p. 511, &c.) Those who take more interest in the fate of ideas and the progress of thought than in personal matters will do well to read this letter.

interest) this new calculus, which in the course of the eighteenth century had in the hands of Lagrange been adapted to all the purposes and problems contained or suggested in Newton's 'Principia.'

12. Modern analytical methods.

This leads me to a third and yet more important element of scientific thought, which was peculiar to the Continental, and especially to the French mathematicians, counting among them Leibniz, who, though a German, was wholly trained in the French school. This factor is the establishment of pure mathematics on an independent foundation, and the cultivation of research into the abstract relations of quantity, without reference either to geometrical or mechanical problems and applications. It is the modern analytical spirit introduced by the great French algebraists of the seventeenth century, which looks upon geometry, mechanics, and astronomy merely as "questions d'analyse," and makes their solutions depend upon the perfecting of an abstract calculus rather than on the study of these individual problems themselves. Opposed to this spirit of analysis, which in general seeks the solution of any given question by looking upon it as a special case of a wider and more abstract problem, is the method known to the ancients, which never loses sight of the actual application, be it a figure in geometry or a special arrangement of physical forces, and is more interested in the peculiarities of the individual case than in the abstract formula of which it may be considered an application. This opposite view regards the calculus and mathematics in general merely as an instrument, the value of which lies solely in its application to real physical problems. It is usually

termed the synthetical method, and has in modern times survived principally in England, where inductive reasoning, based upon observation of detail, has since the age of Lord Bacon been most successfully cultivated.[1] These different ways of approaching the same subject will frequently engage my attention in the course of this survey: the greatest mathematicians of modern times have recognised the importance of both aspects, and the enormous progress of the science itself has depended, no doubt, on an alternating employment of them. Leibniz clearly foresaw this when, in his correspondence with Huygens and others, he urged the necessity of not abandoning the purely geometrical view, or entirely sacrificing the older for the modern methods.[2] There can, however, be no doubt that

13. Older synthetical method.

[1] See on this point the opinion of an authority, Hermann Hankel, in his highly interesting and suggestive lecture, 'Die Entwickelung der Mathematik in den letzten Jahrhunderten' (Tübingen, 1869, republished by P. du Bois-Reymond, 1884). Speaking of the age of Leibniz he says: "Though on the Continent mathematicians were not so conservative as in England, where a purely geometrical exposition was considered to be the only one worthy of mathematics, yet the whole spirit of that age was directed to the solution of problems in geometrical clothing, and the result of the calculus had mostly to be retranslated into geometrical forms. It is the inestimable merit of the great mathematician of Basel, Leonhard Euler, to have freed the analytical calculus from all geometrical fetters, and thus to have established analysis as an independent science. Analysis places at its entrance the conception of a function, in order to express the mutual dependence of two variable quantities. . . . The abstract theory of functions is the higher analysis. . . . The conception of a function has been slowly and hesitatingly evolved out of special and subordinate conceptions. It was Euler who first established it, making it the foundation of the entire analysis, and hereby he inaugurated a new period in mathematics" (p. 12, &c.)

[2] To Huygens, 16th September 1679: "Je ne suis pas encor content de l'Algèbre, en ce qu'elle ne donne ny les plus courtes voyes, ny les plus belles constructions de Géométrie. . . . Je croy qu'il nous faut encor une autre analyse proprement géométrique ou linéaire, qui nous exprime directement *situm*, comme l'Algèbre exprime *magnitudinem*. Et je croy d'en avoir le moyen, et qu'on pourroit représenter des figures et mesures des machines et mouvements en caractères, comme l'Algèbre représente les nombres ou grandeurs" (Leibniz, Mathem. Werke, ed. Gerhardt, vol. ii. p. 19).

the great success which attended Laplace's work, the elaboration of a system of the universe out of the principles of Newton, was largely due to the perfection which the analytical methods had gained in the hands of his predecessors, and to the skill with which he himself reduced the several problems to purely analytical questions.

But however much exact methods, learned societies, and regal endowments may do to promote the growth of the scientific spirit, experience has shown that popular favour and interest furnish a still more effective stimulus. Even the most abstract reasonings of the mathematician require to be brought into some connection with the general concerns of mankind, before they can attract talent from outside, or enter into that healthy action and reaction which are the soul of all mental progress. In this respect, also, France during the second half of the eighteenth century was far in advance of other countries.

14. Influence of science on French literature.

No other literature of that age can be compared with that of France, when we look at the influence or the expression which modern scientific views and interests had already attained in it; and no other country could at the end of the eighteenth century boast of such splendid means of scientific instruction as then existed in Paris. In two important departments—the popularisation and the teaching of science—France for a long period led the way.[1] A general interest was thus created in the proceedings and debates of the Academy, and the discoveries of its illustrious members found their way into the lectures and text-books of the professors. Whatever eminence German science may have gained in this century, from a purely literary point of view, through the works of A. von Humboldt, or English science through those of Darwin, the history of both literatures during the eighteenth century can be written almost without any reference to science at all—so small was the direct influence of such giants as Newton and Leibniz on the popular mind. But who could exclude from a history of the elegant literature of France the names of Voltaire, of Buffon, of D'Alembert, or of Condorcet? These form a connecting link between science and general literature.[1] A study either of English or

To Bodenhausen (about 1690): "I am of opinion that in the problems of ordinary Geometry the *methodus Veterum* has certain advantages over *Analysin Algebraicam*, and I think I have remarked to you that there remains an *Analysis geometricæ propria, toto cælo ab Algebra diversa et in multis longe Algebra compendiosior utiliorque*" (ibid., vol. vii. p. 359). "It is certain that algebra, by reducing everything *a situ ad solam magnitudinem*, hereby very frequently complicates things very much" (p. 362).

[1] Perhaps it would be more correct to say that science was fashionable than that it was popular in the eighteenth century in France. But it became popular through the influence of the great schools of Paris. Before becoming popular with the masses it became so in cultivated and literary circles. The result has been that science in France alone has attained to a perfect form of expression. Whereas in other countries the great models of original research and thought were written in the severe style handed down by the ancients (Newton's 'Principia' and Gauss's 'Disquisitiones Arithmeticæ'), the great work of Lagrange (the 'Mécanique analytique') is a model of literary style in the modern sense. Science in our age has become popular through its applications. It is the utilitarian spirit that has popularised science in Germany and England. In France alone science, before coming under the influence of the utilitarian, came under that of the literary spirit. It was the influence of the academies that brought this about. See Maury, 'Les Académies d'autrefois,' vol. i. p. 178, &c. More than with Richelieu, the interest in science nowadays is unfortunately only too often purely "metallic" (quoted from Lord Chesterfield's Letters). See also on the literary as compared with the modern practical character of science, Maury, ibid., p. 161.

[1] "On érigeait même en principe la nécessité pour un philosophe de ne rester étranger à aucune science. 'L'esprit philosophique fait tant de progrès en France depuis quarante ans,' écrivait Voltaire à madame Du Châtelet, en lui dédiant sa tragédie d'Alzire, 'que si Boileau vivait encore, lui qui osait se moquer d'une femme de condition, parce qu'elle voyait en secret Roberval et Sauveur, il serait obligé de respecter et d'imiter celles qui profitent publiquement des lumières des Maupertuis, des Réaumur, des

of German eighteenth-century literature does not introduce one to the great controversies of science, but a study of Voltaire leads one into the midst of the profound problems of the Newtonian and Cartesian philosophy, the disputes on the correct measure of force.[1] Buffon's influence, also, by spreading a taste for the study of nature and by making objects of natural history attractive, was probably much more important than his actual contributions to the natural sciences themselves.[2]

15. Absence of this influence in England and Germany.

16. Schools of science in Paris.

For the growth and diffusion of the scientific spirit itself, the great schools in Paris were even of greater value than the popular writings of Voltaire and Buffon. Most of the Academicians were trained in these schools,

Mairan, des Du Fay et des Clairault; de tous ces véritables savants qui n'ont pour objet qu'une science utile, et qui, en la rendant agréable, la rendent insensiblement nécessaire à notre nation. Nous sommes au temps, j'ose le dire, où il faut qu'un poëte soit philosophe et où une femme peut l'être hardiment.' En parlant ainsi, Voltaire ne faisait qu'exprimer l'opinion de son siècle, et ambitieux lui-même de réunir le titre de géomètre à celui de poëte et d'historien, il s'était fait expliquer par madame Du Châtelet la physique de Newton" (Maury, 'Les Acad. d'autrefois,' vol. i. p. 156).

[1] See Maury, vol. i. p. 157, &c.; and Du Bois-Reymond, "Voltaire als Naturforscher" in 'Gesammelte Reden,' vol. i. p. 1.

[2] "Sans l'éloquence de Buffon, la zoologie serait demeurée encore longtemps le privilége d'un petit nombre; elle eut laissé indifférents ceux que la nature émeut moins que le charme de la parole. La vieille éducation classique avait le tort de nous laisser très-ignorants des choses du monde créé. Buffon communiqua aux sciences le charme des lettres. La curiosité s'éveilla, et en 1760, Valmont de Bomare put ouvrir à Paris le premier cours d'histoire naturelle; il fut assidûment suivi" (Maury, vol. i. p. 283). A. von Humboldt had a similar influence in Berlin seventy years later. See Du Bois-Reymond, *loc. cit.*, vol. i. p. 510. Guardia, 'Histoire de la Médecine' (Paris, 1884), says of Buffon, "Fontenelle avait rendu la science aimable et accessible. Buffon l'associa à la philosophie et aux lettres et l'introduisit définitivement dans la société" (p. 384). What a contrast, when we read in the 'Life of Sir W. R. Hamilton' (by R. P. Graves, vol. ii. p. 196) that Dr Buckland's communication at the Bristol meeting of the British Association (1836) "was apparently the first occasion of bringing before the public mind in England the geological doctrine of the great antiquity of the earth; for out of the expressly scientific circles, very little—you [viz., Count Adare] are aware—is known of what scientific men are about"!

and many of them taught there for many years.[1] It was with a true insight into the higher intellectual needs of the nation that the successive Governments of the Revo-

[1] Before the age of the Revolution, which did so much to promote higher scientific education, Paris possessed already many great schools. First in importance was the Collège de France, founded in 1530 by Francis I. Gassendi and Roberval taught there in the seventeenth century, and about the middle of the eighteenth century science began to be more extensively represented, Lalande and Daubenton occupying chairs. The Collège et École de Chirurgie was an ancient establishment. There was the Jardin des Plantes, with Buffon, Lemonnier, Daubenton, and Fourcroy; the École royale des Mines, founded in 1783, where Duhamel taught metallurgy; the École des Ponts et Chaussées, founded by Turgot in 1775. Daubenton, Fourcroy, and Vicq d'Azyr taught in the École vétérinaire d'Alfort, founded in 1766. Besides the Académie des Sciences, the Académie royale de Chirurgie, founded by Lapeyronie under Louis XV. in 1731, had a great influence on the development of anatomy and surgery during the eighteenth century. Tenon and Petit, as well as Quesnay the economist, were amongst its members, and it kept up a lively intercourse with anatomists all over Europe. The Paris academies had also their representatives and connections in the provinces. Independent academies of science were affiliated with the Académie des Sciences—1716 at Bordeaux, 1706 at Montpellier, 1746 at Toulouse, 1766 at Béziers. Before having received their *lettres patentes*, which gave their members certain privileges, most of these academies had existed as independent societies. Other provincial academies, such as Arles (1668), Nîmes (1684), Soissons (1674), Marseilles (1726), were affiliated with the Académie française. Others, such as Caen (1705), Lyons (1724), Dijon (1740), Rouen (1744), Amiens and Nancy (1750), Besançon (1757), Metz (1760), Clermont (1780), Orléans (1786), were not specially affiliated. These dates show how very much earlier a literary and scientific organisation existed in France than in other countries. The Protestant universities in Germany formed an organisation of a different kind, with which I shall deal later on. The academic system, so early developed in France, was of great use to the culture of the sciences. French science is usually considered to be almost entirely located in Paris. M. Bouillier ('L'Institut et les Académies de Province,' Paris, 1879) has drawn attention to the great services of this network of academies. Many of the most eminent writers belonged to these provincial centres, and worked for them even after becoming members of the more celebrated academies. Montesquieu is connected with Bordeaux, Cassini and many eminent doctors with Montpellier, Dijon has the honour of bringing out Rousseau, and Toulouse gave prizes to Bossut and Clairault. Robespierre's name is connected with the Academy of Arras, Marat discourses at Rouen and Lyons on electricity and optics, and Danton and Bonaparte compete for the *prix Raynal* at Lyons. "Mais," says M. Bouillier, "ce qui nous semble le plus digne de remarque et d'éloge, ce sont les écoles gratuites de dessin, les cours gratuits de physique, de chimie,

lution, in the midst of the more pressing problems of national safety and welfare, betook themselves to the solution of the great problem of national education and the instruction of all grades of society. "The Convention," says the historian of public instruction,[1] "affords us the strange and grand spectacle of an assembly, which on the one side seems to have no other mission than to crush in the name of public welfare everything that stands in the way of the triumph of the Republican State, and which can see no other way of attaining this than the most terrible and cruel of tyrannies; and which on the other side devotes itself, with a stoical calm and serenity, forming a surprising contrast to its acts, to the study, the examination, and the discussion of all the problems involved in public instruction, of all the measures conducive to the progress of science. It had the glory of creating institutions, some of which were carried away by the blast of the Revolution, but among which the most important still exist for the great honour of France, and bear proof of the loftiness of her ideas."[2]

17. Promoted by Governments of Revolution.

d'histoire naturelle, d'anatomie, d'antiquités, fondés par un certain nombre d'académies et, entre autres, par Dijon, par Rouen, par Bordeaux, par Toulouse, par Montpellier, et dont les professeurs étaient des membres, non rétribués de ces académies. . . . A combien de jeunes talents les académies provinciales n'ont-elles pas donné l'essor, par leurs récompenses solennelles et leurs encouragements? Combien de leurs lauréats ne sont pas devenus des hommes célèbres?" (p. 81, &c.) Besides Bouillier, consult on these matters the several articles, "Académie," "Collége," "École," in the 'Grande Encyclopédie.'

[1] C. Hippeau, 'L'Instruction publique en France pendant la Révolution,' 1e série, préface, p. xix.

[2] It appears nowadays a kind of paradox that, as M. Hippeau remarks, in the very year 1793, when "the Convention was labouring with a feverish ardour at the creation of schools of all degrees," this same Convention, on a report of the Committee of Public Instruction, voted on the 8th of August the suppression of all the academies of Paris and the provinces. On this M. Bouillier ('L'Institut et les Académies,' p. 95) remarks: "Bientôt il est vrai, les académies devaient renaître après la chute de la

It was of immense importance to the cause of science that in many of the discussions of that assembly a marked preference was shown for the scientific side of instruction. In this matter, as in many others, the successful constructive efforts of the Revolutionary Governments came from the side of those brought up in the

Montagne et du Comité de salut public. Nous n'ignorons pas que c'est encore la Convention qui, prise d'un tardif remords, la veille seulement du jour où elle devait faire place à un autre gouvernement moins despotique et moins cruel, décréta l'organisation de l'Institut. Mais la Convention du 3 brumaire an iv. n'était plus celle de 1793; c'était en réalité une autre Convention, épurée, décimée, renouvelée, animée d'un tout autre esprit," &c., &c. The idea of a national Institute for the advancement of letters, science, and arts was a very early one (see 'Rapport de Talleyrand Périgord,' September 1791, Hippeau, p. 102). The explanation how the same Government which was labouring at the problem of a national instruction, crowned by the higher teaching and research of an Institute, could begin by closing the existing academies and universities, lies in this, that the aim was to make education general and learning popular, not merely fashionable, as it had been. See, for instance, what Ducos said on the 18th December 1792: "Les mœurs d'un peuple corrompu ne se régénèrent point par de légers adoucissements, mais par de vigoureuses et brusques institutions. Il faut opter ouvertement entre l'éducation domestique et la liberté; car citoyens, tant que par une instruction commune vous n'aurez pas rapproché le pauvre du riche, le faible du puissant; tant que, pour me servir des expressions de Plutarque, vous n'aurez pas acheminé à une même trace, et moulé sur une même forme de vertu tous les enfants de la patrie, c'est en vain que vos lois proclameront la sainte égalité, la République sera toujours divisée en deux classes: les *citoyens* et les *messieurs*" (Hippeau, 2e série, p. 21). It was because the academies and colleges supported "les messieurs" that they were suppressed. In the end education must always begin from above, and before the people can be taught you must form their teachers. See Lakanal's Report on the Écoles normales, Hippeau, vol. i. p. 408. The academies and colleges of the eighteenth century were closed in order to make room for that uniform system of public instruction described by Talleyrand and Condorcet, but not without a frequently expressed admiration for the work which they had done. See the defence of the academies by Condorcet (Hippeau, *loc. cit.*, vol. i. p. 272), and the tribute to the "Collége de France," by Gilbert Romme (ibid., vol. i. p. 308). The arguments for radical change are summed up by that speaker as follows: "L'existence de ces corps privilégiés blesse tous nos principes républicains, attaque l'égalité et la liberté de penser et nuit aux progrès des arts. Mais si leur organisation est vicieuse, les éléments en sont bons, et nous serviront utilement dans l'organisation nouvelle de l'instruction publique que vous allez décréter" (p. 309).

school of Voltaire and the Encyclopædists, whilst the work of destruction had been performed by the followers of Rousseau. No one has expressed himself on the value of scientific study and knowledge in a clearer or more far-seeing manner than Condorcet. In his 'Report and Project of a Decree on the General Organisation of Public Instruction,' which he presented to the National Assembly in the name of the Committee of Public Instruction, he says:[1] "Many motives have brought about the kind of preference which is accorded to the mathematical and physical sciences. Firstly, for men who do not devote themselves to long meditations, who do not fathom any kind of knowledge—even the elementary study of these sciences is the surest means of developing their intellectual faculties, of teaching them to reason rightly and to analyse their ideas.[2] . . . It is because in the natural sciences the ideas are more simple, more rigorously circumscribed, it is because their language is more perfect, &c., &c. . . . These sciences offer a remedy for prejudice, for smallness of mind—a remedy, if not more certain, at least more universal, than philosophy itself.[3] . . . Those

18. Condorcet.

[1] It was presented on the 20th and 21st April 1792. See Hippeau, 1e série, pp. 185-288. It was printed by order of the Convention, Paris, Imprimerie nationale, 1793.

[2] Ibid., p. 203.

[3] Ibid., p. 204. It is interesting to see how in all these reports the exact sciences are placed in the foreground. See, for instance, what Gilbert Romme says of the teaching of the proposed *instituts:* "Les sciences mathématiques et physiques, morales et politiques, l'agriculture et les arts mécaniques, la littérature et les beaux-arts, composeront l'enseignement des instituts où l'on pourra suivre, dans leurs éléments, l'échelle entière des connaissances humaines" (vol. i. p. 322). "Les lycées seront l'école des gens instruits; ils embrasseront les sciences, les arts et les lettres dans toute leur étendue." One is forcibly reminded that the most perfect realisation of this arrangement of studies is to be found a century later in the provincial science colleges of this country. The preference, however, is now given to science mainly for ultilitarian reasons: the difference is shown by the importance then attached to mathematics as a training of the intellect in precise thinking; nowadays it is the mechanical side that is favoured, and this is only too often destructive of the truly scientific and exact spirit.

who follow their course, see the coming of an epoch when the practical usefulness of their application will reach greater dimensions than were ever hoped for, when the progress of the physical sciences must produce a fortunate revolution in the arts. And lastly, we have yielded to the general tendency of men's minds, which in Europe seem to incline towards these sciences with an ever-increasing ardour. . . . Literature has its limits, the sciences of observation and calculation have none. Below a certain degree of talent, the taste for literary occupations produces either ridiculous pride or a mean jealousy towards such talents as one cannot attain. In the sciences, on the contrary, it is not with the opinion of men but with nature that we have to engage in a contest, the triumph of which is nearly always certain, where every victory predicts a new one."[1]

"It is," says Lakanal, in his report on the "Écoles centrales," 16th December 1794, "of great importance for the nation to assure itself that the mathematical sciences are cultivated and deepened, for they give the habit of accuracy: without them astronomy and navigation have no guide; architecture, both civil and naval, has no rule; the sciences of artillery and of fortification have no foundation."[2] Gradually, under the pressure of exter-

19. Lakanal.

[1] Hippeau, *loc. cit.*, p. 258. Cf. p. 261: "Hâtons-nous . . . de porter dans les sciences morales la philosophie et la méthode des sciences physiques" (Condorcet).

[2] Hippeau, vol. i. p. 432. It is interesting to see how the study and teaching of the sciences in course of the second half of the last century in France undergo a development. The literary interest predominates in Fontenelle. Buffon and Voltaire add to it the philosophical and

nal events, the exigencies of war and the defence of the country gain the upper hand, and a central establishment is founded to cultivate and teach the sciences and arts, "upon which depend the defence of the Republic by land and sea."[1] Few of the higher and philanthropic aims of the great educational leaders of the early years of the Revolution—of Mirabeau, of Talleyrand, of Condorcet—were realised; little was done for primary education; but science can boast of having been worthily represented and supported in the two great schools which still bear their original designation, and which can show a record of celebrated names and magnificent work superior probably to that of any other similar institution in Europe.

20. École normale. École polytechnique.

They are the "École normale supérieure" and the "École centrale des Travaux publics," better known by the title "École polytechnique."[2] The founders of this

philanthropic, the Encyclopædists and Condorcet the educational; the events of the Revolution and the discussions in the Assemblies bring out more and more the instructive, the utilitarian, and the economical aspects. The only creations which resulted were those in which the latter aims were predominant.

[1] Lakanal, see Hippeau, vol. i. p. 447.

[2] To these two great schools must be added as a third the "Muséum d'Histoire naturelle," "le plus magnifique établissement que les sciences aient possédé" (Cuvier, "Éloge de Fourcroy," part ii. of the 'Éloges historiques,' p. 44, Strasbourg, 1819). The foundation of the "École centrale des Travaux publics" was proposed by Barère on the 11th March 1794, and definitely organised on the report of Fourcroy (Hippeau, vol. i. p. 446) by a decree of 7th vendémiaire, an iv. (name changed to École polytechnique, 15th fructidor). The opening of the courses was announced for the 10th frimaire following (Hippeau, vol. ii. pp. 139, 174, 175). The foundation of the "Écoles normales" was proposed by Barère (13th prairial, an ii.), and decreed on a report of Lakanal (Hippeau, vol. i. p. 423) on the 9th brumaire, an iii. (30th October 1794) (ibid., vol. ii. p. 179). The courses opened on the 1st pluviose. The work of the school was distributed as follows: Mathematics, Lagrange and Laplace; physics, Haüy; descriptive geometry, Monge; natural history, Daubenton; chemistry, Berthollet; agriculture, Thouin; geography, Buache and Mentelle; history, Volney; morals, Bernardin de St Pierre. (Hippeau, vol. ii. p. 180; where also will be found extracts from the 'Moniteur' of the 9th pluviose on the opening addresses.) The oldest pupil was Bougainville, the great traveller. The École polytechnique received an allocation of £12,000, and had 400 pupils to start with. On the 20th frimaire, an iii., the Convention, on a report of Thibaudeau, voted the necessary expenses for the enlargement of the Muséum d'Histoire naturelle (Hippeau, vol. ii. p. 196),—*viz.*, nearly £8000 for expenses, and £200 for each of the professors. The Museum had been originally destined for the culture of medicinal plants. Tournefort had given a great impetus to botanical, and Buffon, with Daubenton, to zoological studies. The Convention added several to the courses regularly held there on natural history, botany, mineralogy, and general chemistry. "Ces cours," says Thibaudeau, "fournissent 500 leçons par an, offrent l'ensemble le plus vaste et le plus complet d'enseignement sur toutes les branches d'histoire naturelle dont le plus grand nombre manquaient totalement à la France et dont quelques-unes manquent encore à l'Europe, l'application immédiate de toutes les sciences naturelles au commerce et aux arts."

Of other scientific and teaching institutions I must mention the "Bureau des Longitudes." This was organised by the Convention on a discourse by Grégoire, 7th messidor, an iii. (24th June 1795), in which he refers to the British Board of Longitude and the superiority of the British navy (Hippeau, vol. ii. p. 219). The appointments to this bureau were the *géomètres* Lagrange and Laplace, the *astronomes* Lalande, Cassini, Méchain, Delambre, one of whom had to deliver a course of astronomy, the travellers Borda, Bougainville, the *géographe* Buache, and the artist Carocher. It had charge of the observatory, which had already been reorganised by a decree promoted by Lakanal on the 31st August 1793 (Hippeau, vol. ii. p. 76), and published in the 'Connaissance des Temps.' There were, besides, several military schools and the medical schools, not to mention other foundations less connected with our subject but equally important, such as the School of Oriental Languages, established in the Bibliothèque nationale (germinal, an iii., Hippeau, vol. ii. p. 215); the Écoles de Santé, established 14th frimaire, an iii., on a report of Fourcroy, in Paris, Strasbourg, and Montpellier (Hippeau, vol. ii. p. 194).

magnificent institution recognised "that, in spite of the diversity of applications, mathematics and physics are the indispensable basis of the studies in view."[1] Though the first period of the life of the École normale only counted four months,[2] we are indebted to it for the

[1] Ibid., vol. i. p. 450.

[2] The École normale was closed on the 30th floréal, an iii., on a decree of the Convention dated the 7th of that month. Danton explained that the school had not taken the line which the Convention had marked out—the courses in general having offered a direct teaching of the sciences rather than an exposition of the methods which are to be adopted in teaching (Hippeau, vol. ii. p. 215). It also seems that the eminent teachers of this institution had few pupils sufficiently prepared to follow them. The École normale was reopened in the year 1808 under the Empire, by the same decree of 17th March which organised the University of France.

21. Monge's 'Descriptive Geometry.' foundation of a new branch of science—the 'Descriptive Geometry' of Monge, which was given to the world through shorthand notes[1] from his lectures delivered in that institution. They form the beginning of the new science, since developed by Poncelet, Steiner, and others, and known under the name of "projective geometry."[2]

Next to mathematics with its analytical and graphical application to physics and the arts, the subject most cultivated in these higher educational establishments of Paris at the end of the last century was the new science of
22. Science of Chemistry. chemistry. With some justice this science has been termed a French science,[3] not so much because even at that time

[1] See the account of the origin of this branch of mathematics in Brisson's edition of the 'Géométrie descriptive,' Paris, 1847. In the programme prefixed to the treatise the three aspects of the new school—the national, the practical, and the educational—are well set forth: "Pour tirer la nation française de la dépendance où elle a été jusqu'à présent de l'industrie étrangère, il faut premièrement diriger l'éducation nationale vers la connaissance des objets qui exigent de l'exactitude. . . . Il faut, en second lieu, rendre populaire la connaissance d'un grand nombre de phénomènes naturels. . . . La géométrie descriptive est un moyen de rechercher la vérité; elle offre des exemples perpétuels du passage du connu à l'inconnu; et parcequ'elle est toujours appliquée à des objets susceptibles de la plus grande évidence, il est nécessaire de la faire entrer dans le plan d'une éducation nationale." Monge generalised and placed on a scientific basis the methods used previously by carpenters and stonecutters, and partially dealt with geometrically by Courcier, Derand, Mathurin, Jousse, and Frezier. See Montucla, 'Histoire des Mathématiques,' vol. iii. p. 15.

[2] Monge taught also at the École polytechnique from the beginning. See the remarks of Chasles ('Rapport sur les Progrès de la Géométrie,' Paris, 1870, p. 2): "L'enseignement théorique et profond qui a été la base de la première et judicieuse organisation de ce grand établissement était éminemment favorable aux progrès de la science, en même temps qu'il préparait sérieusement les élèves à l'entrée dans les écoles d'application." The author then refers with regret to the less scientific tone which had crept into the studies of that great school in the course of this century. See also p. 379.

[3] A. Wurtz ('Histoire des Doctrines chimiques,' Paris, 1868, p. 1): "La chimie est une science française; elle fut constituée par Lavoisier." Cf. Dumas ('Leçons sur la Philosophie chimique,' Paris, 1837, p. 137). Buckle ('History of Civilisation,' &c., 3 vols., vol. ii. p. 366, London, 1866) says: "That we owe to France the existence of chemistry as a science will be admitted by every one who uses the word science in the sense

chemistry was not indebted to illustrious foreigners[1] for some of its most important discoveries, as because the modern scientific spirit of accurate measurement first took hold of chemical phenomena on a large scale in the many important investigations which bear the name of Lavoisier and his followers, through whom the great reform of modern chemical knowledge and research was permanently established. It has been significantly pointed out[2] that it was the union of mathematical with empirical knowledge which, through men like Laplace, Meusnier, Monge, first

in which alone it ought to be understood, &c. . . . Until Lavoisier entered the field there were no generalisations wide enough to entitle chemistry to be called a science." The correctness of this view is fully and impartially examined by Hermann Kopp ('Die Entwickelung der Chemie in der neueren Zeit,' München, 1873, p. 89, &c.) He fully upholds the claims of Lavoisier to be called the father of modern chemistry (p. 145). See also what Liebig says.

[1] These were mainly, Black (discovered carbonic acid, called fixed air, in 1754), Cavendish (discovered hydrogen or inflammable air in 1767), and Priestley, who between 1771 and 1774 discovered oxygen (dephlogisticated air), nitrogen (phlogisticated air), and several of its compounds, among them ammonia (alkaline air). Of Priestley it is said by Cuvier that he may well be considered as one of the fathers of modern chemistry, "mais c'est un père qui ne voulut jamais reconnaître sa fille" ('Éloges,' vol. i. p. 208). Elsewhere ('Rapport historique sur les Progrès des Sciences naturelles,' Paris, 1810, p. 90) Cuvier dates the revolution in chemistry from the introduction of the mathematical spirit: "Il en est une cause encore plus essentielle à laquelle même on doit à proprement parler, et cette théorie nouvelle, et les découvertes qui l'ont fait naître. . . . C'est l'esprit mathématique qui s'est introduit dans la science et la rigoureuse précision qu'on a portée dans l'examen de toutes ses opérations. . . . C'est dans le Traité élémentaire de Lavoisier que l'Europe vit pour la première fois avec étonnement le système entier de la nouvelle chimie," &c.

[2] Kopp, *loc. cit.*, p. 202: "Indeed, if we look at those who first worked together with Lavoisier or in his spirit, we shall find such as had devoted themselves principally to mathematics or mathematical physics, men like Laplace, Meusnier, Monge. Among chemists Lavoisier stood for a long time almost alone in his opinions." This view is also taken by Cuvier ('Rapport,' p. 91): "Les nouveaux chimistes français . . . ont eu à se louer du concours de quelques-uns de nos géomètres les plus distingués," &c.; and he attributes the next great step in chemical science to a similar introduction of a "rigueur toute mathématique" ('Rapport sur la Chimie lu à la Séance des 4 Acad.,' 23rd April 1826).

brought about the general recognition of Lavoisier's ideas; whereas the more exclusive representatives of chemistry, such as Berthollet and Guyton, held aloof for some considerable time. In the earlier syllabus of the École polytechnique, chemistry was brought into a similar proximity with the mathematical branches. And Berthollet's 'Statique chimique' denotes by its title alone the mathematical spirit in which the work was conceived.

23. New mathematical sciences. About that time also two new sciences were, if not invented, at least set on a firm basis, by which the use of mathematics was very largely extended, and by which great realms of interesting facts were made accessible to accurate measurements and exact reasoning. Both these sciences can be claimed by France as almost exclusively
24. Crystallography. her own creations. They are the science of crystallography and the great theory of probabilities. The former was the work of the Abbé Haüy; the latter formed, next to the mechanics of the heavens, the main original contribution by which Laplace has perpetuated his name in the history of science. (The theory of the Abbé Haüy, who first taught how crystals are built up from small particles of definite and regular geometrical forms, such as cubes, pyramids, &c., came to the aid of the mineralogists, who before him had vainly groped in the dark, searching for some method by which order and system could be introduced into the lifeless forms of nature as by the methods of Linnæus and Jussieu it had been introduced into the world of plants and animals. Before Haüy, the doctrines of mineralogy had been either attached to geology—especially in the celebrated school of Werner, or latterly, after the great developments in chemistry had

set in, to chemistry—especially by Bergmann.[1]) Haüy established the science of minerals on an independent foundation by studying and systematising the forms of their crystallisation; and he brought the science of mineralogy from Sweden and Germany into France, and gave it an independent position. Thus it came to form a connecting-link between the mathematical—*i.e.*, the measuring and calculating—and the purely descriptive sciences. "Mineralogy, though it is that part of natural science which deals with the least complicated objects, is nevertheless also that which lends itself least to a rational classification. The first observers named the minerals vaguely according to their external appearances and their use. It was not until the middle of the eighteenth century that it was attempted to subject them to those methods which had done service to geology and botany: the hope existed of establishing among them genera and

[1] See an account of the work of the chemical school, to which Cronsted (the inventor of the blow-pipe), Bergmann, Kirwan, and Klaproth belonged, in Cuvier's 'Rapport' (p. 163). Also his "Éloge de Haüy" ('Éloges histor.,' vol. iii. p. 143, &c.) The beginnings of geometrical crystallography seem to go back to Linnæus; but his view was discouraged in France by Buffon, who disliked Linnæus's writings. Whewell, who was himself an authority on crystallography, thinks Romé de l'Isle, who was not an Academician, had only scant justice done to him by Haüy and his friends ('Hist. of the Induct. Sciences,' 3rd ed., vol. iii. p. 176). More recent writers, such as Kobell ('Geschichte der Mineralogie,' München, 1864, p. 73, &c.) and Nicol (article "Crystallography," 'Ency. Brit.'), have done him justice. The 'Grande Encyclopédie' thus summarises the work of Romé de l'Isle: "Il mesura mécaniquement [*viz.*, with Carangeot's goniometer] les angles et établit que ces angles ont toujours une valeur constante dans une même espèce minéralogique." That of Haüy is summarised in the two laws: "1°, Tous les éléments semblables d'un cristal sont toujours semblablement et simultanément modifiés (loi de symétrie); 2°, toute facette modifiante intercepte sur les arêtes de la figure primitive des longueurs proportionelles à des multiples simples de la longueur de ces arêtes (loi de dérivation)" (Berthelot in 'Grande Encyclop.,' vol. xiii. p. 397).

species, as among organised beings, and it was forgotten that in mineralogy the principle is absent which had given birth to the idea of species—*viz.*, that of generation. The principle of individuality, such as it is conceived in the organic world—*viz.*, the unity of action of different organs which co-operate in the preservation of the same life—can scarcely be admitted in mineralogy."[1]

The Abbé Haüy, by founding the science of minerals on their regular forms of crystallisation, made mineralogy "as precise and methodical as astronomy; in fact, we can say in one word that he was to Werner[2] and Romé de l'Isle, his predecessors, what Newton had been to Kepler and Copernicus."[3]

25. Theory of Probability.

From that well-defined province of science which deals in a precise and strict manner with the simple numerical relations which seem to underlie all forms of movement in nature, be they on a stupendous or on a minute scale

[1] Cuvier, "Éloge de Haüy" in 'Éloges historiques,' vol. iii. p. 155.

[2] The character of Werner (1750-1815) is nowhere better painted than by Cuvier in his "Éloge de Werner" (*loc. cit.*, vol. ii. p. 303, &c.) "Il commence l'époque la plus remarquable de la science de la terre, et même l'on peut dire qu'à lui seul il la remplit. . . . Il s'est formé des académies entières, qui ont pris son nom" (for instance, the Edinburgh Wernerian Society, founded by Jameson, 1808-1859), "comme si elles eussent voulu invoquer son génie et s'en faire un patron d'une espèce auparavant inconnue. Qui ne croirait, à entendre parler de succès si peu ordinaires, que ce fut quelqu'un de ces hommes ardens à propager leur doctrine, qui par des ouvrages nombreux et éloquens, ont subjugué leurs contemporains, ou qui se sont procuré des partisans par l'ascendant d'une grande richesse ou d'une position élevée dans l'ordre social! Rien de tout cela: confiné dans une petite ville de Saxe, sans autorité dans son pays, il n'avait aucune influence sur la fortune de ses disciples; il n'entretenait point de liaisons avec des personnes en place: d'un naturel singulièrement timide, hésitant toujours à écrire, à peine subsiste-t-il de lui quelques feuilles d'impression. . . . C'est ainsi qu'en peu d'années la petite école de Freyberg, destinée seulement, dans le principe, à former quelques mineurs pour la Saxe, renouvela le spectacle des premières universités du moyen âge," &c., &c.

[3] Cuvier, ibid., p. 163.

—*i.e.*, from the province of mechanics and astronomy—two different roads lead into those extensive domains in which, not simplicity and regularity, but endless variety and complication, seem to be the order and the rule of Life. Even a century ago the contrast must have been striking between the 'Principia' of Newton and the 'Exposition du Système du Monde' of Laplace on the one side, and the great array of volumes of Linnæus, Buffon, Jussieu, Cuvier, and Lacépède on the other; though these after all embraced only a small portion of the living forms of nature which they attempted to classify or to describe.[1] I have pointed out how the new and especially the French methods of chemistry and crystallography conquered a large portion of intermediate ground, subjected many tangled phenomena to exact treatment, and pushed the mathematical method far into the dominion of natural history. It is that other history, not natural, but human and often unnatural, which presents the opposite extreme of the great panorama of world-life. It is significant that almost at the same time that mathematical reasoning found its way into natural history, conquering an extensive province of its vast territory, an entirely different method was invented with the aim of dealing in a still more vigorous manner with the phenomena of human life and society. This was the science of statistics, and

[1] Cuvier gives some figures as to the increase of the known species during his own lifetime. Lacépède had described about 1200 or 1300 distinct species of fishes; but when Cuvier pronounced his Éloge in 1826, the Cabinet du Roi contained already more than 5000 species ('Éloges historiques,' vol. iii. p. 317). Linnæus had counted in 1778 about 8000 species of plants. Cuvier in 1824 estimates the number as 50,000 or more (see 'Éloges,' vol. iii. p. 469, &c., where he also gives some idea of the numbers of known species in the different classes of animals).

connected with it the doctrine of averages and the mathematical theory of probabilities.[1] The same great mind

[1] The beginnings of the science and theory of probabilities are not subject to controversy, as were those of the infinitesimal calculus. Pascal and Fermat about the middle of the seventeenth century entered into a correspondence relative to a question in a game of chance, propounded by the Chevalier de Méré, a noted gambler. They agreed in their answer, but could not convince their friend, who moreover made this the occasion of denouncing the results of science and arithmetic. But this comparatively insignificant problem — so different from the great cosmical problems which led to the invention of the infinitesimal calculus about the same time — was the origin of a series of investigations and discussions in which the greatest mathematicians, such as Huygens, James and Daniel Bernoulli, De Moivre, D'Alembert, and Condorcet joined. Most of them did not escape the errors and misstatements which creep in an insidious manner into the discussion and vitiate the conclusions. In fact, the science advanced through the influence of those who depreciated it like D'Alembert, and those who exaggerated its importance like Condorcet. At length, under the hands of Laplace, who defined it as common-sense put into figures and attributed to it a high educational value, it assumed a state wellnigh approaching to that perfection which Euclid gave to geometry and Aristotle to logic. Since the publication of Laplace's celebrated 'Théorie analytique des Probabilités' (Paris, 1812) writers on the subject have found ample occupation in commenting on the theorems or recasting the proofs given in that work, which holds a similar position to that occupied in another department of mathematics by the 'Disquisitiones Arithmeticæ' of Gauss (1801). Up to the present day there exist differences of opinion as to the value of the science, the two opposite views being represented in this country by Mill ('Logic,' 5th ed., vol. ii. p. 62) and Jevons ('Principles of Science,' vol. i.), the latter summing up his opinion as follows: "In spite of its immense difficulties of application, and the aspersions which have been mistakenly cast upon it, the theory of probabilities is the noblest, as it will in course of time prove perhaps the most fruitful, branch of mathematical science. It is the very guide of life, and hardly can we take a step or make a decision of any kind without correctly or incorrectly making an estimation of probability" (1st ed., p. 248). A similar opinion seems to have been held by James Clerk Maxwell (see Life by Campbell and Garnett, p. 143), who called the calculus of probabilities "Mathematics for practical men." In this country A. de Morgan and Todhunter, the former in a popular essay in the 'Cabinet Cyclopædia' and in a profound treatise in the 'Encyclopædia Metropolitana,' the latter in his well-known History (London and Cambridge, 1865), have done a great deal to make this subject better understood. The applications of the theory have gradually increased through numerous mortality and insurance calculations; as also in the estimations of error in astronomical and physical observations, where the well-known method of least squares (first employed by Gauss in 1795, see Gauss, Werke, vol. vii. p. 242; first published by Legendre in 1806, and then proved by Laplace in his 'Théorie,' &c., 1812) is now extensively employed. Of this branch of mathematics Bertrand says: "Les plus grands géomètres ont écrit sur le calcul des probabilités; presque tous ont commis des erreurs: la cause en est, le plus souvent, au désir d'appliquer des principes à des problèmes qui par leur nature échappent à la science." In the hands of Clerk Maxwell the calculus has acquired an additional interest and importance through the distinction which he made between what he termed the "historical" and the "statistical method" of treating phenomena, and the application of the latter to the kinetic theory of gases (see Life, pp. 438, 562). This subject will occupy our attention in a special chapter.

which elaborated the principles of Newton into a system of the universe, and attacked the intricate mathematical problem which this system presented, gave to the world likewise the first complete treatise on that calculus which comes into play if we eliminate from the apparently most arbitrary region of phenomena, that of human life and history, all regard for final or efficient causes, for providential design and freewill, for human error, human malice and benevolence—in fact, all notice of that element which from another and equally important point of view forms the subject of greatest interest—the inner life of the individual. It was proposed, and it has since been carried out, to look upon human beings and human events not as things possessed of an inner world of thought and freewill, but as lifeless units, more uniform and regular than the balls thrown into the urn at an election, or the counters in a game of chance. By overstepping with one bound the great field of human activity, full of so much confusion and so much interest, it was proposed to investigate what knowledge would result from a purely mathematical inspection, in which human beings figured merely as units and symbols.[1] This attempt, which has since

[1] The beginnings of the science of statistics belong likewise to the age that produced the higher mathematics. More extensive "countings" seem to have been contemporaneous with more refined calculations. Hermann Conring, professor at Helmstädt, a friend of Leibniz (see Leib-

led to such interesting results, and which has furnished almost all the knowledge upon which a judicious regulation and government of society depends, was the work of Laplace, and was produced in an age and in a nation which seemed to have set at naught all ideas of order and method in human affairs, which defied all authority and all tradition, and trusted its fate to the most radical revolution which civilised society ever witnessed.[1]

It is curious to read the criticism which the first Napoleon, that wayward child of the Revolution, passed on the author of the mechanics of the heavens and the theory of probability. Laplace, like so many other men of science, had been called by the Emperor to assist in the labours of administration, but, according to his judgment, proved himself a poor administrator, being unable

niz's 'Philosophische Schriften,' ed. Gerhardt, vol. i. p. 155), lectured about 1660 on subjects now comprised under the term "Statistics," and about the same time John Graunt of London published 'Natural and Political Annotations made upon the Bills of Mortality' (1666). Sir William Petty, one of the founders of the Royal Society, published in 1683 'Five Essays in Political Arithmetick.' The newly discovered calculus of probabilities induced mathematicians to take an interest in the subject, and to urge the desirability of gaining data for their calculations. Many of these turned upon questions of mortality and the ravages of diseases, such as the smallpox. But though undoubtedly the fact that during the French Revolution mathematicians for the first time had a great influence in administrative and governmental matters contributed enormously to the introduction of statistical methods, the great epoch in this science is allied with the name of the Belgian Quetelet (1796-1874), of whom more later on.

[1] Cantor ('Historische Notizen über die Wahrscheinlichkeitsrechnung,' Halle, 1874, p. 6) says: "The tendency of thought which prepared the Revolution, and which is marked by an unsparing and destructive criticism of the conditions of society in state and family, could not dispense with an instrument which, more than any other, enables one to subject to general views the most different factors of civilisation. It belonged to the favourite ideas of that age, that the calculus of probabilities should be among the most important subjects of public instruction; for it was said to be the calculus of common-sense, through which alone the influence of hope, fear, and emotion on our judgment could be destroyed, and prejudice and superstition removed from the decisions of social life."

to grasp practical issues, and always descending into infinitesimals. It is hardly to be doubted now, after the lapse of a century, that the infinitesimals of Laplace play a more important part in problems of administration and government than the ideas of Napoleon. Laplace, unlike some other great scientific thinkers, attached great value to a popular exposition of the principles of his discoveries. Descartes required a Fontenelle and Newton a Voltaire to make their ideas accessible and useful to the mass of students. Laplace was his own Fontenelle and Voltaire. "Few works," says Sir John Herschel, "have been more extensively read, or more generally appreciated, than Laplace's 'Essai philosophique sur les Probabilités,' and that on the 'Système du Monde' by the same author. It is not, perhaps, too much to say that were all the literature of Europe to perish, these two essays excepted, they would suffice to convey to the latest posterity an impression of the intellectual greatness of the age which could produce them, surpassing that afforded by all the monuments antiquity has left us. Previous to the publication of the 'Essai philosophique,' few, except professed mathematicians or persons conversant with assurances and similar commercial risks, possessed any knowledge of the principles of this calculus, or troubled themselves about its conclusions, regarding them as merely curious and perhaps not altogether harmless speculations. Thenceforward, however, apathy was speedily exchanged for a lively and increasing desire to know something of a system of reasoning which for the first time seemed to afford a handle for some kind of exact inquiry into matters no one had ever expected to see reduced to calculation, and bear-

ing on the most important concerns of life. Men began to hear with surprise, not unmingled with some vague hope of ultimate benefit, that not only births, deaths, and marriages, but the decisions of tribunals, the results of popular elections, the influence of punishments in checking crime, the comparative value of medical remedies and different modes of treatment of diseases, the probable limits of error in numerical results in every department of physical inquiry, the detection of causes, physical, social, and moral—nay, even the weight of evidence and the validity of logical argument—might come to be surveyed with that lynx-eyed scrutiny of a dispassionate analysis, which, if not at once leading to the discovery of positive truth, would at least secure the detection and proscription of many mischievous and besetting fallacies."

Both ways of approaching the intricate phenomena of nature and history, that of mechanics dealing with the general laws of motion and of lifeless masses, and that of statistics dealing with the arithmetical properties of large numbers of units, leave out of consideration that hidden and mysterious phenomenon to which alone is attached, if not order and method, yet certainly all that commands interest in the created world: the factor of life—the existence of individuality.

26. Laplace gained his results by disregarding the principle of individuality.

The view which Laplace took of the universe or of human affairs is an attempt to see how far science and reasoning can go while disregarding the principle of individuality.[1] The

[1] See Clerk Maxwell on 'Science and Freewill' (Life by Campbell and Garnett, p. 438): "Two kinds of knowledge, which we may call for convenience dynamical and statistical. The statistical method of investigating social questions has Laplace for its most scientific and Buckle for its most popular expounder. Persons are grouped according to some characteristic, and the number of persons forming

method has been most fruitful, and, far from being exhausted, promises undreamt of results in the future. It was probably more from the desire to keep his view clear and his method simple, than with any necessarily sceptical tendency, that when Laplace was questioned by Napoleon how it was that in the great volumes of the 'Mécanique céleste' the name of God did not appear, he replied, "Sire, je n'ai pas besoin de cette hypothèse."

27. Individuality the centre of interest in the sciences of life.

But French science did not leave that great field of research uncultivated, which is the very playground of individual life. Its cultivation was the work of that other great representative of French science—the contemporary of Laplace—Georges Cuvier.[1] Linnæus had

the group is set-down under that characteristic. This is the raw material from which the statist endeavours to deduce general theorems in sociology. Other students of human nature proceed on a different plan. They observe individual men, ascertain their history, analyse their motives, and compare their expectation of what they will do with their actual conduct. This may be called the dynamical method of study as applied to man. However imperfect the dynamical study of man may be in practice, it evidently is the only perfect method in principle, and its shortcomings arise from the limitation of our powers rather than from a faulty method of procedure. If we betake ourselves to the statistical method, we do so confessing that we are unable to follow the details of each individual case, and expecting that the effects of widespread causes, though very different in each individual, will produce an average result on the whole nation, from a study of which we may estimate the character and propensities of an imaginary being called the Mean Man."

[1] It is not necessary here to explain the reasons which have induced me to confine myself mainly to the two great names of Laplace and Cuvier as the great representatives of the exact scientific spirit, as it first asserted its supremacy in France, and from there gradually fought its way all over Europe. To me it seems that nowhere has this modern scientific spirit been represented in greater completeness and greater purity. This is so much the more remarkable, as other influences and temptations were not wanting in that age and country which might have interfered with the application of the purely scientific method. The scientific spirit is in danger of being contaminated by two interests which are essentially foreign to it: the one is the practical, the other the philosophical. Frequently they are united; and when united their influence on the progress of science has frequently been disastrous. In no department of knowledge has this

begun the work of natural history by inventing a system of classification and a technical language or nomenclature. Buffon in his brilliant and elegant portraits had cast around it the charms of poetry and romance. Jussieu had imported botany from Sweden into France, and in the garden of Trianon had given a living model of the arrangement of plants; botanising had become popular through the

union of the practical and philosophical spirit been more marked than in the medical sciences. Essentially interested as it is in the immediate application of scientific discoveries to the needs of suffering mankind, we witness in the course of the seventeenth and eighteenth centuries a one-sided alliance of the art of healing with chemistry (Sylvius, 1614-1672), with physics (Borelli, 1608-1679), and with mechanics (Pitcairn, 1652-1713), and the reaction of the animists (Stahl, 1660-1734, and Hoffmann, 1660-1742), and the vitalists (Bordeu, 1722-1776, and Barthez, 1734-1806). A large portion of the history of medicine (see Haeser, 'Geschichte der Medicin,' Jena, 1881, vol. ii., and Guardia, 'Histoire de la Médecine,' Paris, 1884) consists in the account of the opposition to premature generalisations, adopted from other sciences, or still more dangerously from metaphysics. As examples of the metaphysical tendency we have the Scotch systems of Cullen and Brown, and the German "Philosophy of Nature." The reasons why philosophy has so frequently allied itself with medicine, thus preventing the purely scientific spirit from gaining admission, are twofold. "Young men," says Cuvier, "adopt these theories with enthusiasm, because they seem to abridge their studies and to give a thread in an almost inextricable labyrinth" ('Rapport,' p. 333). The other reason is that the art of healing has as much a psychological as a physical side, and a philanthropic as much as a scientific interest. In respect of this it is well to note that the age and country which gave to Europe the great models of purely scientific research in Laplace and Cuvier was rich also in great thinkers who applied themselves in a philosophical spirit to the advancement of scientific and practical medicine, to the reform of hospitals, to the care of the insane, to the education of the deaf and dumb. The whole school of the ideologues, headed by Condorcet, Cabanis, and Destutt de Tracy, was closely allied with the medical profession. But however important this side of French thought may have been, its influence on the rest of Europe at that time cannot be compared with that of the purely scientific writings belonging to mathematics and natural science. Such names as Cabanis and Bichat belong to a different current of European thought, which I purposely separate from the exact or purely scientific. And this separation is justified historically by the fact that in the Académie des Sciences for a considerable time medical science was only meagrely represented, whilst philosophy during the period of the suppression of the Académie des Sciences morales et politiques, from 1803-1832, had no academic representation at all. The great name of Bichat is not among the Academicians, and Cuvier himself

writings of Rousseau; gardening and the study of plant-life had become a royal pastime, and a favourite recreation for those oppressed with the troubles of the State or the sorrows of private life. Cuvier, while asking the reason why other portions of natural history had not shared the same attention, breaks out into the following eloquent words: "The study of animals presents diffi-

explains the exclusive attitude of the Academy to the medical profession in his Éloges of Hallé, Corvisart, and Pinel ('Éloges,' vol. iii. p. 339, &c.) See also Maury (p. 304): "Les sciences physiques, chimiques et naturelles avaient pris une telle extension dans les travaux de l'Académie, qu'à la fin du dix-huitième siècle, la médecine, qui n'y avait jamais été au reste bien largement représentée, fut de plus en plus reléguée à l'arrière plan; ce n'était plus que de loin en loin que les médecins, les chirurgiens de la Compagnie, . . . y présentaient des observations sur des points médicaux. . . . La médecine, qui, selon la juste observation de Cabanis, tend aux hypothèses par la nature même du sujet auquel elle s'applique, n'offrait point assez de constance dans ses principes et d'évidence dans ses démonstrations pour satisfaire des esprits qui se détachaient tous les jours davantage des vieilles spéculations de l'école. C'est ce qui explique le peu de faveur qu'elle rencontrait à l'Académie." To what extent this rigid demarcation, according to which "observations relatives aux dispositions morales et intellectuelles des individus n'entrent assurément dans les attributions d'aucune académie des sciences" ('Mémoires de l'Institut,' vol. ix. p. 110), was beneficial to medical science is an important question. In the organisation of the Institute of the 3rd brumaire, an iv. (25th October 1795), there are awarded out of 60 members only 6 to medicine and surgery combined, and in the "nouvelle organisation" of 3rd pluviose, an xi. (23rd January 1803), there are 6 members out of 63. This section is given as the last, even after "économie rurale et art vétérinaire" (see Aucoc, 'L'Institut,' p. 3, &c.) It is interesting to note how in contrast to this the medical profession occupied for a long period a foremost place in the Royal Society of London, so much so that frequently opposition was made to the admission of new members belonging to it (see Weld, 'History of the Royal Society,' vol. i. chap. 4; vol. ii. p. 153). Of 5336 papers contained in the 'Philosophical Transactions' from 1665 to 1848, 1020, the largest number in any department, belonged to anatomy, physiology, and medicine (ibid., vol. ii. p. 565). Babbage complained of the influence of the Colleges of Physicians and Surgeons in the Royal Society, as occasionally filling the pages of the 'Transactions' with medical papers of very moderate merit; and also because the preponderance of the medical interest introduces into the Society some of the jealousies of that profession ('Decline of Science in England,' 1830, p. 188). In the foundation of the British Association this union with the medical interest was dropped; though the older "Versammlung deutscher Naturforscher und Ärzte," after which it was modelled, established and maintained that union.

culties which only great zeal can surmount; we have to subject them to torments in order to appreciate their physical powers; their innermost energies only reveal themselves to the dissecting-knife—only by living among corpses can we discover them. Among them we find the same spectacle as in the world, whatever moralists may say: they are hardly less wicked or less unhappy than we are; the arrogance of the strong, the meanness of the weak, vile rapacity, short pleasures bought by great efforts—death brought on by long suffering—that is the rule among animals as much as among men. With plants existence is not surrounded by pain—no sad image tarnishes their splendour before our eyes, nothing reminds us of our passions, our cares, our misfortunes—love is there without jealousy, beauty without vanity, force without tyranny, death without anguish—nothing resembles human nature."[1]

28. Into this centre Cuvier carried exact research.

Into the centre of individual and organised life—the life of the animal and human creation—Cuvier carried exact research, grounding it on the science of comparative anatomy.[2] At the same time, he marked out as the principal problem, around which all investigations must turn, and upon which all classification must depend,

[1] 'Éloges historiques,' vol. i. p. 91.

[2] Cuvier, in the Introduction to 'Le Règne animal, distribué d'après son organisation, pour servir de base à l'histoire naturelle des animaux et d'introduction à l'anatomie comparée' (Paris, 1817), says that for thirty years he had devoted to comparative anatomy all his time (p. v), that the first results had appeared in 1795, his 'Leçons d'Anatomie comparée' in 1800 (p. vii), that he has made anatomy and zoology march side by side (p. vi). He compares natural history as a science with other sciences, stating that dynamics is become a science almost entirely of calculation, that chemistry is still a science altogether of experiments, that natural history will for a long time to come remain in most of its parts a science of observation (p. 5); he maintains that geometry is a study of syllogisms, natural history a study of method (p. xviii).

the phenomenon of individual life, that great vortex into which agencies, processes, and the elements of inorganic nature are continually drawn, from which they are continually ejected, preserving not the unity of substance but, among changing events, the unity of form.[1]

"It is not," he says, "in the substance that in plants and animals the identity of the species is manifested, it is in the form. There are probably not two men, two oaks, two rose-trees, which have the compound elements of their bodies in the same proportion—and even these elements change without end, they circulate rather than reside in that abstract and figured space which we call the form; in a few years probably there is not left one atom of that which constitutes our body to-day—only the form is persistent; the form alone perpetuates in multiplying itself; transmitted by the mysterious operation which we call generation to an endless series of individuals, it will attract successively to itself numberless molecules of different matter, all of them merely transient."[2]

[1] "La vie est donc un tourbillon plus ou moins rapide, plus ou moins compliqué, dont la direction est constante, et qui entraîne toujours des molécules de mêmes sortes, mais où les molécules individuelles entrent et d'où elles sortent continuellement, de manière que la forme du corps vivant lui est plus essentielle que la matière" ('Règne animal,' p. 13, &c.) "Il vient sans cesse des éléments du dehors en dedans: il s'en échappe du dedans au dehors: toutes les parties sont dans un tourbillon continuel, qui est une condition essentielle du phénomène, et que nous ne pouvons suspendre longtemps sans l'arrêter pour jamais. Les branches les plus simples de l'histoire naturelle participent déjà à cette complication et à ce mouvement perpétuel, qui rendent si difficile l'application des sciences générales" ('Rapport,' p. 150, &c.) "Dans les corps vivans chaque partie a sa composition propre et distincte; aucune de leurs molécules ne reste en place; toutes entrent et sortent successivement: la vie est un tourbillon continuel, dont la direction, toute compliquée qu'elle est, demeure constante, ainsi que l'espèce des molécules qui y sont entraînées, mais non les molécules individuelles elles-mêmes. . . . Ainsi la forme de ces corps leur est plus essentielle que leur matière," &c. (ibid., p. 200).

[2] 'Éloges historiques,' vol. iii. p. 156.

Keeping this unity of form, this absorbing vortex of life, the totality of organisation, always before him, Cuvier, in surveying the whole region of animated nature,[1] fixes finally for the purposes of classification and division on that system of organs which expresses most truly the peculiarity of each of the great branches into which he divides the animal world—namely, the nervous system.[2] But rather than follow him at present into the

[1] "La partie anatomique du problème général de la vie est résolue depuis longtemps pour les animaux, au moins pour ceux d'entre eux qui nous intéressent le plus. Les voies que les substances y parcourent, sont connues; . . . il aperçoit aussi comment ces routes, si compliquées dans l'homme, se simplifient par degrés dans les animaux inférieurs, et finissent par se réduire à une spongiosité uniforme. Les recherches de M. Cuvier—dans les leçons d'anatomie comparée—ont achevé d'assigner à chaque animal sa place dans la grande échelle des complications de structure" ('Rapport,' p. 202, &c.)

[2] It is not my object here to give an account of the views of Cuvier, still less of his contributions to natural history, which—in spite of the special theories and laws which he and his followers established (see especially Flourens, 'Histoire des Travaux de Georges Cuvier,' 3me éd., 1858)—remained in his hands to the last pre-eminently a science of observation. It has been pointed out that Cuvier only gradually (probably about 1812) arrived at the final principle of division—*viz.*, the nervous system—and that he adopted it from others (notably Virey and De Blainville), that before 1812 he had successively used the organs of generation (1795), of nutrition, and of circulation as principles of classification. In his Report of 1808, in mentioning his own labours, he says: "M. Cuvier, en étudiant la physiologie des animaux vertébrés, a trouvé dans la quantité respective de leur respiration, la raison de leur quantité de mouvemens, et par conséquent de l'espèce de ces mouvemens. . . . En effet, M. Cuvier, ayant examiné les modifications qu'éprouvent dans les animaux sans vertébres les organes de la circulation, de la respiration, et des sensations, et ayant calculé les résultats nécessaires de ces modifications, en a déduit une division nouvelle où ces animaux sont rangés suivant leurs véritables rapports" ('Rapport,' p. 311, &c.) Compare also Carus, 'Geschichte der Zoologie,' München, 1872, p. 602; Flourens, "Éloge de Cuvier," in his 'Éloges historiques,' 3me série, Paris, 1862, p. 122, &c.; Hahn in the 'Grande Encyclopédie,' article "Cuvier." See also the Introduction to the 'Règne animal,' which proposes to arrange living beings according to their "organisation," by investigating their "structure," their "internal as well as external conformation." Cuvier here states that no one before had tried to arrange the classes and orders according to the "ensemble de la structure" (p. vi). He is thus led to the law of the "subordination des caractères, . . . ayant soin d'établir toujours la correspondance des formes extérieures et intérieures qui, les unes comme les

details of his natural history, his comparative anatomy, or his palæontology, of which latter sciences he is the creator, it serves our present purpose better to learn how he viewed the object of natural science in general—how he defined its task. As the first step in civilisation was the creation of a language possessing definite rules, so the first step in the growth of a science is that taken by Linnæus, who was not terrified by this enormous work, that of giving names, of framing a nomenclature.[1] "But," says Cuvier, "to name well, you must know well. These

autres, font partie intégrante de l'essence de chaque animal" (p. xiv). He opposes former artificial classifications, such as the principle that living beings can be arranged "de manière à former des êtres une seule ligne" (p. xx). "Un être organisé est un tout unique, un ensemble de parties qui réagissent les unes sur les autres pour produire un effet commun. Nulle de ses parties ne peut donc être modifiée essentiellement sans que toutes les autres ne s'en ressentent" ('Éloges,' vol. ii. p. 279).

[1] The formation of a nomenclature or a terminology is one of the most important steps in the beginning and the progress of science. Cuvier refers frequently to this: "Nos livres saints, à leur début, nous représentent le Créateur faisant passer ses ouvrages sous les yeux du premier homme, et lui ordonnant de leur imposer des noms. . . . Ces noms, qu'il est prescrit à l'homme d'imposer, ne sont pas des signes incohérens appliqués au hasard à quelques objets isolés. Pour qu'ils deviennent réguliers et significatifs, ils exigent, comme il est dit, que les êtres aient passé devant le nomenclateur" ('Éloges,' vol. iii. pp. 450, 452). Nowhere is terminology more important than in chemistry. "L'un des moyens qui ont le plus puissamment contribué à faciliter l'enseignement de la science en général, et à préparer l'adoption universelle de la théorie nouvelle, c'est la nomenclature créée par cette société de chimistes français. . . . Donner aux élémens des noms simples; en dériver, pour les combinaisons, des noms, qui exprimassent l'espèce et la proportion des élémens qui les constituent, c'était offrir d'avance à l'esprit le tableau abrégé des résultats de la science, c'était fournir à la mémoire le moyen de rappeler par les noms la nature même des objets. C'est ce que M. Guyton de Morveau proposa le premier dès 1781, et ce qui fut complètement exécuté par lui et par ses collègues en 1787" ('Rapport,' p. 88, &c.) Cf. 'Éloges,' vol. iii. pp. 194, 482, 496. Cuvier ('Éloges,' vol. iii. p. 302) mentions "cette antipathie pour les méthodes et pour une nomenclature précise à laquelle Buffon s'est laissé aller en tant d'endroits"; he speaks of Pinel "qui avait cherché d'abord à former pour les descriptions des maladies un langage précis, modelé sur celui que Linnæus avait introduit en botanique" (ibid., vol. iii. p. 386).

beings and their parts which are to be known are to be counted by the million; it is not enough to know them singly, for they are submitted to an order, to mutual relations, which must likewise be appreciated, for it is according to this order that each has its part to play, that each disappears at its time, that they reappear similarly made, always in the same proportions, and armed with the necessary forces and faculties for the maintenance of these proportions, and of the whole of this perpetual vortex. Not only is each being an organism, the whole universe is one, but many million times more complicated; and that which the anatomist does for a single animal—for the microcosm—the naturalist is to do for the macrocosm, for the universal animal, for the play of this alarming aggregation of partial organisms." [1]

It was this sustained regard for the value of detailed research and minute observation, coupled with an equal appreciation of the unity of all regions of existence, and all branches of learning, that elevated Cuvier to the height of the science of his age and his country, and made him a true exponent of the modern scientific spirit. The works of Newton and Laplace may contain more formulæ of lasting value, more instruments of permanent scientific use—they may, for all time, have traced a few lines of the enwoven cipher of the all-pervading mechanism of nature; it is, however, well to note that he only who keeps in steadfast view the life rather than the mechanism of existence, approaches the great secret of nature, and gauges rightly the value of each component

[1] Cuvier, 'Éloges historiques,' vol. iii. p. 453.

part, or the worth of each human effort.[1] In this respect the nineteenth century knows no greater figure than Cuvier; not even Humboldt, great and comprehensive as was his scientific view. The advantages also of Cuvier's position as permanent Secretary of the French Academy of Sciences were exceptional, and well fitted to bring out his extraordinary talents. We can say that in him science has become fully conscious of its true methods, its usefulness, its most becoming style, its inherent dignity, its past errors, its present triumphs, the endless career which lies before it, and the limits which it cannot transgress.

29. Cuvier's training.

Educated in Germany, at the same school as Schiller and Dannecker,[2] imbued by early experience and by

[1] "C'est la continuation de ce commandement de voir et de nommer, par où s'ouvre la vie de notre espèce, c'est la voie qui devait nous conduire soit à des contemplations plus hautes, soit seulement à des inventions utiles. En effet l'histoire naturelle ne fait aucun pas sans que la physiologie et la philosophie générale marchent d'un pas égal, et sans que la société reçoive leur tribut commun" ('Éloges,' vol. iii. p. 474).

[2] Cuvier has himself written an account of his early life and studies. It is given by Flourens, 'Éloges,' vol. i. pp. 167-193. He was born in 1769, of a Protestant stock, at Montbéliard, the capital of a small principality, situated in the Jura, and then belonging to Würtemberg. The autocratic Duke Charles (1737-1793) had founded a military academy in Stuttgart, his capital, where 400 youths were at his expense housed and educated according to a strict rule, but under the guidance of enlightened masters, and in a thoroughly modern spirit. The institution was a kind of opposition to the Protestant Church rule, which had very early spread a system of popular and compulsory education throughout the country. It is a chapter of history well worth reading. The great problems of popular education as against higher instruction, Protestant discipline in the lower as against military discipline in the higher schools, the democratic as against the aristocratic spirit, the independence as against the State-regulation of University teaching, were fought out by the dukes and the Estates of Würtemberg in a prolonged warfare, a sample of similar movements all over Germany, and well told by Perthes in his 'Politische Zustände und Personen in Deutschland zur Zeit der französischen Herrschaft' (Gotha, 1862, pp. 501-548). Cuvier evidently saw the better side of the system, for he entered after the imperious character of the duke had been subdued by the victorious estates. Forced to change his ways, which he conscientiously did, the duke laid by for his country, as a local historian says, "a fund of in-

personal contact with that spirit of general education and universal training which then animated the German-speaking nations of the Continent, thoroughly grounded in classics and mathematics, with a cosmopolitan knowledge of languages and literature, which fitted him to understand the merits of different nations, he became the great exponent of that peculiar system of higher culture which since the time of Colbert the French had elaborated—the academic system.[1] The centre of this

telligence and acquisitions by which we have benefited up to modern times" (Perthes, p. 510). We know the other and older side of the picture from the 'Life of Schiller' (see, *inter alia*, Carlyle, 'Life of Schiller,' collected works, library edition, vol. v. p. 258). Cuvier gives a long description of the "Karlschule": "C'était un établissement vraiment magnifique. Environ quatre cents boursiers et pensionnaires, logés dans un édifice tel qu'il n'y en a aucun d'approchant en Europe (parmi ceux qui sont consacrés à l'instruction de la jeunesse), vêtus d'un bel uniforme, conduits par des officiers et des sous-officiers tirés des régiments du duc, recevaient des leçons de tout genre de plus de quatre-vingts maîtres ou professeurs. On a beaucoup parlé de l'esprit de despotisme avec lequel le duc disposait de leurs personnes et choisissait pour chacun d'eux l'état qu'il devait embrasser, et je crois en effet qu'il en était ainsi dans l'origine de l'établissement; mais de mon temps, je n'ai rien vu de semblable, et ce qui est certain, c'est que personne ne prétendit même me donner de conseil à cet égard. Il y avait cinq facultés supérieures, droit, médecine, administration, militaire et commerce" (Flourens, *loc. cit.*, p. 171).

[1] The first great representative of this academic spirit and culture was Fontenelle, who, living during a hundred years, from 1657 to 1757, was Secretary of the Académie des Sciences during forty-two years, from 1699 (the year of the reconstitution of the Academy) to 1741. Among his successors were men like Condorcet, Delambre, Cuvier, and Arago. Fontenelle gave to scientific subjects a dignified popularity, separated the departments of science and metaphysics, kept the scientific interest free from the commercial, and through his connection with the Académie française did probably more than any other writer to establish that superiority of style and diction for which the great French men of science are so remarkable and so superior to those of other countries. Bertrand, himself a successor of Fontenelle, says of him: "Prêtant aux travaux de ses confrères la finesse de ses aperçus et la vivacité ingénieuse de son style, il a su dans leurs portraits, qui sont des chefs-d'œuvre, plus encore que dans l'analyse de leurs découvertes, donner aux plus humbles et aux plus modestes une célébrité imprévue et durable, et le juste et sérieux hommage qu'il rend au vrai mérite fait aimer et respecter tout à la fois les savants et la science" ('L'Académie des Sciences et les Académiciens,' p. 113). See also Voltaire's

system was the old Academy of Sciences, which, with a short interruption during the storm of the Revolution, survived,[1] and formed the principal feature in the Institute. Allied with this institution, and directly inspired by its spirit, were the great schools of natural science, the great collections of natural objects, latterly also the great medical institutions of Paris. It professed to protect scientific studies in a royal and generous manner, attracted talent from outside, rewarded foreign as well as French research,[2] and tried to keep the scientific spirit of inquiry, as well as the form in which it found expression, pure and undefiled.[3] It favoured the co-

'Siècle de Louis XIV.'; Cabanis, 'Révolutions de la Médecine' (Œuvres, Paris, 1823, vol. i. p. 200); Flourens, 'Éloges historiques,' vol. iii. p. 31, &c.; Maury, 'Les Académies d'autrefois,' vol. i. p. 153, 163 *et passim*; Bouillier, 'Éloges de Fontenelle,' Introduction.

[1] "Tandis que tout a été renouvelé dans la politique et les mœurs publiques . . . la vie scientifique et littéraire a sensiblement gardé sa constitution. . . . Le Collège de France, l'Académie française, l'Académie des Inscriptions et Belles-lettres, l'Académie des Sciences, la Bibliothèque impériale, l'Observatoire, le Muséum d'Histoire naturelle, subsistent encore, comme au siècle dernier, et dans nos provinces, une foule d'académies sont d'une création antérieure à 1789" (Maury, *loc. cit.*, p. 1).

[2] "Euler fut quatre fois couronné pour des questions de physique et de mathématiques. . . . Daniel Bernoulli obtint le prix dix fois" (Maury, p. 171). Among the celebrated Éloges by Fontenelle there are those of Leibniz, of Peter the Great, of Newton, of Marsigli, of Boerhaave; among those by Condorcet there are those of Haller, Linnæus, Hunter, and Euler; among Cuvier's there are those of Gilbert, Priestley, De Saussure, Cavendish, Pallas, Rumford, Werner, Banks, and Davy.

[3] "Jusqu'à présent," says Fontenelle in 1699, "l'Académie des Sciences ne prend la nature que par petites parcelles. Nul système général, de peur de tomber dans l'inconvénient des systèmes précipités dont l'impatience de l'esprit humain ne s'accommode que trop bien, et qui, étant une fois établis, s'opposent aux vérités qui surviennent" (quoted by Flourens, 'Éloges,' vol. iii. p. 19). "L'esprit de l'Académie des Sciences a donc toujours été l'esprit d'expérience, d'étude directe, d'observation précise, l'amour de la certitude. D'abord cartésienne, elle devint ensuite Newtonienne," &c. (ibid., p. 21). Fontenelle contrasts the "philosophie des mots et celle des choses, de l'École et de l'Académie" ('Éloge de Du Hamel' in Bouillier, p. 10). "Fontenelle se plait à multiplier les exemples de cette incapacité chez les savants de faire fortune et de ce noble désintéressement." "Il aimait mieux

operation of many minds in rearing the great edifice of science, and found a place for the minutest research, as well as a field for the development and sway of great and governing ideas. Of the best form of this spirit and system—the Académie—Cuvier was the greatest representative. Through several dozen Éloges which he pronounced on the decease of a number of the most illustrious scientific men of Europe, as well as through several Reports, in which he summed up the labours and progress of his age, and the peculiar features of his period, he affords to the student of history an insight into that distinctive phase which scientific thought had entered in France at the end of the eighteenth century. This he allows us to contrast with other phases of thought, such as the philosophical or individual, which obtained in other ages or countries, and suggests as well as gives the means of answering the question, to what extent the scientific ideal

30. Cuvier the greatest representative of the Academic system.

étudier que subsister," he said of one of the Academicians (Bouillier, pp. ix, xii). Cuvier was very watchful over the Academy in keeping out the speculative spirit. See what he says in the joint Report on geology with Haüy and Lelièvre ('Mém. de l'Institut,' vol. viii. 1607, p. 136). "Que doivent donc faire les corps savans pour procurer à une science aussi intéressante et aussi utile, les accroissemens dont elle est susceptible ? . . . Ils doivent tenir la conduite, qu'ils ont tenue depuis leur établissement, à l'égard de toutes les autres sciences : encourager de leurs éloges ceux qui constatent des faits positifs et garder un silence absolu sur les systèmes qui se succèdent." Compare with this what he says about the use of the principle of "vital force," always referring to Newton's method ('Mém. de l'Inst.,' vol. vii. p. 77, &c.), further in his analysis of Gall and Spurzheim's Mémoire ('Mém. de l'Inst.,' vol. ix. p. 65): "Les commissaires de la classe . . . ont donné leur assentiment à presque toutes les propositions de MM. G. & S., qui ne dépendent que de l'inspection anatomique, &c. . . . les commissaires ont cru également de leur devoir de prévenir le public, qu'il n'y a aucun rapport direct, aucune liaison necessaire entre ces découvertes et le doctrine enseignée par MM. G. & S., &c. . . . Toutes ces matières sont encore trop étrangères aux attributions de la classe, elles tiennent aux faits sensibles d'une manière trop lâche, elles prêtent à trop de discussions vagues, pour qu'un corps tel que le nôtre doive s'en occuper" (p. 159).

of the end of this century agrees with or differs from that of its beginning. Upholding the Newtonian rather than the Baconian and Leibnizian standard in the mathematical and physical sciences,[1] he has marked that line which our whole century has contributed to trace out more distinctly; whilst, as regards the purely natural sciences, his continued emphasising of the great problem of organisation, and his later controversy with Geoffroy de Saint-Hilaire, mark that point in which this century has most distinctly departed from the prevailing ideas of its early years.[2] He also recognised earlier than any other mind of similar eminence what our century increasingly realises, how, without a system of condensation, contained in reports, statistics, and figures, aided by classifications and systems, the growing bulk of accumulated knowledge becomes chaotic and unmanageable.[3]

[1] Cuvier was not brought up in the school of the Encyclopædists, and I cannot find that he attached the great importance to the writings of Bacon which that school commonly did. As to Newton and Leibniz, he contrasts their methods, considering them "comme les chefs et les représentans des deux méthodes opposées qui se sont disputé l'empire de la science" ('Histoire des Sciences naturelles,' publiée par Magdeleine de Saint-Agy, Paris, 1841, vol. iii. p. 19, &c.) See also in his joint Report with Haüy and Lelièvre on the Science of Geology ('Mém. de l'Institut,' 1807, p. 133): "On vit renaître dans cette partie de l'histoire naturelle la méthode systématique de Descartes, que Newton semblait avoir bannie pour jamais de toutes les sciences physiques, . . . et lorsqu'on songe que Leibniz et Buffon sont au nombre des philosophes dont je parle ici," &c.

[2] A future chapter will deal specially with this subject. Cuvier, as is well known, maintained the fixity of species, and opposed the theories of St Hilaire and Lamarck, in which a later generation recognises the beginnings of the Darwinian doctrine of the transmutation of species. "On est obligé d'admettre certaines formes, qui se sont perpétuées depuis l'origine des choses, sans excéder ces limites; et tous les êtres appartenans à l'une de ces formes constituent ce que l'on appelle une espèce" ('Règne animal,' vol. i. p. 20).

[3] Cuvier was the first great scientific writer who undertook to give a historical survey of the position of the different natural sciences, with a view of ascertaining what had been achieved and what remained to be done. He did what

Cuvier had also a true historical sense, which enabled him to trace the connection of science with political history, with literature, with the fine and useful arts. And he helps to answer a question which to us is of paramount interest, How did science fare during the great cataclysm of the Revolution? how under the reactionary despotism of the First Empire? Before attempting to reply to these questions in the light of subsequent and general European history, I will select a few passages from Cuvier which throw light upon these points:[1]—

31. On the fortunes of science during the Revolution and the First Empire.

"There is always a revolution required in order to change habits which have become general, and the most necessary revolutions do not take place without some circumstance, which is sometimes long delayed. We have been able to see how in such a case everything furthers the sciences, even the delays and contrarieties which they seem to suffer under.

"The events which disturbed the world, and which for natural science temporarily dried up the sources of its riches,[2] obliged it to return to itself, and to make a new study of what it possessed, more fruitful than the most

a generation later the British Association undertook to do, and what in Germany the many "Jahresberichte" do nowadays. See his "Analyse des Travaux," &c., 'Mém. de l'Institut,' vol. ix. p. 53, and his celebrated 'Rapport historique sur le Progrès des Sciences naturelles depuis 1789,' Paris, 1810.

[1] 'Éloges historiques,' vol. iii. p. 456, 1824.

[2] This refers to the isolation of France during the war and the Continental blockade, which deprived it of foreign imports and the scientific collections of foreign specimens; see also 'Éloges,' vol. i. p. 9; vol. iii. p. 202: "Quand la jalousie des peuples nous privait des produits étrangers, la chimie les faisait éclore de notre sol." "Le conseil des mines établi en 1793, lorsque l'interruption de tout rapport avec l'étranger fit sentir le besoin de tirer parti de notre territoire a donné à ces sortes de recherches une impulsion toute nouvelle" ('Rapport,' p. 178).

fortunate departures could have been. During this apparent rest, all the different parts of method were deepened; the interior of natural objects was studied; even minerals were dissected and reduced to their mechanical elements; a still more intimate analysis was made by a perfected chemistry; the earth itself was, during this interval, if the expression is allowable, dissected by the geologists; its depths were sounded; the order and layers of rock which form its shell were recognised.[1] In the absence of foreign contributions the interior of the soil on which we walk became tributary to science. The beings of which it contains the remains came to light, and revealed a natural history anterior to that of today, different in its forms, and nevertheless subject to similar laws, thus giving to these laws a sanction which no one expected. The botanists did not gather so many plants in their collections, but with the lens in hand they demonstrated more and more the intimate structure of the fruit, the seed, the various relations which connect the parts of the flower, and the indications which these relations furnish for a natural division. The most delicate forms of organic tissues were exhibited; medicine

[1] Cuvier refers here to the investigation of the fossils in the Paris basin, which he undertook during the years 1804 to 1808: "La singularité des animaux dont je découvrais les ossements à Montmartre me fit désirer de connaître plus en détail la composition géologique des environs de Paris. Mon ami Brongniart s'associa à moi pour ce travail; nous fîmes ensemble et séparément beaucoup de courses. . . . Ces recherches ont donné une face toute nouvelle à la géologie, et ont occasionné toutes celles qu'ont faites ensuite en Angleterre MM. Webster, Buckland, Labêche et autres" (Cuvier, "Mém. sur sa Vie" in Flourens, 'Eloges,' vol. iii. p. 188). This was the beginning of the Science of Palæontology, a term which Cuvier did not use himself (Flourens, 'Travaux de Cuvier,' p. 147). See also Cuvier, 'Recherches sur les Ossemens fossils de Quadrupèdes,' &c., 1st ed., 1812, 3rd ed., 1825, in the Introduction.

and chemistry united their efforts to appreciate in the minutest detail the action of external elements on the living organism.[1] The different combinations of organs, or what we call the different classes, the different genera, were not less studied than general theories. There were no animals, ever so small, the inner parts of which, unveiled by anatomy, did not become known as well as our own. Every organic system was likewise submitted to a special examination. The brain, marking the degree of intellectual power; the teeth, signs of the nature and energy of the digestive forces; the bony system, above all, which is the support of all others, and which determines the connected forms of animals, —all these were followed into the smallest species and into the minutest parts. We see how, after such studies, there could be no more talk of superficial or artificial methods. The old natural history had ceased to rule. It was not that old natural history any more, but a science full of life and youth, armed with quite novel ways and means, which beheld the world reopened by the Peace."[2]

In an earlier passage,[3] speaking of the reopening of academies and schools by the Government of the Revolu-

[1] Compare with this the 'Rapport' of the year 1808, p. 201, &c. The above remarks refer mainly to Bichat. "Bichat a donné à l'anatomie un grand intérêt, par l'opposition de structure et de forme qu'il a développée, entre les organes de la vie animale, c'est-à-dire, du sentiment et du mouvement, et ceux de la vie purement végétative. . . . L'attention particulière donnée par Bichat au tissu et aux fonctions des diverses membranes, et l'analogie qu'il a établie entre celles de parties très éloignées, ont jeté aussi des lumières nouvelles sur l'anatomie, principalement dans ses rapports avec la médecine" ('Rapport,' p. 218).

[2] This refers to the peace which concluded the Napoleonic wars, and re-established the free intercourse of France with the rest of the world.

[3] In the 'Éloge of Fourcroy," of the year 1811 ('Éloges,' vol. ii. p. 40, &c.)

tion, Cuvier remarks: "It was not merely a question of isolated discoveries, but of institutions, which, in assuring the conservation of the sciences, would multiply their progress indefinitely. What was needed was no longer a simple experimenter, master of his subject and his instruments, it was a man obliged to battle against all kinds of obstacles, and to benefit his fellow-citizens, mostly in spite of themselves. The Convention had destroyed academies, colleges, universities; nobody would have dared to ask boldly for their restitution; but soon the effects of their suppression showed themselves in the most susceptible point; the armies were without doctors and surgeons, and these could not be created without schools.[1] But who would believe that time was required to give courage enough to call them schools of medicine. Doctor and surgeon were titles too contrary to equality, apparently because there is no authority over the patient more necessary than that of the doctor; therefore the odd term "schools of health" was used, and there was no question of either examination or diploma for the students. In spite of this, a penetrating glance reveals, in the regulations which were carried, the intentions of him (Fourcroy) who drew them up. The three great schools founded at

[1] See 'Éloges,' vol. i. p. 353. "Cependant les gens qui avaient fait toutes ces suppressions eurent promptement lieu de s'apercevoir que, s'il était à la rigueur superflu d'apprendre toute autre chose, on ne pouvait guère se dispenser d'apprendre la médecine. Toute la France se précipitait aux frontières, et, après des prodiges inouis de dévouement et de valeur, les défenseurs de la patrie ne trouvaient aucun secours pour leurs blessures et pour leurs maladies. On commença donc par l'érection des écoles de médecine cette longue suite de restaurations, que l'établissement de l'université vient de couronner et de lier en un ensemble aussi imposant par l'étendue de son plan que par la vigueur de son organisation." See also 'Rapport,' &c., p. 360.

this epoch,[1] received an abundance of means, of which up to that time there was no idea in France, and which still form the finest ornament of the University."

Similar passages might be collected in which Cuvier enlarges on the influence of war and revolutions, of the Continental blockade and the isolation of the country; on the reconstruction of hospitals and the admission of medical science into the Academy; on the creation of new industries; on the development of the mining and mineral wealth of the country; on the scientific value of colonies and travels, and many other interesting topics. In confining myself more closely to the history of thought and the growth of the modern scientific spirit, I will make some reflections which his remarks force upon us.

32. France has done more than other countries to popularise science.

I have noted above how France more than any other country worked for the popularisation of science, how her polite literature alone during the eighteenth century bears the strong impress of modern scientific ideas; no other country has a Fontenelle, a Voltaire, a Buffon. This peculiarity must be recognised as a very powerful and valuable stimulus to the growth of the scientific spirit. It emanates largely, if not exclusively, from the peculiar position of the old Academy of Science. It must, however, not be forgotten that it was not a popularisation of the kind we witness nowadays.

33. Difference between the literary and the national popularisation.

The class of literature which in our age spreads broadcast the discoveries or ideas of science; the endless number of magazines, reviews, and daily papers; the small treatises, the cheap primers, the compact text-books, did

[1] They were the three "Écoles de Santé" at Paris, Strasbourg, and Montpellier (see Hippeau, 'L'Instruction publique en France,' vol. ii. p. 194).

not then exist.[1] Science was not a subject of general, still less of popular, instruction. It was an occupation of the few, who, privileged by fortune or talent, or gifted with inordinate perseverance, forced their way into the *salons* of society[2] or the rooms of the Academy. The first public course of natural history was opened in Paris by Valmont de Bomare in 1760.[3] Science still stood far out of the reach of the practical man or the poor man; it had not yet become an element of education or an instrument for industry. It was a fashionable pursuit, a luxury of the great, a key that occasionally opened the door of the palace; but it was not a thing of immediate use, except in adding glory and renown to its royal protectors, or to the rare genius which could make new discoveries. Almost the only application made of it was in navigation, and in the construction of instruments connected therewith. This essentially literary—not national—popularisation of science had also its great dangers. No ideas lend themselves to such easy, but likewise to such shallow, generalisations as those of science. Once let out of the hand which uses them, in the strict and cautious manner by which alone they lead to valuable results, they are apt to work mischief. Because the tool is so sharp, the object to which it is applied seems to be

34. Dangers of the merely literary popularisation.

[1] Cuvier, in his 'Rapport,' &c., p. 361, mentions the elementary works published by some of the medical professors at the beginning of the century, but says also that "En Allemagne, surtout, où l'usage des livres élémentaires est plus commun que chez nous, il n'est presque aucune université, dont les professeurs n'en aient publié d'excellens."

[2] See Maury, p. 182, &c. Also Cuvier, 'Rapport,' vol. ii. p. 427 : "En France la réputation des ouvrages dépend, pour l'ordinaire, des femmes et de quelques gens de lettres, qui croient pouvoir juger des sciences positives, parce qu'ils ont combiné quelques idées générales de métaphysique."

[3] See Maury, 'L'ancienne Académie des Sciences,' p. 283.

so easily handled. The correct use of scientific ideas is only learned by patient training, and should be governed by the not easily acquired habit of self-restraint. It is well known how the fundamental notions of a mechanical science, let loose into literature by Fontenelle, by D'Alembert, by Condorcet, or absorbed by Voltaire and Diderot, were expanded into a system of materialistic philosophy in 'L'Homme Machine,' the 'Système de la Nature,' and other works, the extreme views of which the great scientific thinkers could hardly approve of.[1] These hasty but

[1] As a great deal of confusion existed for a long time in European literature as to the exact succession in time of the different works which assisted to spread mechanical views of the world and of life, I put down the main dates:—

Fontenelle (1657-1757) published his Eloges of the great Academicians, in which the principles of the philosophy of Descartes, Leibniz, and Newton were popularly expounded and discussed, from 1700 onward. His 'Pluralité des Mondes' had appeared already in 1686; it had popularised Cartesian ideas.

Voltaire (1694-1778) published his 'Élémens de la Philosophie de Newton' in 1738.

La Mettrie (1709-51) published his 'Histoire naturelle de l'Âme' in 1745, and his 'L'Homme Machine' in 1748.

D'Alembert and Diderot published the first volume of the 'Encyclopédie' in 1751.

Buffon (1707-88) published, 1749, his 'Théorie de la Terre,' being the first portion of the 'Histoire naturelle.'

Holbach (1723-89) published under the name of Mirabaud, 1770, the 'Système de la Nature.'

Of these works, the three which created the greatest popular sensation — *viz.*, Voltaire's 'Élémens,' La Mettrie's 'L'Homme Machine,' and Holbach's 'Système'—were all published in Holland. Voltaire, D'Alembert, and Diderot appear to have approached philosophical problems mainly from the position of Newton's natural philosophy, La Mettrie from the teachings of the great Boerhaave, Holbach principally from a study of chemistry. It is unnecessary to say that none of them had the sanction of their great masters for the applications they made of principles which had been established and used for special scientific purposes. And the same may be said with reference to the influence of Locke, which in almost all the instances mentioned was combined with that of the great naturalists. But this does not belong to the line of thought in which we are interested at present. For the sake of completeness only I mention that Locke's teachings as well as Newton's were made popularly known in France by Voltaire's "Lettres sur les Anglais' (burnt by order of the Parliament of Paris in 1734), whereas Condillac's (1714-80) more systematic treatise, entitled 'Essai sur l'Origine des Connaissances humaines,' appeared in 1746. It is important, in dealing with the extreme materialistic writings which French literature produced between 1745 and 1770, to keep distinct the different origins from which they started, and the different influences which combined to produce them: the mathematical and mechanical principles borrowed from Newton, the physiological and medical emanating from Linnæus and Boerhaave, and the psychological coming from Locke and Shaftesbury. Lange, in his 'History of Materialism' (transl. by Thomas, London, 1880, 3 vols.), was the first to point out clearly the correct chronology and succession of these writings (see especially vol. ii. pp. 49-123), and to dispel the misconceptions which, since the appearance of Hegel's 'Geschichte der Philosophie' in 1833-36, had passed through nearly all historical works published in Germany. From his exhaustive references, it is evident that the extreme views of La Mettrie, Diderot, and Holbach cannot be fathered on any of the great scientists or philosophers, but were an attempt to apply scientific principles to the solution of philosophical, ethical, or religious questions, frequently for practical and political purposes.

brilliant generalisations, expressed frequently in the most perfect language, did no good to the truly scientific cause; they did not spread the genuine scientific spirit. Much of the good done by Fontenelle, by Voltaire, by Buffon, was spoiled or neutralised by premature and ill-founded theories. How much, or how little, they contributed (either directly or by a kind of reaction which set in against them, of which Rousseau may be regarded as the centre) to bring about the Revolution is a matter of much controversy; certain it is that the Revolution broke their sway, and destroyed their immediate influence.[1] To the purely literary the Revolution added

35 The Revolution added the modern practical popularisation of science.

[1] It would probably be more correct to say that these daring attempts to deal with the general problems of knowing and being, with the nature of the soul and the conduct of life, were discarded as premature, and that the followers of Condillac and Locke betook themselves to a more patient study of the facts of the inner life, as the followers of Buffon forsook his brilliant generalisations for the more patient and fruitful study of all the forms of physical nature. And in this respect the Government of the Revolution took a memorable step when it founded on the 3rd brumaire, an iv. (25th October 1795), on a Report of Daunou, based mainly on ideas expounded by Condorcet, the "Académie des Sciences morales et politiques." It was the intention to abandon metaphysical generalisations, and to combine the scientific and historical spirit in the study of mental, moral, and social phenomena, drawing extensively on the assistance of the medical sciences, or a knowledge of human nature in its nor-

something different—*viz.*, the modern practical popularisation of science: it established its educational and its technical importance. Science was to be not an elegant amusement, or a refined luxury, nor even exclusively the serious occupation of the rare genius: it was to be the basis of a national instruction, and the foundation of the greatness and wealth of the nation. The Memoirs of the Academy were cleansed of all dangerous generalisations which might have brought them into touch with political controversy; the language was confined to the measured and concise statement of facts, or to theories capable of mathematical verification and treatment; conjectural matter was carefully excluded, and a standard of scientific excellence, both in matter and form, was raised, to which we still look up with admiration.[1] At the same time, this lofty and dignified spirit enlivened the courses

mal and diseased conditions. This organisation produced, during its short existence of only seven years, some memorable works; but its position was for various reasons secondary only: it was eclipsed by the European renown which the "Académie des Sciences" possessed, owing to its historical antecedents and its brilliant discoveries and the practical usefulness of its labours. But the idea of including ethical and political studies under the term "Science," due probably to Condorcet, was fixed by this organisation, and has in the course of the century acquired increasing influence. From these beginnings we shall have to study its career in another portion of the present work.

[1] According to Cuvier, "la langue naturelle de l'Académie des Sciençes" is "la langue des chiffres" ('Éloges,' vol. i. p. 24); "l'Académie a toujours eu pour principe de ne se rendre qu'à des calculs ou à des expériences positives" (vol. iii. p. 12). Compare also 'Mém. de l'Institut,' vol. vii. p. 77, where he speaks of the method of Newton, showing how little the employment of a principle like that of "vital force" in physiology can be compared with that of gravitation, employed by Newton to explain the movement of the heavenly bodies; again, vol. viii. p. 139, where he refers to the great service rendered by the Academy, "s'il parvenait à diriger les esprits vers des recherches positives, mais longues et pénibles." And vol. ix. p. 61: "On aime toujours à voir se multiplier dans les sciences expérimentales les moyens simples d'arriver à la précision et de se rapprocher des sciences mathématiques," and other passages quoted above, p. 115 and p. 128. See also his remarks on the Philosophy of Nature, 'Rapport,' p. 335.

of lectures delivered in the great schools by the first men of the nation, and became, through them, the habit of a large number of ardent pupils, who were to carry it further into more popular teaching, or into the applications of art and industry.[1] The results of both are well known. We still live, at the end of the century, under their immediate influence. If now we continually appeal to scientific authorities for aid in the solution of practical problems, it is well to remember that nothing helped more to raise science to the eminence of a great social power than the action of the Revolutionary Government in 1793. Whilst it guillotined Lavoisier, Bailly, and Cousin; drove Condorcet to suicide, and others like Vicq-d'Azyr and Dionis du Séjour into premature death;[2] it had to ap-

[1] See Cuvier, "Réflexions sur les Sciences," 1816, in 'Éloges,' &c., vol. i. p. 24, &c.: "Que l'on recherche, ce qu'ont valu à la France depuis vingt ans les inventions pratiques dérivées des découvertes de MM. Berthollet, Chaptal, Vauquelin, Thénard, &c., dans la seule chimie minérale, dans cette branche assez bornée des sciences physiques; l'extraction de la soude, la fabrication de l'alun, du sel ammoniac, des oxydes de plomb, des acides minéraux, toutes substances que nous tirions de l'étranger; l'épuration des fers, la cémentation de l'acier et enfin le développement des arts qui emploient ces matières premières: il est clair que c'est par centaines de millions qu'il faudra calculer." Also, vol. iii. p. 202: "Les applications de la science à la pratique avaient fait de M. Berthollet, lorsque la guerre de la révolution éclata, le chimiste le plus connu du public, après Lavoisier; et il était presque impossible que l'on ne recourût pas à lui au moment où la chimie devint pour la guerre un auxiliaire de première necessité, et lorsqu'il fallut demander à notre sol le salpêtre, la potasse et jusqu'aux matières colorantes; qu'il fallut apprendre à faire en quelques jours toutes les opérations des arts. Chacun se souvient de cette prodigieuse et subite activité qui étonna l'Europe, et arracha des éloges même aux ennemis qu'elle arrêta. M. Berthollet et son ami M. Monge en furent l'âme."

[2] Vicq-d'Azyr (1748-94), the great forerunner of Cuvier in the new science of comparative anatomy, "au sortir d'une de ces parodies sinistres décorées du nom de fête nationale, était saisi d'un mal qui l'enlevait en quelques instants dans le délire de la peur. Dionis du Séjour (1734-94), après deux années d'effroi et de misère, ne trouvait plus assez de force pour goûter les temps moins malheureux amenés par la chute de Robespierre" (Maury, 'Les Académies d'autrefois,' vol. i. p. 332).

peal for its most necessary requirements to the society of scientific authorities, which it professed not to need. "Everything," says the historian of the Academy,[1] "was wanting for the defence of the country—powder, cannons, provisions. The arsenals were empty, steel was no longer imported from abroad, saltpetre came not from India. It was exactly those men whose labours had been proscribed who could give to France what she wanted. Fourcroy, assisted by researches begun by Lavoisier, taught the methods of extracting and refining saltpetre; Guyton de Morveau and Berthollet made known a new method of manufacturing gunpowder, and studied the making of iron and steel; Monge explained the art of casting and boring cannons of brass for land use, and cast-iron cannons for the navy. On the 6th of August 1793 the Convention had again to appeal to the Academy in order to know what advantage it would be to refine as much as possible the coins of the Republic?" In the space of a few years science had become a necessity to society at large.[2] In the Constitution of the regenerated Academies it was placed at the head, as the most important department of knowledge.

[1] Maury, *loc. cit.*, vol. i. p. 329. See also Biot's 'Essai sur l'Histoire générale des Sciences pendant la Révolution française.' Paris, 1803.

[2] The last entry in the record of the "procès-verbaux de l'Académie" before the suspension was a Report by Borda, Laplace, and Lagrange, in answer to a demand of the Convention, dated 19th January 1793, for advice on the new system of weights and measures which the Republic should adopt. And so necessary had the assistance of men of science become to the Government, that even during the suspension, which lasted from the 8th August 1793 till the 22nd August 1795, Lakanal had succeeded in procuring the following decree from the Government of the Convention: "La Convention nationale décrète que les membres de la ci-devant Académie des Sciences continueront de s'assembler dans le lieu ordinaire de leurs séances, pour s'occuper spécialement des objets qui leur auront été ou pourront leur être renvoyés par la Convention nationale" (Maury, *loc. cit.*, p. 331; Aucoc, 'L'Institut de France,' p. ccvii, &c.)

36. Influence of the first Napoleon on science

The influence of the first Napoleon on science is naturally a matter of as much controversy as his merit in almost every branch of administration. The reports[1]

[1] According to a decree of the Government, dated 13th ventôse, an x. (4th March 1802), the Institute, then consisting of three classes—the "Académie des Sciences physiques et mathématiques," the "Académie des Sciences morales et politiques," and the "Académie de Littérature et Beaux-arts"—was ordered to furnish "un tableau de l'état et des progrès des sciences, des lettres et des arts, depuis 1789 jusqu'au 1re vendémiaire an x." This "tableau" was to be divided into three parts according to the three classes of the Institute. These Reports were to be repeated every five years. The first (and only) Reports were not presented before February and March 1808. The Republican Government had then been superseded by the Empire, and by a decree of the 3rd pluviôse, an xi. (23rd January 1803), the Institute had been reorganised. There were now four classes: 1. Des Sciences physiques et mathématiques (corresponding to the old Académie des Sciences). 2. De la langue et de la littérature françaises (corresponding to the old Académie française). 3. D'histoire et de littérature ancienne (corresponding to the "Académie d'Inscriptions et de Belles-lettres"). 4. Des beaux-arts. "On supprima la classe des sciences morales et politiques qui existait dans l'organisation du 3 brumaire, an iv. Ce fut un trait caractéristique de la répugnance du premier Consul pour la discussion des matières politiques et leur enseignement" (Thibaudeau, 'Le Consulat et l'Empire,' Paris, 1835-37, vol. iii. p. 396). Accordingly there were prepared four, or rather five, Reports, he first in two parts by Delambre and Cuvier on the progress of the Mathematical and Physical Sciences; the second by Marie-Joseph Chénier on the progress of Literature; the third by Dacier on the progress of History and Classical Literature; the fourth by Le Breton on Fine Arts. Of these the two Reports of Delambre and Cuvier gave great satisfaction, that of Dacier gave less satisfaction; Chénier, who himself admired the eighteenth-century philosophy, had an embarrassing task to perform, of which, however, he acquitted himself worthily (Thibaudeau, *loc. cit.*, vol. vi. p. 557). The Report of Chénier has been several times reprinted. The new science which was founded by Condillac, Turgot, Condorcet, and others, and which aimed at introducing the truly scientific spirit into psychology, psycho-physical researches, and questions of society and legislation, received no recognition, as it had also lost its representation in the suspended "Académie des Sciences morales et politiques." After the re-establishment of this section of the Institute in 1832, a royal decree of 22nd March 1840 ordered a Report on the progress of the Moral and Political Sciences from 1789 to 1832. The task was so great that it could not be accomplished before the Revolution of 1848, and was therefore abandoned (Aucoc, 'L'Institut de France,' pp. 62 note, 300). Some reference to the subject is contained in the introduction to Chénier's Report, and in the last chapter of Dacier's, which was written by De Gérando. The true history of the new science has been recently written by F. Picavet, 'Les Idéologues,' Paris, 1891.

which Delambre and Cuvier drew up at his request, touching the progress of science during the twenty years which followed the outbreak of the Revolution, have become classical as monuments of the achievements of a great age,[1] and as examples of the best style in which to treat such a subject. Written immediately under his eye, they cannot be considered quite impartial, so far as the tone is concerned in which they refer to his personal favours and protection.[2] There can, however, be no doubt that he recognised scientific merit, and drew many eminent men of science into the service of the Government. The institutions on which he prided himself so much,—the École Normale, the École Polytechnique, and the unfinished scheme of a great centralised Institution of Learning and Education, descending from the heights of the Institute, through the various branches of the higher and secondary into a multitude of primary schools, bearing the name of the "University,"—had either existed, or been planned before him.[3]

[1] Napoleon in discussing at the council meeting the decree which ordered the several reports, said to Regnaud: "Soignez bien cette rédaction, car elle sera examinée par les pédagogues de toute l'Europe" (Thibaudeau, *loc. cit.*, vol. ii. p. 496).

[2] See what Cuvier himself says on this subject (Mémoires, &c., in Flourens, 'Éloges,' vol. iii. p. 187): "Un rapport sur le progrès des sciences devait être présenté aux consuls en fructidor an xi. . . . Ou ne fut prêt qu'à la fin de 1807: ce n'était plus aux consuls mais à l'empereur que l'on avait à présenter le travail. Il le reçut avec un grand appareil dans la séance du conseil d'État. M. Delambre et moi présentâmes le nôtre les premiers; le 3 févr. 1808, accompagnés de Bougainville, président, et des doyens de toutes les sections. La cérémonie fut solennelle; l'empereur fit une belle réponse, qui est imprimée à la fin du rapport. Je sus le lendemain, par M. de Ségur et d'autres conseillers d'État, qu'il avait exprimé une grande satisfaction de mon rapport en particulier: 'Il m'a loué comme j'aime à l'être, dit-il.' Cependant je m'étais borné à l'inviter à imiter Alexandre et à faire tourner sa puissance au profit de l'histoire naturelle."

[3] Regarding the University, see 'Code Universitaire ou Lois, Statuts et Règlemens de l'Université Royale de France, mis en ordre par M. Ambroise Rendu,' Paris, 1835. In the Introduction we read as follows: "Bonaparte passait à Turin. Un jour qu'il parcourait le palais de l'Université fondée en 1771 par Charles Emmanuel III., il se fit représenter les statuts qui régissaient cette institution. Il y vit quelque chose de grand et de fort qui le frappa. . . . Tout ce plan d'éducation établi sur la base antique et impérissable de la foi chrétienne, tout cela lui plut, et il en garda la mémoire jusqu'au sein de ses triomphes en Italie et en Allemagne. Rassasié enfin de gloire militaire, et songeant aux générations futures, après avoir solidement établi l'administration civile, après avoir relevé les autels et promulgué le Code Napoléon, après avoir par différentes lois, substitué les Lycées aux Écoles Centrales, régénéré les Écoles de Médecine, et créé les Écoles de Droit, il voulut fonder aussi pour la France un système entier d'instruction et d'éducation publique. Il se souvenait de l'université de Turin et l'agrandissant comme tout ce qu'il touchait, dans la double proportion de son empire et de son génie, il fit l'Université impériale."

It will therefore always remain a matter of doubt to what extent he originated ideas, or merely adopted those of others before and around him. He favoured the mathematical sciences, and created great prizes for physical, notably electrical, discoveries, partly because these pursuits promised to surround his Government with glory, partly because he recognised their practical importance for the purposes of the state and nation; partly also, because he himself had had a mathematical training.[1] During his

37. Napoleon favoured the mathematical sciences.

[1] Among many references relating to this subject, I select one from Villemain, 'Souvenirs contemporains d'Histoire et de Littérature,' which in the first volume (9me éd., Paris, 1874, p. 137) contains, the description of a visit to the École Normale in 1812, and a discussion with Narbonne, to whom the Emperor had fully expressed his aims regarding education and learning. "L'Empereur n'est inquiet que d'une chose dans le monde, les gens qui parlent, et à leur défaut les gens qui pensent. . . . Il veut, et il me l'a dit vingt fois, que son règne soit signalé par de grands travaux d'esprit, de grands ouvrages littéraires. Être loué comme inspirateur de la science et des arts, être le chef éclatant d'une époque glorieuse pour l'esprit humain, c'est l'idée qui le flatte le plus; c'est ce qu'il a cherché par des Prix Décennaux. . . . Il veut (à l'École Normale) des études fortement classiques, l'antiquité et le siècle de Louis XIV.; puis quelques éléments de sciences mathématiques et plus tard la haute géométrie, qui est, dit-il, le sublime abstrait, comme la grande poésie, la grande éloquence est le sublime sensible." Napoleon said to Narbonne: "J'aime les sciences mathématiques et physiques; chacune d'elles, l'algèbre, la chimie, la botanique, est une belle application partielle de l'esprit humain; les lettres, c'est l'esprit humain lui-même. . . . Aussi, j'ai deux ambitions: élever la France au plus haut degré de la puissance

campaigns in Italy and Germany, and on his expeditions to Egypt and the East, he surrounded himself with some of the greatest scientific authorities, such as Berthollet and Monge. From political as well as personal motives, he discountenanced the once fashionable sensualistic philosophy. This philosophy has now fallen to the second rank, though still represented by eminent thinkers, such as Cabanis, Destutt de Tracy, Daunou and Garat. It was these thinkers of whom Napoleon sneeringly spoke under the designation of "Idéologues."[1]

38. He discountenanced the contemporary representation of philosophy.

After all that has been said by admirers to magnify, and by opponents to minimise, Napoleon's merits in promoting the cause of science, and in spreading the modern scientific spirit, I cannot but recognise that he was, amongst the great heroes and statesmen of his age, the first and foremost, if not the only one, who seemed thoroughly to realise the part which science was destined to play in

guerrière et de la conquête affermie, puis y développer, y exciter tous les travaux de la pensée sur une échelle qu'on n'a pas vue depuis Louis XIV. C'était le but de mes Prix Décennaux qu'on m'a gâtés par de petites intrigues d'*idéologues*, et de couronnements ridicules, comme celui du catéchisme de Saint-Lambert."

[1] A full account of these authors, their influence and their aims, will be found in F. Picavet, 'Les Idéologues, Essai sur l'histoire des idées et des théories scientifiques, philosophiques, religieuses, &c., en France depuis 1789,' Paris, 1891.

Thibaudeau, 'Le Consulat et l'Empire,' gives many details regarding Napoleon's connection with science, with literature, and with the growing industries of France. Among the latter see especially the great efforts made to supersede colonial and foreign goods by home productions. Prizes and encouragements of all sorts were given; technical schools and colleges were established; exhibitions were promoted. Sheep were imported from Spain, sugar was made from raisins and beetroot, saltpetre and soda by chemical processes, the *garance* or madder root and the *kermès* were to take the place of *cochenille*; the *pastel* the place of the imported indigo. That an enormous impetus was thus given to chemistry cannot be denied. (See Thibaudeau, *passim*, and especially vol. v. p. 248, &c.) See also Cuvier's 'Rapport,' &c., for an account of applications of science, especially chemistry, pp. 376-386, and Delambre, 'Rapport,' &c., pp. 326-362.

the immediate future. This part, as we know, it has played both by entirely changing the external face of things, and by running out into endless applications; and we have seen the importance of that statistical spirit of numbering, measuring, and registering, by which alone a survey of complicated phenomena is possible. Of the statistical method Napoleon himself made use on an extensive scale: perhaps he was the first among rulers to do so.[1] That the great leader of men has to recognise not only the inductive philosophy of statistics and averages, but likewise governing ideas of a different class, Napoleon was well aware, and his ultimate failure may be traced to the fact that, however great as a general and as a calculator, his soul had no room for those high, religious, and unselfish motives of which he himself said to Fontanes, that they in the end always decide the fate of nations.[2] Yet he belongs to the small company of great military figures in history—a company which includes Alexander the Great, Cæsar, and Peter the Great

39. He himself made extensive use of the statistical method.

[1] See Delambre, 'Rapport,' &c., p. 222. "Depuis le peu de temps qu'on s'en [*i.e.*, with statistics] occupe en France, elle y a fait les plus grands progrès, au moyen de l'attention particulière et des secours que le Gouvernement françois donne à tous les travaux utiles. Les préfets des départemens ont été invités à recueillir et à transmettre au Ministre de l'intérieur les renseignemens les plus précis sur toutes les questions qui sont du ressort de la statistique."

[2] See 'Œuvres littéraires de Napoléon Bonaparte,' vol. iii. p. 5; Conversation avec Fontanes, Saint Cloud, 19 Sept. 1808: "Fontanes, savez-vous ce que j'admire le plus dans le monde? C'est l'impuissance de la force pour organiser quelque chose. Il n'y a que deux puissances dans le monde: le sabre et l'esprit. J'entends par l'esprit les institutions civiles et religieuses. À la longue, le sabre est toujours battu par l'esprit." Also vol. iv. p. 423: "Les vraies conquêtes, les seules qui ne donnent aucun regret, sont ceux que l'on fait sur l'ignorance. L'occupation la plus honorable comme la plus utile pour les nations, c'est de contribuer à l'extension des idées humaines. La vraie puissance de la République française doit consister désormais à ne pas permettre qu'il existe une seule idée nouvelle, qui ne lui appartienne."

—who have succeeded in permanently inscribing their names in the annals of science beside those of its true and great representatives. Some of the glory of Laplace and Cuvier falls upon him. Except for this Napoleon has scarcely a place in the history of thought. In it those who were Napoleon's servants are rulers and lawgivers; it is they who enlighten our century. They were the first great exponents of the scientific spirit, nursed under the influence of the academic system. This was peculiarly a product of the French mind and culture. It is well to recall in the words of Cuvier what the scientific spirit is. At the end of the report which he presented in the year 1808 he says:[1] "These are the principal physical discoveries which have lighted up our period, and which open the century of Napoleon. What hopes do they not raise! how much does not the general spirit signify, which has brought them about, and which promises so much more for the future! All those hypotheses, all those suppositions, more or less ingenious, which had still so much sway in the first half of the last century, are now discarded by true men of science: they do not even procure for their authors a passing renown. Experiments alone, experiments that are precise, made with weights, measures, and calculation, by comparison of all substances employed and all substances obtained: this to-day is the only legitimate way of reasoning and demonstration. Thus, though the natural sciences escape the application of the calculus, they glory in being subject to the mathematical spirit, and by the wise course which they have invariably adopted, they do not expose them-

40. His scientific glory is mainly derivative.

[1] 'Rapport,' &c., p. 389.

selves to the risk of taking a backward step; all their propositions are established with certainty, and become so many solid foundations for that which remains to be built."[1]

41. Deserved prominence given to French names by Cuvier.

Nor can we look upon the great prominence which Cuvier gives to French names in the course of his survey as unjust or partial. He was well aware of the contributions of other nations: no one has spoken in more generous and correct terms of Priestley and Cavendish, of Banks and Rumford, of Pallas, Werner, and Humboldt. We must admit the correctness of the remark, "that even in those departments where chance has willed that Frenchmen should not make the principal discoveries, the manner in which they have received, examined, and developed them, and followed them out into all their consequences, places their names next to those of the real inventors, and gives them in many ways the right to share in the honour."[2]

In the first decades of this century the home of the scientific spirit was France: for though not born there, it was nevertheless there nursed into full growth and vigour. But it soon set out on its wanderings through

[1] Compare also the "Réflexions sur la marche actuelle des Sciences," being the introduction to the 'Éloges historiques,' vol. i. p. 1, &c.

[2] 'Rapport,' p. 391. It is also remarkable how clearly Cuvier here announces the defects which the teaching of science was still labouring under. Whilst he rightly praises the great Paris institutions, the medical schools, the mathematical, physical, and polytechnic establishments, the new schools of technology and agriculture, as unequalled organisations for higher instruction, he draws attention to the absence of equally efficient elementary schools and to the neglect of those provincial institutions which before that age had already done so much to disseminate knowledge and learning. At the end of our century both France and Great Britain have still only very partially supplied the wants which Cuvier so clearly defines in the beginning.

other lands and nations. At the end of our century—nay, even during the whole of the second half—we find this spirit naturalised in Italy, in Germany, in England, in the north and east of Europe. There is now no science which can be named pre-eminently after one nation. All nations have contributed their share to the cosmopolitan power and influence which science possesses. They have enlarged and deepened the scientific spirit and widened its career. Thus far it has been the growth of the scientific spirit which has occupied us; we must now proceed to study its diffusion, and learn to recognise the peculiar features which Germany and England have on their part contributed. In doing so, we must turn away for a moment from the academic system with which we have been specially occupied.

CHAPTER II.

THE SCIENTIFIC SPIRIT IN GERMANY.

"No Augustan epoch flowered,
No Lorenzo favours showered
Ever German Art upon;
She was not by glory nourished
And her blossom never flourished
In the rays of Royal sun."[1]

Perhaps with more correctness Schiller might, early in the century, have applied these lines to German science than to German art. If art and poetry were only slightly indebted to princely protection, German science was still less so.[2] Leibniz's scientific labours languished while he

[1] Schiller, "Die deutsche Muse."

[2] Astronomy was the only science that enjoyed some little princely favour. William IV., surnamed "the Wise," son of Philip the Magnanimous of Hesse and himself Elector, was an astronomer of some note, and stood in intimate relations with Mercator, Tycho, and other astronomers. In 1561 he built himself an observatory at Cassel and appointed Rothmann to be his "Mathematicus." Frederick II. of Denmark gave Tycho a magnificent observatory, called "Uranienburg," where he laboured from 1576 to 1597, but which was subsequently destroyed. Tycho was then employed by the Emperor Rudolf II., and inaugurated the observatory in Prague (1599-1601); he made Kepler his assistant, and enabled the latter by the use of his observations to find and prove his three celebrated laws ("Astronomia nova," Prague, 1609; "Harmonices mundi," Linz, 1619; "Tabulæ Rudolphinæ," 1627). Full details will be found in Rudolf Wolf, 'Geschichte der Astronomie,' München, 1877, p. 266, &c.

occupied the position of historiographer and diplomatist at the Court of Brunswick,[1] and Tobias Mayer's valuable observations were only published with the aid of English money.[2] But if the German princes did little or nothing directly for the development of science, they indirectly

1. Foundation of German universities.

[1] Leibniz (1646-1716) entered, 1676, the service of John Frederick, Duke of Hanover, as librarian and councillor. The Duke died 1679, and Ernest Augustus, who in 1692 was made Elector of Hanover, succeeded him. Leibniz's time was taken up with diplomatic and legal researches and negotiations referring to the position of the House of Hanover, and the reunion of the Protestant and Roman Catholic Churches; latterly with genealogical and antiquarian studies referring to the history of the House of Brunswick. He wrote the 'Annales imperii occidentis Brunsvicenses,' beginning with the year 768, the date of the accession of Charles the Great, from whom Leibniz proved that the House of Brunswick descended through the Italian House of Este. He carried the history down to the year 1005, closing a few days before his death with the words "quos ex tenebris eruendos aliorum diligentiæ relinquo." The work was not printed till 1843, when G. H. Pertz, the first editor of the celebrated 'Monumenta Germaniæ' founded by the great Stein, published it with an elaborate preface. Of the annoyances to which Leibniz was subjected in the course of his studies, see an account in the correspondence with the Minister von Bernstorff (1705-16), published by Doebner, Hanover, 1882, introduction. See also Guhrauer, 'Leibnitz, eine Biographie,' 2 vols., 2nd ed., Breslau, 1846. Considering the greatness of Leibniz in so many different directions, his motto is noteworthy: "Didici in mathematicis ingenio, in natura experimentis, in legibus divinis humanisque auctoritate, in historia testimoniis nitendum esse."

[2] Tobias Mayer (1723-62), born at Marbach, the birthplace of Schiller, from 1751 Professor of Economics and Mathematics at Göttingen. To use the words of Karsten Niebuhr, "Though he had never seen a big ship, he taught the English how to determine the longitude on the open sea." He competed for the great prize of £20,000 offered in 1713 by the Board of Longitude for a method of determining the longitude at sea within $\frac{1}{2}°$ accurately; smaller prizes being offered for an accuracy of $\frac{2}{3}°$ and $1°$. The prize of £5000, and subsequently of £10,000, was awarded to Harrison in 1758 and 1764 for his chronometers. Euler and Mayer laboured in a different direction at the same subject, by publishing lunar tables and perfecting the lunar theory. After repeated revisions, Mayer sent his tables, 1755, to London, where they were submitted to Bradley, who reported favourably on them. After further corrections, and after also submitting his theory, Mayer's widow received, in 1765, £5000, Euler £3000, and the work was published, 1770, by order of the Board of Longitude, under the title 'Tabulæ motuum solis et lunæ novæ et correctæ, auctore Tob. Mayer: Quibus accedit methodus longitudinum promota eodem auctore.'

furthered her cause most powerfully by founding that great institution of culture, which more than anything else is characteristic of the German mind, in which it has found its most perfect expression, and where it can be most exhaustively studied—the system of the German universities.

2. Development of the universities by the people.

"There is no people," says Mr James Bryce, "which has given so much thought and pains to the development of its university system as the Germans have done—none which has profited so much by the services universities render—none where they play so large a part in the national life."[1] If it is correct to say that this system owed its foundation to the German princes, it is equally true that its development is the work of the German people.[2] It may be doubtful whether, without the

[1] See James Bryce's preface to the English translation of Conrad's valuable book, 'The German Universities for the last Fifty Years,' Glasgow, 1885, p. xiii.

[2] A great deal has been written about the German universities. For the purposes of a History of Thought, I confine myself to a reference to the valuable writings of F. Paulsen, 'Geschichte des gelehrten Unterrichts auf den deutschen Schulen und Universitäten,' Leipzig, 1885, and two essays in the 45th volume of Von Sybel's 'Historische Zeitschrift,' 1881. The succeeding phases of mediæval and modern, of Roman Catholic and Protestant, of the thought of the Church, the Renaissance, the classical and the modern ideals, are all reflected in the foundation and reform of the universities and high schools of Germany and the surrounding countries. The *first* foundations, in imitation of the universities of Paris and of Italy, were Prague 1348, Vienna 1365, Heidelberg 1386, Cologne 1388, Erfurt 1392, Würzburg 1402, Leipsic 1409, Rostock 1419. A *second* epoch—under the influence of the humanistic studies—begins in the middle of the fifteenth century and adds eight new foundations—Greifswald 1456, Freiburg 1457, Trier 1457, Basel 1459, Ingolstadt 1472, Tübingen 1477, Mainz 1477, Wittenberg 1502, Frankfort on the Oder 1506 (Paulsen, 'Geschichte,' p. 14). A *third* epoch begins with the Reformation. The first Protestant university is Marburg, founded by Philip of Hesse, 1524. Melanchthon's influence is everywhere decisive. Tübingen is reconstituted by Duke Ulrich 1535; Leipsic by Duke George 1539. Basel, after three years' suspension, is reopened 1532. Frankfort on the Oder is reopened by Joachim of Brandenburg 1537, who also founds the new University of Königsberg 1541. Greifswald is

individual influence of the former, without the divided interests of the dismembered empire, without the conflicting religious views, the political and personal rivalry of the many states and sovereigns,[1] so many scattered centres of culture and learning would have sprung so early into existence; but it is not doubtful that it is owing to the common interests of the nation, to the uniting tie of the same language, the same thought, and the same aspirations, that these scattered centres have been in course of time united into a great network,[2] a vast organisation for the higher intellectual work of the nation and of mankind. The German nation may pride itself on possessing at the present moment the most

reconstituted on a Protestant foundation 1539; Rostock in 1540-50; Heidelberg by the Elector Frederick II. in 1544. Jena is founded 1558 by John Frederick, Helmstädt by Julius of Brunswick in 1568; Giessen followed in 1607; Rinteln in 1621; Altdorf in 1662. Of the greatest influence on German culture were the Dutch Protestant universities—Leyden 1575, Franeker 1585, Utrecht 1634, Harderwyk 1648; they were for a long time—as formerly the Italian universities—the goal of the young scholar's wanderings (Paulsen, p. 179). They—as well as Geneva—held a similar position to the Scotch universities (see Sir A. Grant, 'Story of the University of Edinburgh,' vol. i. pp. 21, 126, 188, 213, 229, 233, 263, 274, 283, 297, &c., vol. ii. p. 263). A *fourth* epoch begins with the foundation of Halle 1694, the first really modern university (Paulsen, p. 353). The spirit of Bacon and Leibniz, represented by Thomasius, is the leading power; it is not by any means irreligious, since Francke (the so-called "pietist") is as important a factor as Thomasius. German is substituted for Latin. Other universities follow the reform, thus Königsberg 1735, Leipsic, Wittenberg, Helmstädt, Kiel, Tübingen, &c. A *fifth* epoch—the evolution of the ideal of science in the German sense, *Wissenschaft*—begins with the foundation of Göttingen in 1737. Of this more in the text.

[1] Conrad, *loc. cit.*, p. 2: "There is scarcely a stronger bond of connection between the various parts of Germany than that supplied by the universities, and in no other respect have the barriers that separated State from State been so long broken down. . . . The historical development cannot be accurately traced unless the growing extent in which the south German universities are attended by students from the north be kept in view."

[2] See especially Paulsen's remarks referring to the foundation of Göttingen under George II. ('Geschichte des gelehrten Unterrichts,' p. 425).

powerful and best equipped army. But this is only the creation of the present age. With greater pride it may boast of having trained in the course of centuries the largest and most efficient intellectual army, ready at any moment to take up and carry to a successful issue great scientific undertakings demanding the intense thought and labour of a few secluded students, or the combined efforts of a large number of ready workers. This army is scattered through the length and breadth of the land, and even beyond its frontiers in neighbouring countries, wherever universities and high schools are situated.[1] It is not a stationary power, but is continually on the move from south to north, from west to east, to and fro, exchanging and recruiting its forces, bringing heterogeneous elements into close contact, spreading everywhere the seed of new ideas and discoveries, and preparing new land for still more extended cultivation.

[1] The extent of the German university system cannot be estimated by the twenty universities marked on the map attached to the translation of Conrad's book, as these represent only the existing universities of the present German empire; nor yet by the forty-three universities given in the appendix, p. 290, as they contain only some of the Austrian, but none of the Swiss universities; nor even by taking up Ascherson's valuable 'Deutscher Universitäts-Kalender,' which contains the German-speaking universities—thirty-four in number in 1887—but of course does not contain the names of those which have been suppressed. There are also the universities of Denmark, Norway, and Sweden, which have exchanged many important professors with Germany, and those of Holland in older, of Belgium in modern times, which have done the same thing. The Russian universities also were largely organised on German models, though since the reforms of 1863 they aim at a more national character. Brandis founded the University of Athens on German lines in 1837. The Russian University at Kasan, that "ultima musarum Thule," was founded in 1804, and Göttingen supplied its first professors. From there and from the hardly less remote Transylvanian town, Maros Vásárhely, there issued the revolution of our fundamental notions in geometry, and there is reason to believe that both Lobachévsky's and Bolyai's theories are ultimately connected with the speculations of Gauss. See Prof. A. Vasiliev's Address on Lobachévsky, translated by Halsted, p. 5 *sqq.*

It is not my intention to dwell on the history of the German universities, on the gradual growth of the university system; though every stage in that history is interesting and important if we wish to understand the inner working and usefulness of this great organisation. Neither do I wish to do more than just mention, as an equally important subject, the geography of the German universities; how through nearly fifty larger or smaller towns, in the course of six centuries, learning and higher education have been spread over the German-speaking countries of Europe. These figures alone suggest the intricacy of the subject, the many springs, the continual ebb and flow of the rising tides of ideas, the many courses of thought, the many schools of learning, the internal conflicts, the unavoidable friction, the healthy competition and rivalry, the republican spirit, the impossibility of any creeping stagnation of life, the absence of any lengthened tyranny of doctrine, of an oppressive hierarchy, or of idols of opinion and belief. I leave it to my readers to indulge in comparisons easily suggested by these different aspects, to fasten upon the strong and upon the weak points of this great system of the German universities.[1] What I wish to emphasise

3. Geographical distribution of the German universities.

[1] The migration of students as well as of eminent professors from one university to another is one of the most important features of German academic life. Thus we find the imaginative tendencies of the southern intellect represented by Hegel and Schelling in philosophy transplanted into the midst of the encyclopædic and logical sciences of the North, or into the centre of industrial Switzerland in the person of Vischer; the theological criticism of the Tübingen school wandering northward to Marburg and Berlin in Zeller; and the philological criticism of Gottfried Herrmann locating itself in Zürich in his celebrated pupil and biographer Köchly, and in Bavaria through Thiersch. Jacobi came from the lower Rhine to Munich, where also Liebig formed a centre of modern scientific celebrities. Savigny in Berlin and Thibaud in Heidelberg represent the historical and philosophical schools of German jurisprudence. Vienna for a long time was the most celebrated German training-school of practical

very strongly here is the existence in the midst of European life, all through our century, of this vast organisation for intellectual work, this great engine of thought; and to assign to it one of the foremost places among the great agencies with which we shall have to deal.

4. Full development of the German university system.

The beginning of the present century found this great institution of university education in full swing among all the German-speaking nations.[1] The eighteenth century brought it to that state of perfection in which we have been accustomed to see it. In the course of that century it outgrew its earlier and more limited phases of existence, its period of more restricted usefulness; it emancipated itself from Court and personal favouritism, from ecclesias-

medicine and surgery, whereas Berlin concentrated the great representatives of the more recent scientific developments. In the course of the last hundred years no one university has been allowed to retain for any length of time the supremacy in any single branch. The light has quickly been diffused all over the country, when once kindled at one point. How will the future compare in this respect?

[1] This is not quite the case as regards Switzerland. The city of Basel, which before the Reformation was the seat of much learning, the names of Sebastian Brandt, Reuchlin, and Erasmus being intimately connected with it, had a university from 1459. The antagonism to classical and polite literature which characterised a large section of the Reformers (see Paulsen, p. 128 *sqq.*) destroyed many flourishing centres of culture; amongst them the University of Basel, which was suspended in 1529, when the city accepted the Reformation, but reopened three years later in 1532. Geneva, though this is outside of the German-speaking area and presents a culture quite peculiar to itself, had an academy from 1559, with many celebrated professors and numerous students of theology from all countries of Europe. Lausanne, Bern, and Zürich had colleges or high schools in the seventeenth century. But down to the nineteenth century Basel remained the only university in the Continental sense. The reasons why Switzerland developed her university system so late are discussed in Tholuck, 'Das akademische Leben des 17ten Jahrhunderts,' vol. ii. p. 314, &c., where also minute information is given on the several high schools of Switzerland. The question is interesting, seeing that the greatest in many branches of science—such as Bernoulli, Euler, Haller, Cuvier, Steiner—have come from Switzerland, and that by reason of the names of Rousseau and Pestalozzi it has become the centre of modern ideas on education.

tical protection and influence; it acquired through the statutes of governments or special foundations larger and better secured means of subsistence; it substituted the vernacular for the Latin tongue. The circle of studies, though from early times professedly all-embracing, did not become worthily filled up and cultivated with equal and impartial care till the fourth faculty, the *philosophical faculty*, was properly developed. Theology, law, and medicine conduct their studies for practical ends and purposes; the two former especially were frequently liable to be used merely for the ends of the Church or the State; but the philosophical faculty embraces all those studies which aim at establishing truth, be this defined as merely formal or as real, as belonging to method or to knowledge. We can assign a definite date to the firm establishment of the "libertas philosophandi," and the professed introduction of the "libertas docendi" in the university programme[1]—namely, the opening (in 1734) of the University of Göttingen (inaugurated in 1737). "The foundation stone," says Professor Paulsen, "of the academic constitution is the 'libertas docendi.' On this point Von Münchhausen, whom we may call the real founder of the university, and his two advisers, Mosheim, the theologian of Helmstädt, and Böhmer, the jurist of Halle, were agreed. All 'inquisitiones,' so writes the former, choke the powers 'ingeniorum,' and spoil the beginnings of a learned society. He advises above all that the greatest care should be used in the equipment of the theological faculty. Accordingly Münchhausen laid his eye upon men whose teaching led neither to

5. The philosophical faculty.

6. The University of Göttingen.

[1] Paulsen, 'Geschichte des gelehrten Unterrichts,' p. 424, &c.

'Atheismo' nor 'Naturalismo,' who neither attack the 'articulos fundamentales religionis evangelicæ,' nor introduce enthusiasm, nor yet evangelical popedom. Likewise the jurists received full freedom for teaching and for the expression of legal opinions, whereas at Halle, following the common rule, the Prussian interest, at least in matters of public law, was the measure of things. At Göttingen the chief stress was laid on the culture of the essentially modern sciences. In the foremost rank stood the administrative and historico-political branches where Pütter, Achenbach, Schlözer, Gatterer, Heeren, gave to the university her world-wide fame; the mathematical and scientific branches are marked by the brilliant names of Haller, Lichtenberg, Blumenbach, Kästner; the philological branches by Gesner, Heyne, Michaelis. The university met the demand for encyclopædic discourses. Münchhausen arranged in 1756 that a member of each faculty should deliver a public course on the whole field of the sciences taught there; in the philosophical faculty Gesner treated philologico-historical, Kästner physico-mathematical subjects. An 'Index Lectionum' of the year 1737 shows nine professorships: 1. Politics and Morals. 2. History of Literature. 3. History. 4. Elocution and Poetry. 5. Logic and Metaphysics. 6. Oriental Languages. 7. Mathematics and Physics. 8. Administrative Sciences; to which is added, lastly, a professorship of Philosophy without special definition."[1]

It is evident that, owing to their constitution, as well

[1] The original endowment of Göttingen was fixed at 16,000 thalers, equal to £2400. This was more than double the endowment of Halle. (Paulsen, p. 425.)

as to their number, the German universities were destined to become the most powerful organisation for the diffusion of knowledge. Further, they have been in the course of the present century more closely linked with many hundreds of high schools, and with the growing number of technical schools.[1] For both of these they had to train the teaching staff, and from the ranks of these they again largely filled their own chairs. Thus they not only combined in themselves the spirit of research and the profession of teaching, but they infused into the widely scattered teaching staff of many hundreds of

7. Relation of universities and high schools.

[1] The technical schools in Germany and Switzerland are a creation of modern times. We can distinguish three classes. (1) The "Realschule." This stands in a kind of opposition to the "Latin school." The name (according to Paulsen, p. 483) occurs first in Halle, where the archdeacon Semler established in 1706 a mathematical and mechanical "Realschule." J. J. Hecker established at Berlin in 1739 an "economico-mathematical Realschule." The object of these schools was to teach "Realia," to introduce practical rather than learned information. A special development was the "philanthropinism" of Basedow, well known even to English readers from Lewes's Life of Goethe (see vol. i. p. 276, &c.) (2) A second class embraces the "Gewerbeschulen," which may be rendered "Schools of industry." Karl Schmidt ('Geschichte der Pädagogik,' vol. iv. p. 163) calls Beuth the founder of them in Prussia, 1817, and gives the school of Aachen as the first. They form a kind of bifurcation with the higher classes of the Gymnasia (or learned schools). They may be more specially commercial, agricultural, or military. (3) Out of these a third class—answering to the growing demand for the practical application of the higher mathematical sciences—has grown up, named polytechnic schools. The celebrated École Polytechnique of Paris has been the model. The first of this class in Germany was established at Vienna in 1816. Then followed Munich, Hanover, Karlsruhe, Stuttgart, Nürnberg, Augsburg, Darmstadt, Zürich, Aachen, latterly also Berlin (Reichsanstalt) and Brunswick (Carolinum). In many ways they equal the universities in the scientific spirit of their teaching. What is wanting is the philosophical, the historical, the encyclopædic treatment. In this respect they form in their best examples a contrast to the Göttingen programme. To many serious-thinking minds they indicate the gradual dissipation of the German ideal of *Wissenschaft*, the narrowing down of *Wissenschaft* to science in the English and French meaning of the word. Their danger lies in the direction of being contented with practical usefulness, as the danger of the German type of university lay in being contented with erudition.

schools the same habit—almost absent in other countries—of looking upon private study and research as a necessary qualification of the lecturer and teacher. The educational organisation of the combined universities and higher schools has thus become an equally powerful organisation for research, and for increasing knowledge. Wherever the progress of learning and science requires a large amount of detailed study inspired by a few leading ideas, or subservient to some common design and plan, the German universities and higher schools supply a well-trained army of workers, standing under the intellectual generalship of a few great leading minds. Thus it is that no nation in modern times has so many *schools of thought* and learning as Germany, and none can boast of having started and carried through such a large number of gigantic enterprises, requiring the co-operation and collective application of a numerous and well-trained staff.[1] The university system, in one word, not only teaches knowledge, but above all it teaches *research*. This is its pride and the foundation of its fame.

8. The university a training-school of research.

[1] The editions of the ancient classics brought out by Tauchnitz, Weidmann, and Teubner are well known. The collections of the Histories of all countries, begun by Heeren and Ukert and continued in this century by the publishing firm of Salomon Hirzel of Leipsic; the 'Jahresberichte,' started by Berzelius for chemistry, and now separately conducted for all the different sciences; contain summaries of the labours of the whole world systematically arranged. There is the geographical establishment of Petermann at Gotha; not to speak of publications specifically national, such as the 'Monumenta Germaniæ,' as other countries possess similar undertakings. Von Zach was the first to establish a regular international organ for astronomical observations. It was started in 1798, and soon became the "living organ of astronomy," equally appreciated by Lalande and Gauss. This "monthly" was soon succeeded by Schumacher's "weekly," the 'Astronomische Nachrichten.' See Wolf, 'Geschichte der Astronomie,' p. 764, &c. Humboldt's and Gauss's scheme for a network of magnetic observations all over the world was taken up by English men of science.

It is a useful and interesting task to trace intellectual developments and habits to their external causes. The centralisation of the powers and resources of a whole nation into one capital, as was the case in Rome and in Paris, may explain the brilliancy of their literatures; the more scattered and diffused culture of Greece and of Germany is likewise reflected in their many schools of thought and learning; the insular position of England has impressed its advantages and disadvantages upon her history, and has influenced her mental life. These influences have frequently been pointed out and examined. The historian of thought has another and more difficult task to perform. Habits of thought and intellectual qualities never become the property of a large number of persons unless they assume a definite form; through this they become a marketable article which can be communicated and transmitted, and in which those also can participate from whom the deeper motives and higher aims remain hidden. Every school has its watchword, in which its leading thought, its ideal, is embodied. The widely scattered and yet closely connected community of intellectual workers represented by the German university system, which covers with its network of universities and high schools the German-speaking countries of Europe, has during the period of its greatest influence developed its own special ideal, and it has expressed this in a special word—namely, the word *Wissenschaft*. Neither the French nor the English application of the word science[1] corresponds to the use or gives the meaning of the word *Wissenschaft*. This meaning cannot be defined by any

9. The ideal of *Wissenschaft*.

[1] Compare the notes at the beginning of the last chapter, p. 89, &c.

single word in the English language. Expressions such as "student of science" or "science tripos" have a meaning in English, but they would have none if translated into German. In each case the word *Wissenschaft* would require a qualification. An "Académie des Sciences" could not according to German usage exist separately beside an "Académie française" or an "Académie des Inscriptions," for it would include them.[1] Scientific treatment in England means the exact experimental or mathematical treatment of a subject: no one ever calls Bentley[2] or Gibbon[3] a great scientific writer, though in

[1] The two older academies in Paris, the "Académie des Sciences" and the "Académie des Inscriptions et Belles Lettres," covered very nearly the same ground as the modern Berlin "Academie der Wissenschaften und Künste," which is divided into two classes, the "mathematisch-naturwissenschaftliche" and the "philosophisch-historische Classe," the two sides being equally comprised under the term *Wissenschaften*. A similar division exists in the learned societies of Vienna, Leipsic, Munich, and Göttingen.

[2] Richard Bentley (1662-1742), popularly known in England mainly through his Boyle Lectures, his controversy about the Epistles of Phalaris, and his thirty years' feud as Master of Trinity College, Cambridge, with the dons of his college, but hardly known "as the first, perhaps the only, Englishman who can be ranked with the great heroes of classical learning" (Mark Pattison, 'Ency. Brit.'), was from the first recognised as a consummate genius by the scholars of Germany, by Grævius and Spanheim, who welcomed him as "novum et lucidum Britanniæ sidus," as "splendidissimum Britanniæ lumen." The many beginnings which he had laid for subsequent critical research among the ancient classical authors were taken up abroad by men like Heyne, Reiz, F. A. Wolf, Gottfried Hermann, and Friedrich Ritschl, in whose hands they have developed into a special school of philology, counting probably over a hundred representatives, many of whom have openly avowed their indebtedness to Bentley. (See Köchly, 'Gottfried Hermann,' Heidelberg, 1874, pp. 115 *sqq.*, 142, 189. Ribbeck, 'Friedr. Wilh. Ritschl,' 2 vols., Leipzig, 1879 and 1881, vol. i. p. 229; vol. ii. pp. 111, 176, &c., 418, 429.)

[3] Gibbon (1737-94) gave a new impetus to the study of the history of Roman law through the celebrated 44th chapter of his 'Decline and Fall of the Roman Empire.' It was translated by Professor Hugo of Göttingen and Professor Warnkönig of Liège, and has been used as the text-book on Civil Law in some of the foreign universities. See Smith's edition of Gibbon's History with the Notes of Milman and Guizot, chap. xliv., note. Herder, Savigny, and Niebuhr stand all under the immediate influence of Gibbon, and Lessing saw

Germany each stands at the head, and forms the beginning, of a definite scientific movement. The distinction between scientific and philosophical thought which I have explained in the Introduction would be unintelligible if science were translated simply by *Wissenschaft;* the word *Wissenschaft* is not opposed to, but embraces, the word philosophy: Fichte, whose whole doctrine was, according to French and English ideas, almost the reverse of scientific, uses the word *Wissenschaftslehre* to denote and characterise his system.[1] In fact the German word for science has a much wider meaning than *science* has in French or English; it applies alike to all the studies which are

10. Has been developed under the German university system.

cultivated under the roof of "alma mater"; it is an idea specially evolved out of the German university system, where theology, jurisprudence, medicine, and the special philosophical studies are all held to be treated "scientifically," and to form together the universal, all-embracing edifice of human knowledge.[2] Such an

in him kindred tendencies, though in a different direction (see Wattenbach, 'Zum Andenken Lessing's,' p. 23).

[1] Fichte (1762-1814) begins his first philosophical work, published in 1794, with the words, "Philosophy is a science," and he then proceeds to give to his philosophy the term *Wissenschaftslehre*, or general doctrine or theory of science. A further definition which he gives is as follows: "A science has a systematic form; all propositions in it hang together in one single fundamental proposition, and are united by it into a whole." It is evident that whoever approached Fichte's writings with the ideal of science, as it was established by the labours of Lavoisier and the great French academicians, would not accept these first sentences of Fichte's book. He would admit that the sciences as cultivated by the great Frenchmen had a unity of method, the exact method, the method of observation, measurement, and calculation, but not necessarily a unity of system, or a highest all-embracing proposition. It is evident that science means to Fichte something more than it meant to the Académie des Sciences: it meant *Wissenschaft*, not merely methodical, but systematic, unified knowledge.

[2] It would be an interesting task to trace in German literature from the time of Leibniz the gradual evolution of the idea of *Wissenschaft*, to see how the word has grown in pregnancy and significance till it became firmly estab-

idea, the use of such a term, could only be born and developed where the different faculties, the various branches of knowledge, lived habitually, for many ages, under the same roof, coming into continual contact, and learning to regard each other as members of one family, as integral parts of one whole. The German university

lished as denoting a moral as much as an intellectual ideal, which it was the duty of the German university to uphold and to realise. Such an investigation would have to show how the encyclopædic view is represented by Leibniz, how Winckelmann applied the term to the studies of antiquity, how Lessing taught method and clearness, how Herder widened and deepened the view, extending it to the elemental forces as well as to the finished forms of human culture, how it was finally raised as the standard of German university teaching by F. A. Wolf and W. von Humboldt, finding an eloquent exposition in Fichte's lectures on the "Nature of the Scholar" ('Vorlesungen über das Wesen des Gelehrten,' Erlangen, 1805), and a practical realisation in the foundation of the University of Berlin in 1809, during the period of Germany's greatest degradation. The following words of Fichte have reverberated in the soul of many a German scholar to whom Fichte's philosophy was unknown or distasteful, and this same spirit has leavened and united studies which stand apparently in no connection with each other. "The scholar" (and specifically the teacher of scholars) "shows his respect for science [*Wissenschaft*] as such and because it is science, for science generally as one and the same divine Idea in all the various branches and forms in which it appears." Of one who may be seduced into overestimating his own branch, Fichte says: "It becomes evident that he has never conceived science as One, that he has not comprehended his own branch as coming out of this One, that he thus does not himself love his branch as science but only as a trade; this love of a trade may otherwise be quite laudable, but in science it excludes at once from the name of a scholar. . . . In the academic teacher science is to speak, not the teacher himself," he is to speak to "his hearers not as his hearers but as future servants of science," he is to represent the dignity of science to coming generations (Fichte, Werke, vol. vi. p. 436, &c.) I have myself heard expressions similiar to these from the mouth of one who represented what we should now consider the very opposite phase of nineteenth-century thought, from one of the earliest representatives in Germany of exact research, Wilhelm Weber of Göttingen. Driven into a corner by the questionings of devoted friends as to his own discoveries and contributions, which he was modestly fond of tracing to Gauss, and unable to deny his own part, he would warmly exclaim, "But is it not possible that science could do something herself?" Professor Adamson has pointed out ('Fichte,' in "Philos. Classics," p. 79) how the fundamental idea in these writings of Fichte has been made familiar to English readers through the teaching of England's greatest modern moralist, Carlyle.

system has the merit of having elaborated the widest conception of science, of having fixed the highest and most general scientific standards. Opposed to science is that which is unscientific, dilettante, popular; that which is not a vocation, but a handicraft; that which grows and lives outside of the great university system, including in this the innumerable learned schools which form its base, and the academy which forms its summit.

11. In France and England "Science" means "Exact Science."

What France and England have elaborated and termed Science, is called in Germany Exact Science; but it is opposed to the German ideal of science to hold that the exact method is the only method which deserves to be called scientific.[1]

[1] This is perhaps not quite correct. No doubt the term "exact Sciences" is used frequently during the last half-century to denote the mathematical and experimental sciences; very much in the same sense as we see them defined by Cuvier in the beginning of the century, and described as the ground covered by the labours of the "Académie des Sciences." There exists, however, in Germany another school of thought, very influential throughout this century, and one that has exerted a very wide and wholesome influence, which stands in no connection whatever with the mathematical sciences, though it applies the word "exact" to its methods and researches. This is the school which maintains that the real introduction to the study of antiquity lies in a knowledge of the ancient, preeminently the classical, languages, as exact and precise as any mathematical knowledge could be, and sees in an acquisition of such precise knowledge the training necessary for success in philological and historical research, just as familiarity with mathematical formulæ and measuring instruments has long been considered quite indispensable training to success in the natural sciences. Of this view Gottfried Hermann may be considered as a somewhat one-sided, Friedrich Ritschl as a more profound and far-seeing, but equally energetic representative. It is Ritschl who was the most influential. Without at present entering into the controversies which existed between what were termed the "Sprachphilologen" and the "Sach-philologen," I desire here to refer to the fact that such very different representatives of thought as Fichte, Weber, and Ritschl, than whom no men could be more dissimilar in cast of mind, all find their ideal expressed in the word *Wissenschaft*. I have quoted Fichte, the speculative generaliser, and Weber, the exact mathematical physicist. I will add what Ritschl, the critical philologist, says. He trusted, as his biographer reports, "in the indestructible magnetic force of

Before the methods of exact science were introduced into Germany under English and French influences, the Germans possessed many scientific methods. There was the science of philosophical criticism, established by Kant; the science of historical criticism, of Biblical criticism; the science of philology: all these professed to have methods as definite, aims as lofty, and a style as pure, as the exact sciences brought with them.

At present a tendency of thought may exist in Germany, akin to the positive philosophy in France and England, which aims at introducing the methods of the natural sciences so as to cover the whole ground of research, and to allow of no other methods. Should it succeed, it will destroy the essential features of the German university system, and with it the ideal of *Wissenschaft* as it has existed in all the leading minds of Germany during the last hundred years.

I intend to come back to this subject later on, and to define more clearly what the German ideal of science —what *Wissenschaft*—is. That which we are occupied with at present is the diffusion of the scientific spirit, in the narrower sense, as it was firmly established in France through the great mathematicians and scientists at the

the studies of classical antiquity"; he maintained that philology, as science, not the barren training of a pedagogic seminary, is the only right thing for future masters. "The good teacher must, even for teaching purposes, have and know, both in quantity and quality, more than he requires for immediate progress; the portion he requires for immediate communication, for practical teaching purposes, must be delivered out of the fulness and the depth of knowledge; it must, even in its circumscribed nature, contain the germs of further mental development. Such depth, such fructifying power, comes only from science" (*Wissenschaft*). See Ribbeck, 'Leben Ritschl's,' vol. ii. p. 277. And as every mode of thought, if clearly felt and active, finds its expression in language, so Ritschl was fond of characterising his scientific method by the word ἀκρίβεια.

beginning of this century, as it is summed up in their works and in the Memoirs of the Institute. What reception did it find in Germany? How has it thriven under the German university system? These are the questions which interest us at present.

12. Reception of Exact Science in Germany.

The general recognition of the purely scientific studies conducted on a large scale by the French Academy of Science, as an integral portion of the German university syllabus, belongs to the beginning of the present century. During the first forty years of the century complaints were continually heard that some of the most important sciences were not worthily represented.[1] The eighteenth

[1] One of the latest instances of such complaint is to be found in J. Liebig's paper "On the state of Chemistry in Austria" ('Annalen der Pharmacie,' 1838, vol. xxv. p. 339). This was followed by the highly interesting pamphlet 'On the state of Chemistry in Prussia' (Braunschweig, 1840). According to the eminent author, chemistry was the science which was the latest to attain a worthy domicile and an independent footing in the great universities of Germany. Mathematical physics had a centre at Königsberg, physiology had been established as an independent science at Berlin through the appointment of Johannes Müller in 1833, chemistry was still only taught in Prussia in connection with other branches of science, with medicine, with technology, with mineralogy. There were no chemical laboratories to be found in Prussia. Men like Rose, Rammelsberg, Mitscherlich, received none or only the scantiest support in their practical courses of chemistry. It is interesting to note how Liebig, whilst pointing to the enormous importance which chemistry possesses from an economic and political point of view by reason of its working great changes and revolutions, industrial and other, insists on the necessity of teaching chemistry scientifically, and not with an immediate practical bias. In this respect he is as much a representative of the scientific spirit in the wider sense as the great men mentioned in the note to p. 171. The following passage (p. 39) may still be read with interest and profit: "I have found among all who frequent this laboratory [Giessen] for technical purposes a prominent inclination to occupy themselves with applied chemistry. They usually follow hesitatingly and with some suspicion my advice to leave alone all this time-absorbing drudgery, and simply to become acquainted with the necessary ways and means of solving purely scientific questions. By following this advice their minds learn easily and quickly how to find the best means; they themselves adapt them to circumstances and modify them; all operations, all analyses, which serve to ascertain a certain state, which must be made in order to find the conditions

century produced in Germany men of great scientific importance; but their position was irregular and uncertain, and they undoubtedly do not wholly or exclusively belong to the history of the university system. Leibniz, Euler, Haller, Werner, Markgraf, Tobias Mayer, Lambert, and Humboldt are all intimately connected with the growth of modern science: their position and sphere of action were in each case different.[1] Leibniz was a courtier, Euler an

for the solution of the problem, have a definite sense; each of them possesses a certain charm which dispels fatigue, and if the question is really answered, then they know the ways and means of attaining similar ends. I know many who are now at the head of soda-, vitriol-, sugar-factories, of colour-works and other establishments. Without ever having had anything to do with them beforehand, they were in the first half-hour acquainted with the processes, the second already brought a number of appropriate improvements, &c., &c." Similarly Helmholtz in 1862 ('Reden,' vol. i. p. 142): "He who in the cultivation of the sciences aims at immediate practical usefulness, may be pretty sure that he will miss his aim. Science [*Wissenschaft*] can aspire only to a perfect knowledge and a complete understanding of the sway of physical and mental forces. The individual worker must find his reward in the joy over new discoveries, as new victories of mind over matter, in the æsthetical beauty which an orderly display of knowledge affords, &c., &c." How little do our modern colleges of science correspond with this view of *Wissenschaft!*

[1] On Leibniz (1646-1716), see p. 158; Werner (1750-1817), p. 118; and Tobias Mayer (1723-62), p. 158. A. von Humboldt (1769-1859) is well known to English readers. Leonhard Euler (1707-83), a native of Basel, passed the greater part of his life at St Petersburg as a member of the Academy, a portion of it (1741-66) as an Academician at Berlin. He has been termed the father of pure mathematics, inasmuch as he freed mathematical analysis from geometrical conceptions, established the notion of function or mathematical dependence, and did much to make the theory of numbers an independent branch of science. His memoirs are said to number nearly a thousand; his works, if all printed, would fill 60 to 80 quartos (see Hankel, 'Die Entwicklung der Mathematik,' Tübingen, 1884, p. 12). Andreas Sigismund Markgraf (1709-82) was born and lived at Berlin, a member of the Academy. On his various chemical researches see Kopp, 'Geschichte der Chemie,' vol. i. p. 208. Albrecht von Haller (1708-77) was a native of Bern. He was, next to Leibniz, perhaps the most encyclopædic mind of modern times, equally celebrated as botanist, physiologist, and poet. He has been termed the father of physiology. Brought up under the celebrated Boerhaave, he accepted a chair at the newly founded University of Göttingen in 1736, and taught there for seventeen years anatomy, botany, medicine, and surgery.

academician, Werner the head of a great mining school, Humboldt a traveller, Markgraf a private gentleman. Haller, indeed, shone as a great light in the University of Göttingen, where he did more than any other to place scientific studies on a level with classical ones, and to create for them a permanent abode within the pale of "alma mater." He founded in 1751, in close connection with the university, the *Göttingen Society*, which from 1753 published the celebrated 'Göttinger Gelehrte Anzeigen.'[1] Tobias Mayer and Lambert[2] can hardly be said to have got much help either from the university, to which the former belonged, or from the Academy, of which the latter was a member; their celebrity rests on works produced by private and unaided effort. Humboldt also depended upon his personal means and upon his connection with the Paris Academy, and only attained late in life, and in the course of the present century, his eminent position as the head and patron of German science. Von Zach and Olbers, who together with Tobias Mayer and Lambert raised German astronomy during the eighteenth century to the level of English and French science, stood outside the university system. Von Zach was indebted to personal connections, and ultimately to Duke Ernest II. of Gotha, for the position which

[1] The 'Göttinger Gelehrte Anzeigen' had existed since 1739.

[2] Joh. Heinrich Lambert (1728-77), a very extraordinary man, was a native of Mühlhausen, Alsace, which then belonged to Switzerland. He was received as a member of the Berlin Academy, and associated there with Euler and Lagrange. He is celebrated through his 'Photometry' (1760) and 'Pyrometry' (1779), his equation referring to the orbits of comets, employed by Olbers in his method for calculating them (Weimar, 1797, republished by Encke, 1847), and his prophetic prediction of the proper motion of the sun (in his Cosmological Letters, 1761). This motion was actually calculated by Sir William Herschel in his paper "On the proper Motion of the Sun and Solar System" ('Philos. Trans.,' 1783).

he held as a kind of corresponding centre of European astronomy, and as the leader of a large school of German astronomers of this century.[1] Olbers was a practising physician at Bremen,[2] where he followed astronomical studies as a recreation, making himself eminent by great services to science, among them by his method of calculating the orbit of a comet: as the greatest of his services he counted the fact of having discovered, trained, and appreciated the rising genius of Bessel.[3]

[1] Franz Xaver von Zach (1754-1832) was a native of Pesth. After having served in the Austrian artillery, and taken to astronomy as a favourite study, he spent some time in Paris and London, and became acquainted with Lalande, Laplace, Herschel, Maskelyne, Ramsden, and others. He was engaged by Duke Ernest II. of Gotha in 1786 to erect an observatory on the Seeberg near Gotha. This was completed in 1791. Here he trained a number of younger astronomers, and was the first to establish and maintain a periodical specially devoted to astronomy. It was first (1798) published under the title 'Geographische Ephemeriden,' then (1800-13) as 'Monatliche Correspondenz zur Beförderung der Erd- und Himmelskunde.' Lalande and Gauss both testified to the usefulness of this international publication, without which Piazzi's discovery (see p. 182, note 1) would probably have been lost. See Wolf, 'Gesch. d. Astronomie,' p. 764.

[2] Heinr. Wilh. Mat. Olbers (1758-1840) was born near Bremen. He followed astronomy as a private study. He is mainly known by his rediscovery of the first of the smaller planets (see p. 182, note 1), by his theory, once generally accepted, of the origin of the smaller planets through the disruption of a primitive large planet, and by his 'Abhandlung über die leichteste und bequemste Methode die Bahn eines Cometen aus einigen Beobachtungen zu berechnen' (1797). In this work, by using Lambert's equation, he succeeded in perfecting the methods of Newton and his successors so as actually to calculate the elements of several comets. This method is still in general use (see Wolf, *loc. cit.*, p. 519).

[3] Friedr. Wilh. Bessel (1784-1846) attracted the attention of Olbers by his mathematical abilities whilst employed as clerk in a shipping office at Bremen. If Tobias Mayer's lunar tables were remunerated and published with English money, Germany repaid the debt by the industry of Bessel, who calculated and reduced the observations made by Bradley (1692-1762, Astronomer Royal from 1742) at Greenwich during the years 1750 to 1761. They had been neglected and remained unpublished till 1798, when Olbers induced Bessel to make them useful to science. This he did by calculating from them some of the most important and fundamental data of astronomy. After many years of labour he brought out his 'Fundamenta Astronomiæ pro A. 1755 deducta ex observationibus viri incomparabilis James

13. Science not yet domiciled at the German universities during the eighteenth century.

The general impression we receive from a perusal of the histories of science and learning in Germany at the close of the eighteenth century is, that the university system had, so far as philosophical and classical studies were concerned, attained almost to the eminence which it has held during this century, but that it had not—with the exception perhaps of Göttingen—received into its pale the modern spirit of exact research, such as it had been developed by the great French Academicians. Eminent students of science lived outside of the universities, belonging wholly or largely to the international Republic which had its centre in Paris, exerting little influence on higher German education through the universities, and hardly any on German literature, which had meanwhile ripened into the age of Classicism. This scattered condition of German science gave it on the one side a character which was foreign to the general tendencies of German thought, since this had come under the excessive influence of the speculative spirit without that wholesome check which exact research has always exerted.[1]

Bradley in specula astronomica Grenoviænsi per A. 1750-62 instituti' (1818). By his determination (1838-40) of the parallax of the star 61 Cygni he made the first accurate calculation of the distance of a fixed star, which he computed at 12 billion astronomical miles.

[1] It was the age of the *Naturphilosophie*, which, through the influence of Schelling in the south and Hegel in the north of Germany, filled the chairs in the universities, and penetrated into the learned societies. This philosophy of nature had the effect of frequently replacing induction by speculation, the patient work of the calculator, the observer, the experimenter, and the dissector by general theories, such as, applied to literary, historical, and poetical subjects, had acquired a certain importance, and a semblance of veracity and usefulness. In France the whole spirit of the Academy of Sciences opposed this form of learning. Cuvier denounced it or regarded it with suspicion, in England it remained unknown, and in Germany itself individual great minds opposed it, or did their work outside of its influence. Such were notably A. von Humboldt and Gauss. Younger men, such as Liebig and Joh. Müller, came temporarily under its influence. As regards its harmful effect on the natural and medical sciences, the popular addresses of Helmholtz and Du Bois-Reymond may be consulted. Its philosophical value will frequently occupy us in later chapters of this work. Its period can be approximately fixed by the publication in 1797 of Schelling's 'Ideen zu einer Philosophie der Natur.' The death of Hegel in 1831, and Humboldt's Berlin lectures during the years 1827 and 1828, may be considered as marking approximately the end of the generation which came under the one-sided influence of the *Naturphilosophie*. We shall have ample occasion later on to notice how many valuable leading ideas connected with this phase of thought were temporarily abandoned and have since come prominently before the scientific world. The year 1830 marked the victory of Cuvier's ideas over those of his great contemporary Geoffroy St-Hilaire in the French Academy, and with it the temporary defeat of the valuable suggestions contained in the writings of Lamarck and Goethe.

On the other side, we find in the wide domain of general literature valuable beginnings and foreshadowings of later scientific thought, as in Georg Forster[1] and in

[1] Georg Forster (1753-94) was one of those unique men in the history of literature and science who combine the artistic with the scientific spirit, promoting equally the interests of poetry and of exact knowledge by a loving study of Nature, leading to new views of art as well as to deeper conceptions in science. He may be classed with White of Selborne and other naturalists of England among the small number of those who quietly and unostentatiously prepared the healthier forms of Naturalism which permeate the poetical and scientific thought of our century, culminating in the great names of Wordsworth and Goethe, of Humboldt and Darwin, of Wallace and Haeckel. His life presented many interesting and some unhappy episodes; it introduces us into the political aspirations of the early French Revolution, to which he sacrificed himself. It has been written by Moleschott, the naturalist, by Heinrich König, the novelist ('G. Forster in Haus und Welt,' Leipzig, 1858, 2 vols.), by Klein ('Georg Forster in Mainz'). Fr. Schlegel ('Charakteristiken und Kritiken,' vol. i.), Gervinus (Introduction to the 7th vol. of 'Georg Forster's Werke'), and Hettner ('Literatur des 18ten Jahrhunderts,' vol. iii.) have written appreciative essays on him. A. von Humboldt calls him his master ('Kosmos,' vol. i. p. 345), and Herder (Preface to Georg Forster's translation of 'Sakuntala') prophesies his lasting fame against the opinion of his less appreciative contemporaries. He has a place in the classical literature both of England and Germany through his beautiful description of Captain Cook's second voyage round the world — his father, Joh. Reinhold Forster, having been selected as the naturalist on that voyage (London, 1777, 2 vols. 4to), German edition, 1779. Richard Garnett has said of him: "His account of Cook's voyage is almost the first example of the glowing yet faithful description of natural phenomena which has since made a knowledge of them the common property of the educated world. . . . As an author he stands very high; he is almost the first

Goethe;[1] but they could hardly be encouraged and developed sufficiently without that strict training which is acquired through the routine of the class-room, or under the eye of a recognised authority.

14. Scientific periodicals.

The want of academic union and organisation, and the scattered situation of the many small centres of learning and culture in Germany, led, however, to the early development of those scientific periodicals which form such a characteristic feature in German literature. They were the medium for the exchange of ideas, and the collecting-ground for researches, in an age when exact science was not systematically taught at the Universities, and when such researches otherwise would have run the risk of being lost in obscurity or oblivion.

At the end of the eighteenth century Germany,

and almost the best of that valuable class of writers who have made science and art familiar by representing them in their essential spirit, unencumbered with technical details" ('Ency. Brit.,' vol. ix. p. 419). Forster lived in the period of transition from the thought of the eighteenth century to that of the nineteenth, and a study of his Life, Works, and Correspondence is a very good introduction to nearly all the great problems which then, especially on the Continent, troubled the minds of the greatest men. If he may be accused of want of patriotism, he is certainly to be admired for his freedom from national narrow-mindedness.

[1] It has taken nearly a century before the real value of Goethe's scientific ideas has been correctly gauged. His non-academic surroundings, his unscientific style, his antagonism to Newton, his mission as a poet—supposed in those days to be less realistic than we have since become accustomed to consider it—all these circumstances contributed to the result that Goethe's scientific writings were not taken *au sérieux* by the naturalists of his age. Then came a period when men of science began to sift the wheat from the chaff; but even they have only tardily recognised that, more than in special discoveries or suggestions, his greatness lies in that general conception of Nature which was so foreign to his age, and which nevertheless is becoming more and more familiar and necessary to ours. See especially Helmholtz's valuable essays on Goethe as naturalist from the years 1853 and 1892 ('Vorträge,' vol. i., and address delivered at the meeting of the Goethe Society at Weimar, 1892), and the remarkable progress of his own views on this subject contained therein. We shall have ample opportunity of reverting to this subject.

though not by its universities, was already an important power in the Republic of exact science which then had its centre in Paris. Just at the beginning of the nineteenth century two events happened which foreboded for the highest branches of the mathematical sciences a revival of the glory which in this department Kepler and Leibniz had already given to their country. These two events are both coupled with the name of Carl Friedrich Gauss. They added greatly to the reputation of the University of Göttingen, with which this remarkable man was connected for half a century.[1] The *first* was the publication of the 'Disquisitiones Arithmeticæ' in Latin in 1801—a work by which Gauss placed himself on a level with the great mathematicians, Euler, Lagrange, and Legendre.[2] The

15. Gauss's mathematical researches.

[1] Carl Friedrich Gauss (1777-1855), a native of Brunswick, called by Laplace the first mathematician of Europe, may be considered as the first and foremost representative of the modern mathematical school, of which we shall have to treat later on. Unlike most of the great mathematicians of the Continent, he was self-taught, and followed in his earliest works quite independent lines of thought; resembling in this the great isolated thinkers of Britain whose ideas take a generation or more to penetrate into the text-books of the school. Gauss had the highest opinion of the dignity of pure science, and it almost appears as if, among the moderns, only Newton had come up to his ideal. For him alone he reserves the adjective "summus," and he adopts his synthetic and classical methods of exposition, removing, as has been said, the scaffoldings by the aid of which he had erected his monumental works. Gauss trained few mathematicians; but among the few who penetrated the secret of his ideas are such original thinkers as the Hungarian Bolyai (1775-1856), the geometers Möbius (1790-1868) and Von Staudt (1798-1867), who all mark quite independent lines of research. On Gauss see Sartorius, 'Gauss zum Gedächtniss,' Leipzig, 1856; Hänselmann, 'K. F. Gauss,' Leipzig, 1878; E. Schering, 'C. F. Gauss,' Göttingen, 1887.

[2] It appears that Gauss, to whom the arithmetical discoveries of Fermat and the proofs of Euler, Lagrange, and Legendre remained for a long time unknown (see his Works, edited by Schering, vol. i. p. 6; vol. ii. p. 444), had independently, in his eighteenth year, as a student at Göttingen, already arrived at a great number of propositions referring to the properties of numbers, and had then also found methods of geometrically constructing the regular polygon of seventeen sides.

second was the invention of a new and shorter method of calculating the orbit of a planet from a limited number of contiguous observations.[1] This method was communi-

The latter was the first addition made after 2000 years to the knowledge of this matter possessed by the ancients. (See 'Disquis. Arithm.,' sec. 365: "Magnopere sane est mirandum, quod, quum jam Euclidis temporibus circuli divisibilitas geometrica in tres et quinque partes nota fuerit, nihil his inventis intervallo 2000 annorum adjectum sit," &c.; and his manuscript note to this passage, given by Schering, vol. i. p. 176: "Circulum in 17 partes divisibilem esse geometrice, deteximus 1796, Mart. 30.") It is probably owing to the independent manner in which Gauss approached the subject that he early found the necessity of treating subjects of higher arithmetic (*i.e.*, of the theory of numbers or "discrete magnitudes" as distinguished from algebra, which is the theory of "continuous magnitudes") by an independent method, for which he invented a language and an algorithm. He thus raised this part of mathematics into an independent science, on which the 'Disquisitiones Arithmeticæ' is the first elaborate and systematic treatise. Legendre's 'Traité des nombres' (1799) is a complete thesaurus of all that was at that time known and of what was added by him, but it does not attempt to establish the science on a new basis.

[1] On the 1st January 1801 Piazzi at Palermo had found a movable star of 8th magnitude, RA. 57° 47′, ND. 16° 8′, which he announced to Bode at Berlin as a comet on the 24th January; but a few days later he concluded it must be a planet, and named it "Ceres Ferdinandea." No one besides Piazzi could find the star, but several astronomers, Piazzi himself, Olbers at Bremen, and Burckhardt at Paris, tried to calculate the orbit from the observations of the discoverer, which were contained within only 9 degrees. The attempt to do so under the supposition of either a circular or a parabolic or an elliptic orbit failed, and Olbers expressed the fear that with the circular or elliptic elements which had been published in Zach's periodical, it might prove impossible to find the star when it should again become visible. Very near the expected time, as late as the beginning of December, Gauss communicated his elements to Von Zach, who published them at once, recommending astronomers to follow Dr Gauss's figures and look 6° to 7° more eastward than the positions of Burckhardt, Piazzi, and Olbers indicated. And actually on the 7th December 1801 Zach himself, and on the 1st January 1802 Olbers, succeeded in finding the star, "like a grain of sand on the sea-shore," very near the positions calculated by Gauss. These results, followed soon by the discovery of other planets by Olbers and Harding, gave a great impetus to the study of astronomy. Gauss's methods were published *in extenso* in the now celebrated 'Theoria motus corporum cœlestium' in 1809. Two problems are herein treated in a novel and complete manner. The first was to calculate by a simple and accurate method from the necessary number of observations the orbit of a planet or comet on the assumption of Newton's law of gravitation, but without any other special conditions.

cated to Von Zach in the course of the year 1801, and enabled him and Olbers to rediscover the first of the small planets, Ceres, which Piazzi had observed on the 1st of January 1801 at Palermo, and afterwards lost as it approached the region of the sun's light. Through this Gauss placed himself on a level with the great French astronomers Laplace, Lalande, and others. The new professor of mathematics and director of the observatory of Göttingen was admitted into the august company of the Paris academicians, who then ruled, and since the death of Euler had almost monopolised, the mathematical studies of the world. Although Gauss thus introduced the higher and abstract branches of exact science into the programme of a German university, and established a link between Paris and Germany in mathematics, as Humboldt had done shortly before in the natural sciences, fully a quarter of a century was to elapse before the spirit of exact research, and of the higher mathematics, really began to leaven the German universities. It then at length entered the field as a third and equally important agent by the side of the

16. Scientific spirit enters the universities in the second quarter of the century.

This was achieved to perfection, a proof of the usefulness of the method being the fact that Gauss succeeded in finishing in one hour a calculation which had taken Euler three days, and had resulted in his blindness. The second problem arises from the fact that the number of observations is always in excess of the number mathematically necessary, and that, owing to the unavoidable inaccuracies, different sets of observations give slightly different orbits. How are these to be used so as to give the most correct average result? This involves a question in probabilities. As early as 1795 Gauss was in possession of the so-called method of least squares, which occurred to him so naturally that he suspected that Tobias Mayer must have already known about it. It also occurred independently to Legendre, who was the first to publish it, in 1806, in his 'Nouvelles méthodes pour la détermination des orbites des comètes.' See Sartorius, 'Gauss zum Gedächtniss,' p. 41 *sqq.*

philosophical and classical spirit. During these twenty-five years Gauss lived and soared in solitary height—a name only to the German student, as Euler had been before him. Probably he was better known to the younger astronomers whom he trained, and the elder ones with whom he corresponded. But astronomy was not then within the pale of the universities. To what extent the character of Gauss's own genius was the cause of this it is difficult to say.[1] He himself had not come under the influence of any great teachers such as Paris then possessed; he was self-taught, and had early imbibed a great admiration for the methods of Euclid, Archimedes, and Newton; he wrote in the classical style fitted for all times, but not for uninitiated beginners.[2] It is certain,

[1] Bjerknes, in his most interesting memoir on Abel, refers frequently to the awe in which Gauss was held by younger mathematicians.

[2] In this Gauss resembled Newton. He was therefore, like Newton, frequently forestalled by others, who published his new methods and ideas in an unfinished and fragmentary form; whereby it is not suggested that these simultaneous discoveries or inventions were not quite independent. Two examples of this may be added to those given above. When Gauss published the 'Disquis. Arith.' in 1801, he left out the last or eighth section, which was to treat of the residues of the higher orders. He had already nearly completed the theory of biquadratic residues. In dealing with this subject he had found it necessary to extend the conception of number beyond the limits then in use. If we confine ourselves to integers, the only extension which then existed of the notion of number was in the use of negative numbers. These were counted on a straight line backward, as positive (or ordinary) numbers were counted forward. Gauss conceived the idea of counting numbers laterally from the straight line which represented the ordinary—positive and negative—numbers. He called numbers which were thus located in the plane "complex numbers," as they had to be counted by the use of two units, the ordinary unit 1 and a new unit i. He also showed that this new unit i stood in such relations to the ordinary unit 1 as were algebraically defined by the mysterious imaginary symbol $\sqrt{-1}$. The complete exposition of this new or complex system of counting was not explained by Gauss till the year 1831, when he published the 'Theoria residuorum biquadraticorum.' In the meantime the geometrical representation of imaginary quantities had been devised and published by Argand (1806), but not being employed for such important researches, it had remained unknown and unnoticed. See on the history of the subject, Hankel, 'Theorie der complexen Zahlensysteme,' 1867, pp. 71, 82. Gauss, through hiding his researches on this subject so long, lost the claim to the priority of the invention, though not of the effectual use of it. In another instance he allowed others to appropriate the merit of cultivating a large new field which had been familiar to him many years before. It was known all through the first half of the century that Gauss was in possession of valuable discoveries in what he termed the "new transcendent functions." References in the 'Disquisitiones,' § 335, in his correspondence with Schumacher, Bessel, Olbers, and Crelle, had made his friends curious to see the "amplum opus" which he had promised. It appears, however, that, independently of him, Jacobi and Abel (1802-29) following the investigations of Legendre (whose labours began in 1786 and culminated in his great work 'Traité des fonctions elliptiques, &c.,' 1825-28, 2 vols. and 3 supplements), succeeded in developing the theory very much on the same lines as Gauss had taken nearly a generation earlier. Eminent mathematicians who, since the publication of Gauss's posthumous papers, have fully investigated the subject, assign to Jacobi and Abel the undisputed priority of publishing, but to Gauss that of discovering, the fundamental properties of the "doubly periodical" functions. Full details will be found in the historical introduction to Enneper's 'Elliptische Functionen,' 2nd ed., Halle, 1890. See also Gauss's Werke, vol. iii. p. 491-496; Dirichlet's Discourse on Jacobi in Jacobi's Werke, vol. i. p. 11; C. A. Bjerknes, 'N. H. Abel,' Paris, 1885; Koenigsberger, 'Zur Geschichte der Theorie der elliptischen Transcendenten,' Leipzig, 1879.

however, that the spirit of exact and specially mathematical research owed its right of domicile within the universities to others who came after him, and to circumstances with which he was hardly connected.

17. Jacobi's mathematical school.

The man to whom Germany owes its first great school of mathematicians was Jacobi. He was self-taught like Gauss; but whilst Gauss followed in the footsteps of Newton and the ancients, Jacobi followed in those of Euler, Lagrange, and Laplace. The style and methods of these mathematicians, being more suited for didactic purposes than the classical style of Euclid, Newton, and Gauss, was probably more congenial to the mind of Jacobi, who from his twenty-first year (1825) developed a great activity as an academic teacher.[1] He was first

[1] Carl Gustav Jacob Jacobi (born at Potsdam 1804, died at Berlin 1851) was the first great mathe-

at Berlin, then at Königsberg; these two universities having become through him and Bessel the German teaching centres of the higher mathematics, both pure and applied. They have up to the present day fully maintained this pre-eminent position. They were teaching centres in the sense defined above—not only as regards mathematical knowledge and method, but likewise as regards mathematical research. For this purpose—as in the philological sciences—the lecture-room was not sufficient; there was also wanted a repository for the independent and original contributions of the school. Like the École polytechnique thirty years before in Paris, the Berlin school of mathematicians started with an important periodical. This was known as Crelle's Journal. Together with the Memoirs of the Paris Academy and the Journal de l'École polytechnique, it forms the principal repository for the higher mathematical work of the first half of the century.[1] It was also through

matical teacher of Germany. Of him Lejeune Dirichlet says: "It was not his business to communicate what was finished and what had been communicated before; his lectures all treated of subjects which lay outside of the field of the text-books, and covered only those parts of science in which he had himself been creative. With him this meant that they exhibited the greatest variety. His lectures were not remarkable for that kind of clearness which is characteristic of intellectual poverty, but for a clearness of a higher kind. He tried primarily to show the leading ideas which underlay any theory, and whilst he removed everything that had an artificial appearance, the solution of problems presented itself so easily to his hearers that they could hope to do something similar. . . . The success of this unusual method was truly remarkable. If in Germany the knowledge of the methods of analysis is now spread to a degree unknown to former times, if numerous mathematicians extend the science in every direction, this gratifying result is principally owing to Jacobi. Nearly all have been his pupils," &c. (Dirichlet's Discourse in the Academy of Berlin, 1852, Jacobi's Werke, vol. i. p. 21.)

[1] The two mathematicians on whom A. L. Crelle (1780-1855) relied mainly for contributions when he started the 'Journal für die reine und angewandte Mathematik' in 1826 were Abel and Steiner. For originality of thought they stand quite alone. Both extended

Jacobi, and still more through his contemporary Lejeune Dirichlet (born 1804 at Düren, of French extraction, and trained in Paris under Laplace, Legendre, Fourier,

the field of research which they cultivated by fundamentally new ideas of such breadth that fully half a century was required before they were thoroughly appreciated by mathematicians. Abel (a Norwegian by birth) died in 1829 when only twenty-seven years old, having during the four years which embrace his published memoirs extended the limits of algebra and laid the foundations for a more comprehensive treatment of the higher or transcendent functions, or forms of mathematical dependence. Mathematicians before him had tried to solve algebraically equations beyond the fourth degree, but had failed. Abel proved that the problem as then conceived could not be generally solved. Legendre had through his unaided labours, extending over thirty years, established the theory of elliptic integrals as far as was possible on the lines then adopted. Abel—and simultaneously Jacobi—treated the subject from an entirely novel point of view, and by doing so opened out quite a new field of research, the extent and importance of which Abel fully recognised when he presented to the French Academy his memoir of 1826, in which he dealt with functions of which those studied by Legendre and Jacobi were only special cases. This memoir, containing Abel's celebrated theorem, which he had already discovered in 1825, and which was published in a brief article in Crelle's Journal in 1829, remained unnoticed, being, as Legendre explained to Jacobi, almost unreadable. See Enneper, 'Elliptische Functionen,' 2nd ed., p. 192; Jacobi's Werke, vol. i. p. 439, &c. Abel has been called the greatest mathematical genius that has yet existed (Oltramare in 'La grande Encyclopédie,' art. "Abel"); his fellow-worker, Jacob Steiner (1796-1863, a Swiss by birth), has been termed the greatest geometrician of modern times. The progress of analysis had thrown into the background purely geometrical researches, although a revival of these had commenced in France with Monge and his followers, and had been further promoted by Poncelet, as well as simultaneously by Möbius and Plücker in Germany. The labours of the two latter remained for a long time unknown and unrecognised. Steiner, who was self-taught, who disliked the calculus, and considered it a disgrace that geometry could not solve her problems by purely geometrical methods, undertook to find the common root and leading principle which connected all the theorems and porisms bequeathed to us by ancient and modern geometry; he brings order into the chaos, and shows how nature with a few elements and the greatest economy succeeds in giving to figures in space their numberless properties. He not only completed that part of geometry which had been treated by the ancients—the geometry of the line, the conic sections or curves of the second order, and the surfaces in space corresponding to them—but he also attacked problems which before him had been solved only by the calculus, and even succeeded in carrying his methods beyond the reach of the calculus of variations, specially invented to deal with geometrical questions. Like Fermat in the theory of numbers,

Poisson, Cauchy), that the great work of Gauss on the theory of numbers, which for twenty years had remained sealed with seven seals, was drawn into current mathematical literature, and became, as Newton's 'Principia' had become a century earlier, an inexhaustible mine of wealth for succeeding generations.

18. Chemical laboratories established in 1826 through Liebig.

About the same time the experimental side of exact research—the use of the chemical balance, through which Lavoisier and his followers had done so much to establish chemistry on a firm and independent basis—received a great impetus by the establishment of the *first chemical laboratories* within the pale of the universities.[1] In this direction the greatest influence probably belongs to the small town of Giessen, where Liebig opened his celebrated laboratory in the year 1826. It became the

Steiner in geometry left to his followers a large number of theorems and problems without proofs which he had solved by his methods; and it was only in quite recent times that the Italian Cremona succeeded in definitely clearing up the whole of this original and valuable bequest. See Hankel, 'Die Elemente der projectivischen Geometrie, chapter i.; Jacob Steiner, Werke, vol. ii. p. 495.

[1] On Liebig's laboratory see Hofmann's Faraday Lecture, p. 8. Chemical laboratories existed for teaching purposes before Liebig's at Giessen. Kopp ('Geschichte der Chemie,' vol. ii. p. 19) mentions one at Altorf, which was founded, 1683, by the council of the city of Nürnberg for academic teaching purposes. For the training of the modern school of chemists no man did more than Berzelius, in whose laboratory there were trained Chr. Gmelin, Mitscherlich, H. and G. Rose, Wöhler, Magnus, Arfvedson, Nordenskiöld, Mosander, and others. Sir William Thomson (Lord Kelvin) in 'Nature,' vol. xxxi. p. 409, mentions the beginnings of laboratory-teaching at Glasgow by Prof. Thomas Thomson in 1828. But what was probably peculiar to Liebig's laboratory was the systematic and methodical training, on a specially devised plan, in qualitative, quantitative, and organic analysis, by which young persons were introduced to a thorough knowledge of chemical properties and manipulations. The guides, text-books, and tables for analytic work of Will, Fresenius, and others were elaborated to meet the requirements of such methodical teaching. Almost simultaneously with Liebig at Giessen, Purkinje at Breslau laid the foundation for the first physiological laboratory. See Du Bois-Reymond, 'Reden,' vol. ii. p. 367.

training-school for the greater part of the eminent chemists outside of Paris, and the model for similar establishments, and extended its influence over the world—into England, Scotland, and America. It also did more than any other institution of that kind for the development of ready and accurate methods of analysis, such as are now used in the remotest regions. But it was significant for German chemistry, and for the cosmopolitan character of German science generally, that this brilliant development of experimental research was stimulated from two independent centres; that German chemists as little as German mathematicians attached themselves in a one-sided manner to the Paris school.

19. Cosmopolitan character of German science.

In mathematical science the classical style of Gauss, transmitted from the ancients through Newton, combined with the analytical or modern French style of Jacobi and Dirichlet to give to German research its character of universality. In a similar manner, when chemistry again found a domicile in Germany and became an integral portion of the university programme, it had been trained in two different schools. For there lived at that time in Sweden the eminent authority Berzelius,[1] who divides with Gay-Lussac the glory of being

[1] J. Jacob Berzelius (a Swede, 1779-1848), one of the most eminent and industrious of chemists, had a great influence on the development of modern chemistry by the number as well as by the accuracy of his experimental determinations, by his invention of methods and apparatus for analysis, and by his extensive proofs of several of the most important theories. The latter directed the labours and governed the opinions of many—especially German—investigators. It was through him mainly that Richter's chemical equivalents and Dalton's atomic theory were extensively verified and applied to all parts of the science, to organic and mineralogical chemistry. He also elaborated, in close connection with Davy's electrical discoveries, his celebrated electrochemical theory, which up to the year 1840 was very generally accepted by chemists; and he assisted through his repeated expositions

the master of the great German chemists of the middle of the century. Mitscherlich at Berlin and Wöhler at Göttingen belonged to the school of the former, whereas Liebig had the good fortune to be introduced through Humboldt into Gay-Lussac's laboratory at Paris as the first pupil.[1]

and criticisms in breaking down the older oxygen theory of acids in favour of Davy's more general views, based upon his recognition of chlorine and iodine as elementary bodies. His handbook of Chemistry, as well as his 'Jahresbericht' (from 1820), probably did more than any other publications for the diffusion of accurate chemical information.

[1] Liebig has himself, in an autobiographical memoir published posthumously, so fully described the merits of the two schools, and at the same time given such a vivid picture of the truly scientific spirit which animated German universities at that time, that I am tempted to give here some extracts. Of his studies in Paris he says: "What influenced me most in the French lectures was their inner truthfulness and the careful omission of all mere semblance of explanations: it was a complete contrast to the German lectures, in which, through a preponderance of the deductive process, the scientific doctrine had quite lost its rigid coherence. . . . I returned to Germany (1824), where, through the school of Berzelius, . . . a great reform had already begun in inorganic chemistry. . . . I always remember with pleasure the twenty-eight years which I passed at Giessen: it was, as it were, a higher providence which led me to the small university. At a large university, or in a larger town, my powers would have been broken up and frittered away, and the attainment of the aim which I had in view would have been much more difficult, if not impossible; but at Giessen all were concentrated in the work, and this was a passionate enjoyment." "The necessity of an institute where the pupil could instruct himself in the chemical art, by which I understand familiarity with chemical operations of analysis and adroitness in the use of apparatus, was then in the air, and so it came about that on the opening of my laboratory . . . pupils came to me from all sides. . . . The greatest difficulty presented itself, as the numbers increased, in the practical teaching itself. In order to teach many at once, an ordered plan was required and a progressive way of working, which had to be thought out and tried. . . . A very short time had sufficed for the celebrated pupils of the Swedish master to give to mineral analysis . . . an admirable degree of perfection. . . . Physical chemistry . . . had through the discoveries of Gay-Lussac and Humboldt, . . . and of Mitscherlich, . . . gained a solid foundation, and in the chemical proportions the edifice appeared to have received its coping-stone. . . . No organic chemistry . . . then existed; Thénard and Gay-Lussac, Berzelius, Prout, Döbereiner, had indeed laid the foundation of organic analysis; but even the great investigations of Chevreul on the fatty bodies received for many years only scant attention. Inorganic chemistry still absorbed too many, and indeed the best, forces.

Twenty years after Gauss's great mathematical achievements, two new discoveries announced to the scientific world that Germany had again taken a foremost position in chemistry. These were Mitscherlich's discovery of isomorphism in 1819,[1] and Wöhler's preparation of an organic compound from inorganic materials in 1828.[2]

20. Liebig's organic analysis.

In 1830 Liebig succeeded in finally establishing that simple and accurate method of organic analysis known by his name. Organic chemistry, in its modern sense,

The direction I had received in Paris was a different one. . . . I saw very soon that all progress in organic chemistry depended on its simplification. . . . The first years of my residence at Giessen were almost exclusively devoted to the improvement of organic analysis, and with the first successes there began at the small university an activity such as the world had not yet seen. . . . A kindly fate had brought together in Giessen the most talented youths from all countries of Europe. . . . Every one was obliged to find his own way for himself. . . . We worked from dawn to the fall of night: there were no recreations and pleasures at Giessen. The only complaints were those of the attendant, who in the evenings, when he had to clean, could not get the workers to leave the laboratory." See 'Deutsche Rundschau,' vol. lxvi. pp. 30-39.

[1] Eilhard Mitscherlich (1794-1863), a pupil of Berzelius, discovered in 1819 that in compound bodies which crystallise in definite forms certain elements can be replaced by others in the proportion of their chemical equivalence without changing the form of crystallisation. Such elements are termed "isomorphous." Berzelius declared this to be the most important discovery that had been made since the theory of chemical proportions had been established.

[2] This synthesis was the preparation of urea, a highly organic substance, out of the compounds of cyanogen, with the examination of which he and Liebig were then occupied. "It was the first example of the fact that an organic substance could, by chemical methods alone, be produced out of inorganic materials; this discovery destroyed the difference which was then considered to exist between organic and inorganic bodies—*viz.*, that the former could only be formed under the influence of vegetable or animal vital forces, whereas the latter could be artificially produced" (Kopp, 'Geschichte der Chemie,' vol. i. p. 442). It must here be remarked that this statement is only correct if the substances, cyanic acid and ammonia, out of which Wöhler produced urea, are considered to be inorganic; inasmuch as neither of them had then been produced otherwise than out of organic substances, the popular notion on Wöhler's important discovery requires this correction. See Kopp, 'Gesch. der Wissenschaften in Deutschland,' vol. x. p. 546.

may be said to date from these and other simultaneous labours of Liebig and Wöhler.[1] But although the pure sciences, mathematics, physics, and chemistry, advanced on new lines in the hands of German students, and although theoretical investigations have always been favourite pursuits of theirs, as we shall have ample opportunity to note in the course of our further survey, the greatest contribution to the progress of science, and the most brilliant performances of the exact spirit of research which emanated from Germany during the first half of this century, lay in a different direction. And it is hard to believe that the conditions favourable to this peculiar growth could have been found anywhere else than in the German universities. The many elements of thought which meet on that ground, the equal dignity

[1] The joint labours of Liebig (1803-73) and Wöhler (1800-82), which have become of such importance to science, form one of the most interesting instances of scientific co-operation between two men pursuing different lines of thought and trained in different schools. See the preface to Hofmann's edition of Liebig and Wöhler's Correspondence. In Liebig's autobiographical sketch, quoted above, he thus enlarges on his relations to Wöhler: "It was my good fortune that, from the beginning of my career at Giessen, similar inclinations and endeavours secured me a friend, with whom, after so many years, I am still (between 1860 and 1870) connected by ties of the warmest affection. Whereas in me the tendency predominated to look for the likenesses of substances and their combinations, he possessed an incomparable talent for seeing their differences; acuteness of observation was joined in him to an artistic aptitude and to a genius for finding new ways and means of analysis such as few men possess. The perfection of our joint researches into uric acid and the oil of bitter almonds has been frequently praised; this is his work. I cannot sufficiently estimate the advantage which both my own and our joint aims derived from my union with Wöhler; for in them were combined the peculiarities of two schools, and the good which each had, attained its value through co-operation. Without grudge or jealousy we pursued our way hand in hand; if one required help, the other was ready. An idea can be formed of this mutual relation when I mention that many of the smaller productions which bear our names belong to one alone; they were charming little presents which one gave the other" (p. 39).

which there belongs to pure and to applied science, the continual contest which exists there between metaphysical and exact reasoning, and the general ebb and flow of rival currents of ideas, all seem to have been necessary to raise to the rank of an exact science those researches which deal with the phenomena of *life* and *consciousness* in their normal and abnormal forms of existence. In the hands of German students[1] chemistry and physics, botany and zoology, comparative anatomy and morphology, pathology, psychology, and metaphysics, have laboured from different and unconnected beginnings to produce that central science which attacks the great problem of organic life, of individuation, and which studies the immediate conditions of consciousness. *Physiology*, or to use its more comprehensive name, *Biology*,[2] may be 21. Biology a German science.

[1] The two greatest discoveries in physiology belong to England. These are Harvey's discovery of the circulation of the blood in the seventeenth century, and Charles Bell's discovery of the difference of sensory and motor nerves in the early part of this century. The two men, however, who have done most to establish physiology as an independent science, whose systematic works have done most for the student of physiology, are probably Haller (see *supra*, p. 176), whose 'Elementa' cast into the shade all older handbooks, and Johannes Müller (1801-58), whose 'Handbuch' (1833-40) was translated into French and English. See Du Bois-Reymond, 'Reden,' &c., vol. ii. pp. 143, &c., 195, 360, who also points out how in other sciences, like mathematics, physics, chemistry, Germans made use almost exclusively of translations of French and English text-books and handbooks, whereas in physiology they furnished for a long period the systematic treatises for the whole world (vol. ii. p. 196). Physiology has therefore with some right been termed a German science (see Helmholtz, 'Vorträge,' &c., vol. i. pp. 339, 362; Du Bois-Reymond, 'Reden,' vol. ii. p. 265). Compare also what Huxley says, 'Critiques and Addresses,' pp. 221, 303. On the connection of physiology with all other sciences see likewise Helmholtz, *loc. cit.*; Du Bois-Reymond, vol. ii. p. 341; Huxley, 'Lay Sermons,' &c., p. 75; 'Science and Culture,' p. 52: "A thorough study of human physiology is, in itself, an education broader and more comprehensive than much that passes under that name. There is no side of the intellect which it does not call into play, no region of human knowledge into which either its roots or its branches do not extend," &c.

[2] The word "biology" seems to have been first used by G. R.

said to be a German science as chemistry has been named a French science. I have already referred to the great Haller in the last century, who may be called the father of physiology; to Blumenbach, the comparative anatomist; and to Liebig and Wöhler, who first among chemists succeeded in producing an organic compound by the processes of inorganic chemistry. I have now to add two names, which together mark a great revolution in our ideas of the structure of organisms, and link together the two sciences which had treated separately of the animal and vegetable worlds. About the year 1838

22. Cellular theory of Schleiden

Mathias Schleiden[1] propounded his cellular theory con-

Treviranus (1776-1837), a learned physician of Bremen, who began to write his 'Biologie oder Philosophie der lebenden Natur' in 1796 and to publish it in 1802 (6 vols., 1802-22). Lamarck used the word in his 'Hydrogéologie,' 1801. They, as well as Bichat about the same time, independently "conceived the notion of uniting the sciences which deal with living matter into one whole, and of dealing with them as one discipline" (Huxley, on the study of Biology, 1876, in 'American Addresses,' p. 136, &c.) The term, though of German origin, has not found favour in that country, and after having been used officially in France and England, makes its appearance in Germany only since the great works of the modern English school, headed by Darwin, have gained so much influence in Germany. In the meantime the biological sciences had been extensively represented at the German universities by chairs of physiology, zoology, botany, &c. According to Huxley, biology has been "substituted for the old confusing name of natural history," and "denotes the whole of the sciences which deal with living things, whether they be animals or whether they be plants" (*loc. cit.*, p. 138). It can be divided into three branches —(1) Morphology, which comprises the sciences of anatomy, development, and classification; (2) the science of the distribution of living beings, present and past; and (3) physiology, which deals with the functions and actions of living beings, and tries to "deduce the facts of morphology and of distribution from the laws of the molecular forces of matter" (Huxley, 'Lay Sermons,' &c., p. 83, 1864). To these three Huxley adds ('Ency. Brit.,' art. "Biology") the infant science of "ætiology," which "has for its object the ascertainment of the causes of the facts of biology and the explanation of biological phenomena, by showing that they constitute particular cases of general physical laws" (p. 688).

[1] Mathias Jacob Schleiden (1804-81), for some time Professor of Botany at Jena, was a man of peculiar ability and disposition, combining a philosophical mind with exact knowledge and a general literary taste, not frequently

cerning the structure and growth of plants. About the same time Theodor Schwann[1] extended this theory to animal organisms. A variety of circumstances combined to make the announcement of the *cellular theory*, which will always be associated with those two names, an epoch in the history of scientific, indeed of general, thought.

and Schwann.

The historian of botany, Julius Sachs, describes the publication of Schleiden's great work as a burst of daylight,[2] and Du Bois-Reymond says: "In order to measure the magical progress which it marks, one must have witnessed the rise of the cellular theory, when it suddenly spread daylight in the darkness of the hidden structure

to be found among men of pure science in Germany. Opposed to the idealistic philosophy as a follower of Fries, and on the other side to the dry systematisation of the Linnæan school, he was the man at once to broaden the scientific view and to create a popular interest in the "life of the plant"-world. The titles of his two best known works are characteristic, 'Die Botanik als inductive Wissenschaft' (1842-45), and his short-lived periodical (filled with the labours of his equally important co-editor, Nägeli), 'Zeitschrift für wissenschaftliche Botanik.'

[1] Through the friendship of Schleiden and Schwann (1810-82, a pupil of Johannes Müller and professor at Louvain), two independent courses of research and scientific thought were brought together. Schleiden placed the "cell" —a term used before him by Hooke, Malpighi, Grew, Wolff, Brown, and Mirbel—in the forefront of his description as the element of form and as the origin of life, or—as we now express it—as the morphological and embryological unit, in the plant. A similar series of great names, beginning with Bichat and leading up to Johannes Müller, marks the studies of animal tissues. Schwann, struck with the analogy of Schleiden's nucleated cells and similar structures which he had observed in the notochord, conceived and verified on a large scale the idea "that a common principle of development exists for the most different elemental parts of the organism, and that the formation of cells is this principle." This is the beginning of the cellular theory, which produced at once a reconstruction of the whole of "general anatomy" by Jacob Henle (1809-85), and subsequently the "cellular pathology" of Rudolph Virchow. As the latter has himself said, he aims at the establishment of a general *biological* principle, and thus the discovery of Schleiden and Schwann is characterised as the transition from the "historical" to the "biological" study of animated nature.

[2] See Julius Sachs, 'Geschichte der Botanik vom 16 Jahrh. bis 1860,' p. 203, and in many other passages.

of animals and plants, where the rays of comparative anatomy and embryology could not reach."[1] This bold generalisation, which had been prepared by a long series of botanical and morphological researches in and out of Germany, met alternately with applause and criticism; it gave rise to a long controversy, and was the starting-point of a whole line of important discoveries.[2] It secured for Germany a long period of supremacy in physiological science. This supremacy was more than maintained by a great volume of minute investigations, which emanated from

23. Ernst Heinrich Weber

the schools, and centred in the names, of E. H. Weber[3]

[1] Du Bois-Reymond, 'Reden,' vol. ii. p. 541, &c.

[2] "Whatever cavillers may say, it is certain that histology before 1838, and histology since then, are two different sciences—in scope, in purpose, and in dignity—and the eminent men to whom we allude may safely answer all detraction by a proud *Circumspice*."—Huxley in his valuable paper on "The Cell Theory" in the 'British and Foreign Medical Chirurgical Review,' 1853, vol. xii. p. 290.

[3] The three brothers Weber (Ernst Heinrich, 1795-1878; Wilhelm, 1804-91; and Eduard, 1806-71) may be looked upon as early representatives of the best form of German research on the lines now recognised as the true and fruitful ones in natural science. Born in an age when other great and more widely known reformers—such as Liebig, Schönlein, and Joh. Müller—freed themselves with difficulty from the prevailing metaphysical systems, they seem to have at once seized the true spirit of exact research without relinquishing the broader philosophical and encyclopædic view of the sciences which they cultivated. Living far into an age when the utilitarian spirit became equally seductive in an opposite direction, they preserved pure and undefiled within themselves the German ideal of *Wissenschaft* as a pursuit carried on for its own intrinsic value, not for any immediate practical object. Their position, especially that of the two elder brothers, is in this respect unique, and may be studied independently of the scientific ideas which they represented, and which will occupy us later on as a chapter in the history of thought characteristic of the German mind and the best type of the university studies. In three works of classical value—'Die Wellenlehre auf Experimenten begründet' (E. H. and W. Weber), 1825; 'Die Mechanik der menschlichen Gehwerkzeuge' (W. and E. Weber), 1836; 'Elektrodynamische Maasbestimmungen' (W. Weber), 1846 onward—and in a great number of special investigations, the method of exact measurement was applied to physical, physiological, and even mental phenomena, and the foundation laid for a mechanical description and mathematical calculation. The later generalisations, known as Wilhelm Weber's law of electro-dynamics and E. H. Weber's law of psycho-physics, have given rise to long controversies and fruitful theories. Their joint labours cover fully half a century. See for a sympathetic picture of the position which the three brothers Weber held in the learned world the biography of Fechner by Kuntze, 1892, p. 243: "They were among the first to raise the study of Nature among Germans to the eminence occupied by the philosophers and discoveries of the Latin races."

and Johannes Müller. The school of the latter especially has the merit of having introduced over the whole field of physiological phenomena exact methods of inquiry, of having established physiological laboratories all over Germany similar to Liebig's chemical laboratory at Giessen, and of having effectually chased away the vague notions of the older metaphysical school, and diffused the true scientific spirit. It boasts of having filled the chairs of medicine, physiology, and anatomy at the German universities with a long list of eminent teachers who have spread this true scientific spirit in every branch of the medical sciences,[1] which it has in consequence drawn into

and Johannes Müller.

[1] The medical sciences, represented by the medical faculty, but also by those biological sciences which, like botany, zoology, anthropology, &c., belong to the philosophical faculty, now furnish the largest number of students to the German universities. In the beginning of the century the theological faculty, which then included the greater part of those who prepared themselves for higher teaching, stood at the head as regards numbers. Under the influence of the philologico-historical movement, which grew and culminated in the course of this century, and the rising tide of the exact sciences, the philosophical faculty for a time gained and maintained the upper hand. Biological—including medical—studies now command the greatest attention. In his statistical report (contained in Lexis, 'Die deutschen Universitäten,' Berlin, 1893) Prof. Conrad gives an interesting table of the changing numerical proportion in the different faculties (vol. i. p. 125, &c.) Prof. Billroth in his admirable treatise, 'Ueber das Lehren und Lernen der medicinischen Wissenschaften,' Vienna, 1876, deals with this subject at all the German universities, including the Austrian. As Vienna is such an important centre of medical studies, the proportion of those students who cultivate biological studies would probably be still greater if we were to include the Austrian universities. I suppose the figure would be about 40 per cent of the whole. To Billroth's treatise I may also refer as confirming in relation to these more modern branches what I said above of the culture of *Wissenschaft*. See p. 279 and the whole section on the relation of the biological sciences to the university, pp. 411-446. It is

the circle of the exact or mechanical sciences. But not only in its far-reaching applications to medical knowledge and practice has the movement which centred in Weber and Müller shown its strength and importance; it has also, from the commencement, extended its influence
24. Psycho-physics. in another direction. To it belongs pre-eminently the cultivation of that borderland which connects the natural and the mental sciences. Müller[1] himself began his career by a study of the mechanism of the perceptions of the senses. He affirmed the law of *specific energies*,

interesting to note that Prof. Billroth does not employ the word biological, but uses the untranslatable compound *naturwissenschaftlich-medicinisch*.

[1] Johannes Müller (1801-58) has been termed the Haller of the nineteenth century, the Cuvier of Germany. A very good account of his work, which forms an important chapter in the history of German biology, is contained in Du Bois-Reymond's 'Gedächtnissrede auf Joh. Müller' (1858), reprinted with extensive notes in his 'Reden,' vol. ii. pp. 143-334. Müller is there considered as the last representative of a dynasty of philosophers who embraced the whole domain of "biology," which since has become divided into various sciences, notably the morphological and the physiological branches. He thus stands out as the master of some of the greatest modern representatives of natural and medical science, such as Schwann and Henle in anatomy, Brücke, Du Bois-Reymond, and Helmholtz in physiology, Virchow in pathological anatomy. He together with Lucas Schönlein (1793-1864) may be considered as the founder of the modern Berlin school of medicine, contemporaneous with which is the modern Austrian school, with the names of Purkinje, Skoda, Oppolzer, and Rokitansky. An excellent characterisation of the different positions and influences, of the cross-currents of thought, of the original homes and of the wanderings of the scientific spirit through the many German-speaking countries and the extensive network of German universities, will be found in Billroth, *loc. cit.*, pp. 307-366. If we imagine a similar life as existing all through the century in other domains of thought—in philosophy, theology, philology, mathematics, chemistry, law, and the science of history—we get a faint idea of the work of the German universities. In Lexis, 'Die deutschen Universitäten,' an attempt has been made to give such a picture. The picture, however, suffers by the exclusion of the Austrian universities, and these—notably in the medical world—hold such a very high position that the record of the united work is somewhat incomplete. The sciences are also in this record cut up into many branches, whereas in the earlier part of the century many of these were united and represented by one great name. Such a name was Johannes Müller in biology.

which declares that the differences of the sensations of light and colour, of sound, of touch, &c., do not depend upon the mode of irritation, nor even upon the different structure of the specific nerves, but upon the nature of the central sense organ. In the school of Müller the phenomena of voltaic electricity, which had been so seductive and misleading to an earlier school of physiologists not experienced in the methods of exact research, were again subjected to scientific investigation, and led to the brilliant researches with which the name of Du Bois-Reymond is so intimately connected. He is as ready as Helmholtz, who in his two great works on physiological optics and musical acoustics has founded new branches of science,[1] to acknowledge the leadership of Johannes

[1] Helmholtz (1821-95), equally celebrated as physiologist and mathematical philosopher, was educated under the influence of Johannes Müller on the one side, of Jacobi and the Königsberg school of mathematicians (Bessel and Neumann) on the other. If we add to this that he also made a profound study of those far-reaching speculations which originated in the philosophy of Kant, we realise how rare is the combination of ability and knowledge which he has brought to bear on the discussion of the most advanced problems in physics, biology, and psychology. In the sequel I shall have to refer so frequently to his writings that I confine myself here to giving the date of his principal, his epoch-making publications: 1847. 'Ueber die Erhaltung der Kraft'; 1858. 'Ueber die Integrale der hydrodynamischen Gleichungen, welche der Wirbelbewegung entsprechen'—both reprinted in 'Wissenschaftliche Abhandlungen,' Leipzig, 1882 and 1883, 2 vols. These two Memoirs may be considered as corner-stones of two of the most important modern theories in physical science, the "conservation of energy" and the "theory of vortex motion." In both, the name of Helmholtz is intimately allied with that of William Thomson (Lord Kelvin). Equally important and more comprehensive have been his researches in the physiology and psychology of sense-perceptions in his 'Physiologische Optik,' Leipzig, 1867; 'Lehre von den Tonempfindungen,' Braunschweig, 1863.

Helmholtz has also contributed largely to the discussion of two very important branches of modern speculation—first, the theoretical views on the nature of electrical phenomena expressed by the opposite conceptions of Wilhelm Weber in Germany and Faraday in England; second, the origin of geometrical axioms, especially the axiom referring to parallel lines. A great interest in this subject had been

Müller. And out of the circle of which E. H. Weber was the centre, has emanated that work of Fechner, 'Elements of Psycho-physics,' which marks an epoch in psychology: it is indeed mainly occupied with the exposition and application of what is termed Weber's law of sensation.[1] In the course of the second quarter of the century, the names of Gauss and Jacobi in mathematics, of Liebig and Wöhler in chemistry, of Schleiden and Schwann in the science of life, of Müller and Weber in physiology, raised German science to the level previously reached by the French Academicians, by Laplace and Lagrange, by Lavoisier and Berthollet, by Cuvier and St-Hilaire, by Vicq-d'Azyr and Bichat. During

created by the posthumous publication of Riemann's celebrated Memoir, 'Ueber die Hypothesen welche der Geometrie zu Grunde liegen,' Göttingen, 1865. Helmholtz's invention of the ophthalmoscope in 1851 marks an epoch in ophthalmology.

[1] Gustav Theodor Fechner (1806-87), professor at the University of Leipsic, was an extraordinary man. The wide range of his interests and his great personal influence are well described in his biography by Dr Kuntze, 'G. T. Fechner, Ein deutsches Gelehrtenleben,' Leipzig, 1892. Together with Lotze he may be said to have brought about the reform of German speculative philosophy, and in relation to this he will occupy our attention largely in a later portion of this book. He belonged to the circle of which E. H. Weber was the centre, and has taken an important place in the history of philosophy and science by his now celebrated work, 'Elemente der Psychophysik,' 2 vols., Leipzig, 1860; 2nd ed., 1890. The object of this work is to establish "an exact doctrine of the relations of body and mind," the principal task being "to fix the measure of psychical quantities." He says in the preface: "The empirical law which forms the principal foundation, was laid down long ago by different students in different branches, and was expressed with comparative generality mainly by E. H. Weber, whom I would call the father of psycho-physics" (Preface, p. v). In early life Fechner did much, by his translations of Biot's 'Physics' and Thénard's 'Chemistry,' as well as by his own experimental works, to introduce the French scientific spirit into German research. His psycho-physical labours have been continued by Prof. Wundt; his importance as marking a turning-point in German philosophy is brought out in Paulsen's 'Einleitung in die Philosophie,' Berlin, 1890. See especially Preface, p. viii, and p. 318, where Fechner is placed before Lotze.

the second half of the century, the influence of French thought on German science has been less marked, partly owing to the independent course which the latter, since the age of Johannes Müller, has struck out for herself in the biological sciences, partly through the more intimate intercourse which has set in between English and German thought. The three great scientific ideas which the second half of the century has been establishing—the law of the conservation of energy, Darwin's theory of descent, and Faraday's novel conception of electrical phenomena—have been elaborated mainly by the co-operation of English and German research, though it must be admitted that at least one of these developments dates back to the beginnings laid by French science,[1] whilst the views of Faraday are subversive of some of the fundamental notions to which the works of the great French mathematicians had given very general currency. Before we can enter more fully on a review of these more modern ideas, I must, however, give a picture of the state of scientific thought in England during the first half of the century. This will be our subject in the last portion of the present section.

[1] Darwin's theory of descent has its forerunners in Lamarck and St-Hilaire, whose merits in this respect are supposed to have been overlooked owing to the overwhelming authority of Cuvier. See Huxley, "Origin of Species" in 'Lay Sermons,' 1891, p. 252; "Evolution in Biology" in 'Science and Culture,' 1888, pp. 296, 313. But whilst it is true that Lamarck and St-Hilaire entertained doubts as to the fixity of species, the explanation of the particular manner in which the change of species takes place is entirely due to Darwin, and without this further step speculations as to the origin of species would have remained for a long time in the vague. Lamarck's speculations were of no real use to Darwin, and had besides been anticipated by Erasmus Darwin. On the other hand, the researches of Sadi Carnot were of great value in the hands of Joule, Thomson, and Helmholtz, who may be regarded as the founders of the doctrine of the conservation of energy.

25. Spirit of exact research and *Wissenschaft*.

But it is my object at present not so much to dwell upon specific ideas or doctrines as on the growth, the diffusion, and the general character of scientific thought, as this has been established by the separate contributions of the three nations in the course of the first half of our century. I therefore cannot leave the subject of German science without still more precisely noting the peculiar character which scientific thought has assumed under the influence of the German university system. As we saw before, when the spirit of exact research, mainly through the influence of the great French mathematicians and physicists, became diffused in Germany, and entered the pale of the German universities, it was met there by that peculiar ideal of learning which the German language terms *Wissenschaft*. This encounter did not everywhere produce a favourable reception for the new school; but in the end it led, like every controversy, to a firmer establishment of the true principles of research. The life of the German universities had in the earlier centuries begun with classical studies; it had been reformed under the influence of the theological and juridical requirements of the Protestant Governments; and ultimately it had been entirely renewed under the influence of the classical and philosophical studies centred in the fourth or philosophical faculty. These classical and philosophical studies combined to create the ideal of *Wissenschaft*, or science, in the broadest sense of the word. This ideal formed the central conception in the new scheme of a higher and general education of the nation; it accompanied the great revival in art, poetry, and literature. In the

philosophy of Kant and Fichte, the republican notions which led the political movements in America and France had been reduced to a system and theoretically proved; the discipline of a classical education was the school in which leaders and youths were trained who marched into the war against the great oppressor. This ideal of *Wissenschaft* had thus acquired a practical meaning, an ethical—not to say a religious—significance; it was allied to the religious revival preached by Schleiermacher and a section of the Romantic school. Of its value as a principle for guiding research and learning it had given proof in that great circle of studies which, since the time of F. A. Wolf and Wilhelm von Humboldt, was comprised under the name of *Philology*. Under its influence new universities were being founded and academies remodelled.

Now, it is the peculiarity of all philosophical and historical studies that they deal with one great subject, which cannot easily be divided into a number of independent parts capable of separate treatment; since their interest attaches mainly to the fact that they explore the workings and manifestations of the human mind in the past and in the present. These studies are therefore forced to keep always in the foreground the idea of a great unity of action and purpose, to aim at completeness of view, and to refer all special researches to general principles and standards. The encyclopædic view, in fact, is forced upon all philosophical and historical sciences. Almost without exception the great masters and teachers who lived in the beginning of this century adhered to this view, and however great in special and

26. Encyclopædic view necessary in philosophy and history.

detailed research, measured the importance of their results according to the light which they were able to throw upon the questions referring to the whole subject and its combined life and unity.

It was also natural, seeing that this comprehensive or philosophical treatment led to such great results in the historical sciences, that an attempt should have been made to deal with the phenomena of Nature by a similar conception. It was not a new or a far-fetched suggestion to regard Nature as the playground of a hidden intelligence, of an unconscious mind, just as history, language, and thought were viewed as the manifestations of the conscious human mind. After this the further conception was not remote that both the mind of Nature and the mind of Man are only two different sides of the universal or absolute Mind. The philosophy of Schelling was the first attempt to put this idea into an applicable form, the system of Hegel the first confident elaboration of it in its various ramifications and applications. At the time when the mathematical and physical sciences were leading the way in France, and gradually forcing their way into Germany, most of the universities in the latter country had one or more representatives of that new and apparently promising school which termed itself

27. Philosophy of Nature.

the "Philosophy of Nature." The trammels of this school had to be shaken off by those who, as they became gradually convinced of its barrenness in actual results, took up the cause of the exact or mathematical sciences now that they had been cultivated by many isolated labourers in Germany and in England, and had been

for the first time connected into a great organisation by the French Academy of Sciences.

28. Conflict between the scientific and the philosophical views.

The opposition in which the new school of exact and detailed research stood to the representatives of the broad philosophical view gave rise to a great many currents of thought; for neither the former nor the latter presented a united front. Among those who advocated the exact methods of research there was a section which clung more exclusively to the empirical side, and cultivated the descriptive and experimental sciences; whereas others, whom we may call the French school of science, developed the mathematical methods, not without a certain ill-disguised contempt for pure empiricism.[1] On the side of classical and philosophical studies there was a section which cultivated the historical[2] in contradis-

[1] On the relations of mathematical and experimental physics, and the different opinions which existed during the first half of the century, see Helmholtz's popular addresses in many places, but especially the discourse on Gustav Magnus (1802-70), who may be regarded as a representative of the experimental school in Germany. In the opinion of this school, which cultivated the borderland of physics and chemistry, of organic and inorganic phenomena, or investigated the less known phenomena of frictional electricity (Riess) or the complicated phenomena of meteorology (Dove), a danger existed that mathematical theories and elaborate calculations might lead to an estrangement from nature and observation, similar to that which speculative philosophy had created before. Helmholtz himself was met by this sentiment when he published his great memoir, 'Ueber die Erhaltung der Kraft,' in 1847; Poggendorf's physical periodical would not receive it, and Jacobi, the mathematician, was the only one who showed any interest in it. See Helmholtz, 'Wissenschaftliche Abhandlungen,' vol. i. p. 73; 'Reden,' vol. ii. p. 46.

[2] As the philosophy of Schelling promoted a study of nature, and in doing so prepared its own downfall, so the philosophy of Hegel led to a study of history, and thus to the proof of the insufficiency of its own generalisations. Many valuable beginnings of historical research emanated also from the Romantic school of literature. In all these instances philosophical interests led beyond the abstract logical and metaphysical treatment into the broad and fertile plains of actual life, be it that of nature or of art or of history. But the true methods of research in

tinction to the philosophical view, and another which elaborated what it termed exclusively the critical methods,[1] not without a certain suspicion regarding those who showed a desire to roam into outlying fields which did not permit of equally strict discipline and treatment. So far as this refers to the purely historical sciences, I shall revert to the subject when I come to treat of the principles which underlie and guide this line of studies. At present I am concerned with the growth and diffusion of the exact scientific spirit and its methods.

29. Alexander von Humboldt.

No one did more to spread the ideas and methods of French science in Germany than Alexander von Humboldt. He himself had done original scientific work[2] be-

these extensive fields were afterwards found not so much in philosophical canons as in a love of detail and observation, and in the exercise of an unbiassed criticism of facts and records. For the relations of philosophy to history in respect of this, see Wegele, 'Geschichte der deutschen Historiographie,' München, 1885, 5th book, p. 975, &c. Equally important are—Gervinus, 'Grundzüge der Historik,' Leipzig, 1837; the 'Nekrolog auf Schlosser,' Leipzig, 1862, including the whole literature which it provoked; and O. Lorenz, 'Die Geschichtswissenschaft,' Berlin, 1886, especially the first chapter.

[1] On the Critical school of philology, and the wider and narrower sense in which the aims and methods of the science of antiquity were defined, see Bursian, 'Geschichte der classischen Philologie in Deutschland,' München und Leipzig, 1883, p. 665, &c.; also O. Ribbeck, 'Friedrich Ritschl,' Leipzig, 1879 and 1880. Further, the essays on Böckh, K. O. Müller, and Georg Curtius in the third volume of Ernst Curtius, 'Alterthum und Gegenwart,' Berlin, 1889; and, finally, the chapter on "Klassische Philologie" by Wilamowitz-Möllendorf in Lexis, 'Die deutschen Universitäten,' vol. i. p. 457, &c.

[2] Alexander von Humboldt (1769-1859) published in 1797, shortly after Galvani's great discovery, his 'Versuche über die gereizte Muskel- und Nervenfaser.' In the history of science his name will live as that of the man who organised that "scientific conspiracy of nations" which is peculiar to our century, and without which the study of geography, meteorology, astronomy, the phenomena of tides and magnetic disturbances—called by him magnetic storms—could not effectually be carried on. The fact also that on his return from his great travels he became next to Napoleon Bonaparte the most famous man in Europe, did more than anything else to raise the natural sciences in the popular mind to that eminence which earlier belonged to polite literature.

fore he left Germany for the extensive travels by which he became celebrated, and through which he founded a new science—the science that deals with the geographical distribution of plant life. Moreover, his absence from his native country fell within that period during which the philosophical school, headed by Schelling and Hegel, attained to its greatest power. He was never drawn into its vortex; on the contrary, he maintained a lifelong protest against the spirit of its doctrine at a time when the circle which surrounded him at Berlin came under its powerful influence.[1] He led a long line of ardent young workers both to the right sources of scientific knowledge and to an ultimate victory over the opposed school of thought. Though not a profound mathematician himself, he appreciated the part which mathematics were destined to play in science. Among other things, he protected and encouraged younger mathematical talents, and tried to draw Gauss from the solitary heights which he inhabited into the midst of the scientific circles of the day.[2] Then there was the great influence which

[1] Cf. p. 178, note 1. It has latterly become the fashion to say so much against the mistaken methods of the *Naturphilosophie* that it is well to remember how many men of foremost rank in the natural sciences belonged at one time to this school or were influenced by it. Foremost of all stands Oken (1779-1851), the founder of the German Association of Science, and editor of the periodical 'Isis.' Further, the comparative anatomist Carus (1789-1869); Oersted (1777-1851), the discoverer of electro-magnetism; Kielmeyer, the friend of Cuvier (1765-1844); Ignaz Döllinger (1770-1841), one of the earliest evolutionists; D. G. Kieser (1779-1862), a medical teacher of great influence. More or less influenced by the teachings of this school were Goethe (1749-1832); Karl Ernst von Baer (1792-1876), whose impartial opinion on the *Naturphilosophie* as early as 1821 is important. Further, Liebig (1803-73); Johannes Müller (1801-58); Röschlaub (1768-1835); Schönlein (1793-1864), the founder of what is called the "natural-history" school of medicine.

[2] See A. von Humboldt's Life by Bruhns, translated by Lassell, 1873, vol. ii. p. 145 *sqq.*

30. Influence of Berzelius on German science.

Berzelius exerted on German science through his teaching and his writings. From him emanated that great perfection of the purely experimental methods which in his own hands, as well as in those of Wöhler, Mitscherlich, Magnus, and others, led to an accumulation of detailed knowledge in chemistry of unforeseen importance and magnitude. His own annual reports, as well as Gmelin's celebrated handbook of chemistry, are monuments of this unparalleled industry.

Others, like Liebig, Johannes Müller, Lucas Schönlein, freed themselves under the influence of French science,[1] or by their own deeper insight, from the sway of the false and misleading philosophy to which they had at one time listened. A third section started from philosophical premisses, but from premisses opposed to the doctrines of Schelling and Hegel.

The school of Fries,[2] in which Schleiden was the most

[1] English science had an important but less marked influence on the development of naturalistic and medical studies in Germany. So far as the latter especially are concerned, see Billroth, 'Ueber das Lehren und Lernen der medicinischen Wissenschaften an den Universitäten der deutschen Nation,' Wien, 1876, p. 33. He roughly divides the medical schools of Germany into two groups, both descending from Boerhaave: the one, the modern Berlin school of Müller, Schönlein, Romberg, and Virchow, through Haller, Reil, Hufeland, and Röschlaub; the other, the modern Vienna school of Oppolzer, Rokitansky, and Billroth, through Gerhard von Swieten, De Haen, Stoll, Frank, Purkinje, and Skoda. Of French names which had great influence he gives Broussais, Corvisart, Bayle, Cruveilhier, and Laënnec; of English, John Hunter, Matthew Bailie, and Astley Cooper. He gives also the name of Immanuel Kant as an important influence in the development of the German schools of medicine.

[2] Jacob Fries (1773-1843) professor at Heidelberg and Jena, led the critical philosophy of Kant into the channels of psychology and anthropology. During the heyday of transcendental philosophy, the philosophy of Fries, like that of the Scotch school, was regarded with contempt by Hegel, and even by Herbart, the opponent of Hegel. It succeeded, however, in the end in influencing a considerable number of philosophical minds, who carried philosophical thought into the inductive sciences. Besides the psychologist Beneke and the theologian De Wette, these were principally members of the Jena school, Apelt, Schlömilch, and others, who edited 'Abhandlungen der Fries'schen Schule,' Jena, 1847; and foremost among them Schleiden, the reformer of botany in Germany. Schleiden's great work appeared with the title 'Botanik als inductive Wissenschaft.' It opened with a philosophical introduction of 131 pages, in which inductive reasoning is recommended in opposition at once to the transcendental *Naturphilosophie*, and to dry empiricism. See Sachs, 'Geschichte der Botanik,' p. 203, &c.

illustrious name, carried on within the pale of the philosophical school of science itself a successful opposition to the philosophy of Nature.[1] But whilst much good and sound work was done by many who were content to remain outside of the favoured studies which set the tone of university culture during the classical and philosophical period of German thought, the great attack upon the mistaken canons of the philosophy of Nature came from that science which had probably suffered more than any other under the baneful influence of hollow theories and empty phraseology. **31. Philosophy of Nature and medical science.**

Helmholtz describes the despair which had taken hold of thinking minds in the medical profession[2]: "My education fell within a period of the development of medicine when among thinking and conscientious minds there reigned perfect despair. It was not difficult to understand that the older and mostly theorising methods of treating medical subjects had become absolutely useless. But with the theories the facts which underlay them were so indissolubly entangled that these two were mostly cast overboard. How the science must be newly built up the example of the other natural sciences had made clear, but yet the new task stood of giant-height before us. A beginning was hardly made, and the first beginnings were

[1] See Schleiden, 'Schelling's und Hegel's Verhältniss zur Naturwissenschaft,' Leipzig, 1844.

[2] See Helmholtz, 'Vorträge und Reden,' vol. i. p. 361.

often very crude. We cannot wonder if many honest, serious, thinking men then turned away in dissatisfaction from medicine, or if they from principle embraced an extreme empiricism."[1] "But the right kind of work brought forth its fruits much sooner than many had hoped. The introduction of mechanical notions into the theories of circulation and respiration, a better insight into the phenomena of heat, the more minutely elaborated physiology of the nerves, speedily produced practical results of the greatest importance; the microscopical examination of parasitic tissues, the stupendous development of pathological anatomy, led irresistibly from nebulous theories to real facts." And again[2]: "Whilst in the investigation of inorganic nature the different nations of Europe progressed pretty evenly, the recent development of physiology and medicine belongs pre-eminently to Germany. The questions regarding the principle of life

[1] Cf. Helmholtz, ibid., vol. ii. p. 178, in his discourse "Ueber das Denken in der Medicin": "At that time there were many among the younger doctors who, in despair about their science, gave up all therapeutics, and took to empiricism, such as was then taught by Rademacher. This on principle regarded as vain all hope of scientific insight." Not only the extreme empiricism of Rademacher (1772-1850), but still more the wild theories of Hahnemann (1755-1843) found during this age of general unsettlement many followers. See on the origin, the principles, and the spread of homœopathy, Häser, 'Geschichte der Medicin,' vol. ii. p. 793, &c. Häser gives the year 1816 as the date at which Hahnemann's doctrines began to be accepted in wider circles. "It must not be forgotten that the heyday of homœopathy fell in that age when medicine, especially in Germany, was in a very deficient state, so that the accusations raised by Hahnemann and his adherents did not appear quite unfounded. It is even to be admitted that homœopathy has contributed to the reaction through which in our times the regeneration of the art of healing has been brought about, though this would have taken place without Hahnemann" (p. 803). Homœopathy has no scientific representative at any of the German universities, and yet it is admitted that it "still enjoys a great reputation in some influential circles among the general public" (Hirsch, 'Gesch. d. medicinischen Wissenschaften,' p. 570).

[2] Helmholtz, *loc. cit.*, vol. i. p. 362.

are closely allied to psychological and ethical questions. To start with, here also that untiring industry is required which applies itself to pure science for purely ideal purposes, without immediate prospects of practical usefulness. And indeed we may glory in the fact that in this German scholars have always distinguished themselves by their enthusiastic and self-renouncing diligence, which labours for inner satisfaction and not for outer success."

32. Science for its own sake.

This habit of self-renouncing labour, of singleness of purpose—in short, the ideal of pure science and its pursuit—had been elaborated in many a secluded workshop of a retired German university mainly under the influence of the classical and philosophical studies of the end of the last and the beginning of the present century. It was held up high and conspicuous by the priests of humanity, beginning with Lessing, Herder, and Kant, and ending in Schleiermacher, Hermann, and Böckh, at the head of a great army of devoted followers, travelling through the wilderness of national depression, barbarism, and despair into the promised land of freedom, culture, and hope. Such an ideal is of priceless worth, and it is this ideal which the philosophical and classical school of thought bequeathed during the first half of the century to that new school of thinkers which was destined to study, in an equally patient and unselfish spirit, the seemingly less elevated, but not less mysterious and fascinating, problems of Nature. Truly Gauss, Weber, and Johannes Müller worthily headed the new army of labourers.

33. Bequest of the classical and philosophical school.

But though the elevated spirit in which scientific work is carried on may be the most valuable bequest of the classical and philosophical to the exact and empirical

school, there were certain more tangible characteristics of German research, which were carried over from the older to the modern type of thought. It will be useful to define these more clearly.

In the course of the second half of the eighteenth century German literature and German philosophy had started from the beginnings laid by other nations, and after mastering and appropriating their achievements, had set out for a new course and a higher flight. Milton and Shakespeare[1] in epic and dramatic poetry; Ossian, the Percy Ballads, and Burns in song and lyric; Gibbon in history; Joseph Scaliger and Bentley in philology; Locke, Hume, and Spinoza in philosophy; Rousseau in prose,—all these great names of a later or earlier past had become familiar watchwords to German poets or students—to Lessing, Herder, and Goethe, to Schlegel, F. A. Wolf, and Wilhelm von Humboldt, to Böckh, Hermann, and Niebuhr, to Kant, Fichte, and Jacobi, before they came forward with their own creations. The same cosmopolitan spirit of looking elsewhere and everywhere for beginnings, and for co-operation in the united work of learning; the same historical taste, the same desire to glean from all quarters,—characterised the early decades of the revival of German science. Hence the many periodicals and annual reports; hence the fact that the

[1] These names are not given as they follow in time, but as they followed in their influence on German thought and literature. Thus the early representatives of the German revival were influenced by Milton and Pope more than by the greater Shakespeare: epic and didactic preceded dramatic poetry: Shakespeare was made familiar to German readers only through Goethe and Schlegel. Similarly the reaction against the school of Leibniz and Wolff in philosophy began with Kant's reply to Hume's sceptical philosophy, whereas the study of Spinoza influenced Kant's followers and opponents, Jacobi, Fichte, and Schelling.

nation which requires them least[1] possesses the most and the best translations of foreign authors. But the quality of greatest value for science which springs from the cosmopolitan and historical spirit is that of completeness and thoroughness of research.

34. Completeness and thoroughness of research.

Secondly, the German man of science was not only thorough, but was as little as the German philosopher or classicist had been, an isolated thinker. He was neither the member of an academy only, nor a solitary genius reduced to the resources of his own study. He lived mostly at a university, surrounded by others, whose labours came in contact with his own, or who treated the same subject from a different point of view. He had thus to define the limits of his science, and to see that no part of the common field was left uncultivated and unexplored. His object could not be to produce simply a work of individual greatness or of finished artistic merit; his work was an integral portion of the one great science; his

[1] This must not be misunderstood. A knowledge of the masterpieces of foreign literature was as necessary to the development of the German mind as it is to that of any other nation; it was and is more complete there than in any other country: what I mean is, that as a knowledge of French and English has been for a long time so common among the educated classes in Germany, translations are more easily dispensed with there than in other countries. In spite of that, German literature abounds in excellent translations of the classics of France and England both in general literature and in science. It is also interesting to note that no modern language has succeeded so well in imitating foreign and classical metres as the German, hexameters having become domiciled in Germany through Voss and Goethe, the Alcaic and Sapphic metres through Klopstock and Herder, the more complicated stanzas through Platen, and above all through Donner's excellent renderings of the Greek dramatists. Rückert excelled in the imitation and reproduction of Persian, Indian, and Arabic poetry, and through him and Friedrich Bodenstedt German literature has been enriched by many lines of which it would be difficult to say whether their home was in Germany or in the far East, so perfectly is the spirit and diction reproduced. The well-known 'Weisheit des Brahmanen' of Rückert, and Bodenstedt's 'Mirza Schaffy' are examples.

labours had to fit in with the general plan, to find a place in the one great edifice.

35. Combination of research and teaching.

Thirdly, the German man of science was a teacher; he had to communicate his ideas to younger minds, to make the principles and methods of research clear, to guarantee, in his course of lectures, something like completeness, to give a comprehensive survey; not to teach "une science faite," but to draw out original talent in others, to encourage co-operation in research, to portion out the common work to the talents which surrounded him, or it might be to direct the flight of the aspiring genius.[1]

[1] Here the two main objects of academic teaching are to impart a knowledge of the right method in the special science, and to give a survey of the whole domain of the science. The two principal institutions by which these objects are attained were first set going in the classical branches of study, and may be defined by two terms—the "seminary" and the lecture on "encyclopædia." Both terms are taken from earlier institutions. The seminary was originally a training-school for priests or teachers. Under such masters of methodical research as F. A. Wolf and Gottfried Hermann, the institution acquired a different character. "The seminaries are the real nurseries of scientific research. They were founded, indeed, with a different object; the first seminaries, the philological seminaries, which were started during the last century at Halle and Göttingen, were or should have been pedagogic seminaries for the future masters in the learned schools. In reality they were—especially that of F. A. Wolf—in the first place institutions in which the art of philological research was taught. This is even more the case in the philological seminaries and societies which during the nineteenth century have been conducted by G. Hermann, Fr. Thiersch, Fr. Ritschl, and others: they were nurseries of philologists, not of teachers. And the same may be said of the numerous seminaries which in modern times have grown up in the other sciences within the philosophical faculty, and also in the faculties of theology and law: they set up as their aim—with few exceptions—the training for scientific work and research, not the utilisation of knowledge for a practical purpose" (Paulsen in Lexis, 'Die deutschen Universitäten,' vol. i. p. 74, &c.) The same idea was in the mind of Liebig when he started the first chemical laboratory at Giessen (see *supra*, p. 188, note). The encyclopædic treatment of every large subject in a special course of lectures arranged for this purpose had the object of preventing the different studies from falling asunder or ultimately failing to unite in the realisation of one great aim. This great aim of all philological studies, for instance, was always held up by men like Wolf, Hermann, Böckh, and Ritschl, among whose favourite lectures were those on "encyclopædia" of philology. Something similar existed, and exists still, in theology, law, and what are called "*Staatswissenschaften*." All these terms are supposed to embrace a variety of studies which are organically combined in one whole, forming a cycle. In philosophy proper Hegel, and later Lotze, delivered well-known and largely attended lectures under the title of Encyclopædia. This is a remnant of the encyclopædic or organic treatment of knowledge sketched out by Bacon, and proposed as a basis for their celebrated work by Diderot and D'Alembert (see *ante*, p. 35 and note). The encyclopædia, as a learned dictionary, we have seen, has since become merely a synopsis. How different from this was the truly encyclopædic treatment given by men like Böckh can be seen from his correspondence with K. O. Müller, where he scolds his younger friend for undertaking to write the article "Topography of Athens" for "such a cursed publication as an encyclopædia," whereas he himself was regularly lecturing on "encyclopædia of philology," in which he took in earnest the idea of classical philology as "the historical science of the life of the ancient peoples" (see Curtius, 'Alterthum und Gegenwart,' vol. iii. p. 138, &c.) Now although the exact sciences when they became domiciled in the German universities did not in general copy this institution, yet the historical and philosophical survey, giving method and unity to a large circle of studies, has been upheld by many among the foremost men of science, especially in the medical faculty. Of these I only mention Joh. Müller (see Du Bois-Reymond, 'Reden,' vol. ii. pp. 195, 279) and his pupil and follower Jacob Henle, who in his lectures on anthropology took a philosophical survey of the whole subject of the medical studies (see 'Jacob Henle' by Merkel, p. 271, &c.)

Lastly, the German man of science was a philosopher. Whatever his aversion might be to special philosophical doctrines, he had generally come under the influence of some philosophical school, the teaching of which he desired either to uphold or to combat. Sooner or later, consciously or unconsciously, he had to make clear to himself and to his disciples the underlying principles which he thought the right ones, to defend them against attacks from others, or to modify them, as progressing research made it necessary. If the historical sciences had benefited most by the philosophy of Schelling and Hegel, which attempted to give new and constructive views on the intellectual and ethical manifestations of the human or the general soul, the mathematical and phy-

36. Combination of science and philosophy.

sical sciences have been most affected by the spirit of Kant's philosophy, which has ineradicably engrained in the German mind the necessity of a criticism of the principles of knowledge. Ever and anon some of the most brilliant intellects in mathematics and science have reverted to the same problems, and, on the whole, they have confirmed the position taken up by Kant a century ago.

It was thus under the influence of the exact methods of experiment and calculation taught by the great French school in the beginning of the century, and at the same time through the philosophical spirit peculiar to German science, that in the middle of the century the different sciences which deal with the phenomena of life and consciousness were remodelled. The great science of biology, based upon mechanical principles, was thus created, and the results gained in it brilliantly applied to the reorganisation of the medical profession. But this great reform does not belong exclusively to one great name; it is the work of a long line of thinkers: nor can I conceive that the exclusive employment of the methods of exact research would have so effectually brought it about, unaided by the philosophical, historical, and critical spirit which formed the peculiar characteristic of German thought before the exact methods had been generally introduced. And just because this reform required to be effected from so many different beginnings, and gradually elaborated and defended before it became firmly established, do the modern sciences of physiology and pathology deserve to be termed pre-eminently German sciences; for no other

37. Biology grown out of science and philosophy combined.

country possessed the necessary conditions and extensive organisations, the habits of combined study and patient co-operation, the large views and the high aims, which had been acquired at the German universities under the guidance of the German ideal of *Wissenschaft*, and under the sway of the philosophical and classical spirit.

A great authority,[1] who as much as any one represents the modern as distinguished from the earlier views in biological science, reviewing the different agencies which have brought about the great change, speaks thus. He is referring to Johannes Müller, the father of modern physiology. "The modern physiological school," he says, "with Schwann at its head, has drawn the conclusions for which Müller had furnished the premises. It has herein been essentially aided by three achievements which Müller witnessed at an age when deeply-seated convictions are not easily abandoned. I mean, first of all, Schleiden and Schwann's discovery, that bodies of both animals and plants are composed of structures which develop independently, though according to a common principle. This conception dispelled from the region of plant-life the idea of a governing entelechy, as Müller conceived it, and pointed from afar to the possibility of an explanation of these processes by means of the general properties of matter. I refer, secondly, to the more intimate knowledge of the action of nerves and muscles, which began with Schwann's researches, in which he showed how the force of the muscle changes with its contraction. Investigations which were carried on with all the resources

38. Du Bois-Reymond on Müller.

[1] See Du Bois-Reymond, 'Reden,' vol. ii. p. 219, &c.

of modern physics regarding the phenomena of animal movements, gradually substituted for the miracles of the 'vital forces' a molecular mechanism, complicated, indeed, and likely to baffle our efforts for a long time to come, but intelligible, nevertheless, as a mechanism. The third achievement to which I refer is the revival among us by Helmholtz and Mayer of the doctrine of the conservation of force. This cleared up the conception of force in general, and in particular supplied the key to a knowledge of the change of matter in plants and animals. By this an insight was gained into the truth that the power with which we move our own limbs (as George Stephenson did those of his locomotive) is nothing more than sunlight transformed in the organism of the plant: that the highly oxygenated excrements of the animal organism produce this force during their combustion, and along with it the animal warmth, the πνεῦμα of the ancients. In the daylight which through such knowledge penetrated into the chemical mechanism of plants and animals, the pale spectre of a vital force could no more be seen. Liebig, indeed, who himself stood up so firmly for the chemical origin of animal heat and motive power, still retains an accompanying vital force. But this contradiction is probably to be traced to the circumstance that the celebrated chemist came late, and as it were from outside, to the study of the phenomena of life. And even Wöhler still believes in a vital force, he who in his time did more than any one to disturb the vitalistic hypothesis through his artificial production of urea."

39. "Vital force" abandoned.

It was a process of critical sifting similar to that which Kant[1] applied to our general metaphysical ideas, which in the middle of the century, through the writings of Berzelius and Liebig, of Schwann and Schleiden, of Henle, Lotze, and Du Bois-Reymond, gradually dispelled the older confused notions, and firmly established the mechanical view in the study of the phenomena of life. But as we are forced to recognise the substance of much of Kant's philosophical criticism in the lucid expositions of Locke and Hume before him, so it has been pointed out that the words of the eminent French physiologist, Vicq-d'Azyr, contain the substance of the more modern ideas on life.[2] It required the co-operation of the exact

40. Mechanical view in biology.

[1] The great influence which belongs to Kant in the development of modern German science has been frequently dwelt on. In more recent times some of the first representatives of the medical and biological sciences have dealt with the subject, and the opposition which fifty years ago originated in the extravagances of some of Kant's successors, has given way to a renewed recognition of the just claims of Kant. We may refer to Du Bois-Reymond, who, forgetting Lotze, calls Kant the last philosopher who took a part in the work of the naturalist ('Reden,' vol. i. p. 33); to Helmholtz, who in many passages of his popular addresses refers to the merits of Kant ('Vorträge und Reden,' 1884, vol. i. pp. 44, 368; ii. 58, 227, 234, 248, &c.); to Haeser ('Geschichte der Medizin,' vol. ii. p. 811). I will add to these the opinion of so great an authority as Prof. Billroth of Vienna, who, speaking of the two modern schools of medicine in Germany, says ('Lehren und Lernen der medicinischen Wissenschaften,' &c., p. 334): "However great the degree of independence may be which the two parallel schools have attained, they would hardly have developed so rapidly without the powerful influence which came from France and in a lesser degree from England; nor yet without that of Immanuel Kant, who in his 'Autophysiology of Reason' enlightened German minds regarding their own selves, and who with his lively imagination fervently embraced natural science."

[2] The remarkable passage referred to is quoted by Du Bois-Reymond ('Reden,' vol. ii. p. 27): "Quelqu' étonnantes qu'elles nous paraissent, ces fonctions (*viz.*, dans les corps organisés) ne sont-elles pas des effets physiques plus ou moins composés, dont nous devons examiner la nature par tous les moyens que nous fournissent l'observation et l'expérience, et non leur supposer des principes sur lesquels l'esprit se repose, et croit

spirit of research with the critical methods acquired in the school of philosophy, and the exhaustive survey of a large array of facts acquired through historical and classical studies, before the significance of this brilliant *aperçu* became evident; before the underlying ideas could become useful guides of research and progress. "Tantæ molis erat Romanam condere gentem."

Though the reform of the biological[1] sciences, and their application to pathological inquiries, are probably the greatest achievement which the methods of exact research, in conjunction with the philosophical spirit, can boast of in Germany in the century, the same habit

avoir tout fait lorsqu'il lui reste tout à faire." This was said at the end of the last century, and fifty years later Du Bois-Reymond (*loc. cit.*) could complain that the truth contained in these words was not yet generally admitted, in spite of the labours of Berzelius, Schwann, Schleiden, and Lotze. Compare also A. von Humboldt's own confessions on this point in his 'Ansichten der Natur,' vol. ii. p. 309, &c., edition of 1849.

[1] I must remind the reader here that though I use the word biological as denoting the more recent point of view from which all phenomena of the living world are being grouped and comprehended, and though the word seems to have been first used by a German, nevertheless the arrangement of studies at the German universities has hardly yet recognised the essential unity of all biological sciences. They are unfortunately still divided between the philosophical and the medical faculties. It is indeed an anomaly, hardly consistent with the philosophical and encyclopædic character of German research, that palæontology, botany, zoology, and anthropology should belong to the philosophical, whereas anatomy, physiology, and pathology are placed in the medical faculty. Eminent biologists and anthropologists, such as Schleiden, Lotze, Helmholtz, and Wundt, have accordingly belonged to both faculties. To place biological studies on the right footing would require a mind similar to that of F. A. Wolf, who evolved out of the vaguer idea of *humaniora* the clearer notion of a "science of antiquity," and who accordingly was able to convert the training-school of teachers, the seminary, into a nursery of students of antiquity. Whether a similar reform in the purely scientific interests of the "science of life," which is now mostly cultivated for the benefit of the medical practitioner, can be effected in this age, when practical aims are gradually taking the place of scientific ideas, is another question.

of thought has shown itself in other fields of research, and led to similar innovations. I will here only mention one other line of inquiry, where neither exact nor metaphysical reasoning alone suffices, but where a combination of both is essential. I mean the gradual change which, mainly through the writings of German mathematicians, has come over our fundamental conceptions in the region of geometry, algebra, and the theory of numbers. This subject belongs so essentially to the domain of pure thought that a history of thought seems specially called upon to take notice of it. Accordingly I intend to devote a special chapter to it. At present it interests us mainly because it is an outcome of that peculiar modification which the exact or scientific spirit of thought underwent when, introduced by French and English models, it came in contact with the philosophical and classical ideal of learning in Germany. I will repeat more clearly and concisely what I mean. The exact methods of thought, mainly elaborated in France, and there largely applied, give to science its accuracy and definiteness. In spite of this accuracy and definiteness, it is not immediately clear whether they will lead to completeness of knowledge, or whether they may not be misapplied. To guarantee completeness, to make sure that in the whole great field no portion has remained untouched and unexplored, that love of detail, that searching and exploring spirit, is required which is nursed pre-eminently by historical and classical studies. And to avoid the abuse of existing methods, there is further required that critical spirit which inquires into the value of principles

41. Criticism of principles of mathematics.

and the limit of their usefulness. These three directions of thought mark three tolerably distinct attitudes of the human mind. Skill in inventing and in applying new and precise methods—the exact habit or attitude of thought; love of detail, and the desire for complete and exhaustive knowledge—the historical habit or attitude of thought; lastly, the desire to become fully alive to the value of existing methods or principles, which implies a consciousness of the limited nature of one and every principle—the critical habit or attitude of thought. The progress of mathematics and natural science depends primarily on the first; classical studies depend on the second; philosophical reasoning mainly on the last. Each of the three nations which have led human progress and thought during the past centuries has probably been possessed of these three cardinal virtues in equal proportions. For though Newton stands pre-eminent in the first, we have Laplace and Gauss and their numerous followers in other countries; though the great volume of classical learning and criticism has emanated from the schools of Wolf, Hermann, and Böckh, they themselves point back to Bentley and Joseph Scaliger; and even Kant's unrivalled enterprise was prepared by Hume, and dates back to Descartes. There need, therefore, be no angry rivalry or carping jealousy. We may point to the remarkably equal contributions of the three nations to the general progress of thought. But a very different and truly legitimate interest prompts us to note how in the great performances of each nation, in the literature of each of the three languages, different factors have been at work—different

42. The exact, the historical, and the critical habits of thought.

agencies have combined to produce the effect. In this regard the spectacles presented by French, German, and English thought differ. And there seems to me little doubt that during a considerable portion of this century the German universities, grown out of theological, legal, and medical studies, and widening gradually till they embraced and deepened all three by the philosophical, the classical, and the exact spirit of research, present that organisation in which the different elements of thought are most equally balanced, through which modern knowledge and the scientific spirit have been most widely and successfully diffused, and that the German ideal of *Wissenschaft* embraces at once the highest aims of the exact, the historical, and the philosophical lines of thought.

43. Combined in the German ideal of *Wissenschaft*.

Nor would it be right to pass from the consideration of this peculiar feature of nineteenth-century thought, which is an outcome of the German university system, without noticing the moral significance which this ideal of *Wissenschaft* acquired, and which marks it as a factor in progress and in culture of much more importance even than the lasting discoveries in science which it has made, or the monuments of learning which it has reared. It is not the political side of this movement which I refer to, not even pre-eminently the educational, though these are interesting and important enough to demand special historical treatment. What I should like to point to as the greatest in this movement is, that it belongs to the few and rare instances in the history of mankind when we see a large number of the most highly gifted members of

44. Moral value of *Wissenschaft*.

a nation following a purely ideal cause, apart from the inducements which gain or glory may furnish. The pursuit of truth and the acquisition of knowledge for its own sake, as an ennobling and worthy occupation, has during a large portion of our century been the life-work of professors and students alike in the German universities. In the biographies of many of them we meet with that self-denial and elevation of spirit which is the true characteristic of every unselfish human effort. In perusing these records of high aspirations, arising frequently amid disheartening surroundings, these stories of privations cheerfully endured, of devotion to an ideal cause, glowing with all the fervour of a religious duty, we gain a similar impression to that which the contemplation of the Classical period of Greek art or the early Renaissance produces on our mind.

Once at least has science, the pursuit of pure truth and knowledge, been able to raise a large portion of mankind out of the lower region of earthly existence into an ideal atmosphere, and to furnish an additional proof of the belief that there, and not here below, lies our true home. We may perhaps have to admit with regret that this phase is passing away under the influence of the utilitarian demands of the present day; we may be forced to think that another—and, we trust, not a lower—ideal is held up before our eyes for this and the coming age. But no really unselfish effort can perish, and whatever the duty of the future may be, it will have to count among the greatest bequests of the immediate past that high and broad ideal of science which the life of the Ger-

man universities has traced in clear and indestructible outlines.[1]

[1] The testimonies by illustrious foreigners to the great work of the German universities are frequent and well known, from the time when Mme. de Staël visited Germany, and her friend Villers wrote his 'Coup-d'œil sur les Universités d'Allemagne' in 1808, through the writings of Cousin, the verdict of Renan, of Cournot, of Dreyfus-Brisac, and of the American, J. M. Hart. To these often-repeated expressions I will add that of the great apostle of higher culture of our age, of Matthew Arnold, who sums up his interesting report on the German system of higher education in these characteristic words: "What I admire in Germany is, that while there, too, Industrialism, that great modern power, is making at Berlin and Leipzig and Elberfeld most successful and rapid progress, the idea of Culture, Culture of the only true sort, is in Germany a living power also. Petty towns have a university whose teaching is famous through Europe; and the King of Prussia and Count Bismarck resist the loss of a great savant from Prussia as they would resist a political check. If true culture ever becomes at last a civilising power in the world, and is not overlaid by fanaticism, by industrialism, or by frivolous pleasure-seeking, it will be to the faith and zeal of this homely and much-ridiculed German people that the great result will be mainly owing" ('Schools and Universities on the Continent,' 1868, p. 256).

CHAPTER III.

THE SCIENTIFIC SPIRIT IN ENGLAND.

1. Scientific organisation abroad.

THE history of science in France and Germany during the first half of the present century is identical with the history of two great organisations, the Paris Institute and the German Universities. It is to them that we owe nearly all the great scientific work in the two countries: to the former we owe the foundation of the modern methods of scientific work during the last period of the eighteenth and the early years of the nineteenth century; to the latter we owe pre-eminently the diffusion and widespread application of those methods.[1] We now turn to the country which, in advance of France and Ger-

[1] In respect of this I cannot sufficiently recommend M. Maury's volume on 'L'ancienne Académie des Sciences,' which is as eloquent a testimonial to the scientific labours of eminent Frenchmen during the eighteenth century as the companion volume on 'L'ancienne Académie des Inscriptions et Belles Lettres' is a proof of the absence of philological studies during that period. The recent publication of Lexis' work, 'Die deutschen Universitäten,' is just as eloquent a testimonial to the labours of the German universities during this century. The first impression we get from the perusal of these two works is that for a long period France almost monopolised the exact sciences, just as later, for a similar period, Germany almost monopolised classical research, the science of antiquity. And yet the former was probably as much indebted to the Englishman Newton as the latter was to the Frenchman Joseph Scaliger for the character each acquired during the two periods I refer to.

many, had produced the greatest scientific model of modern times, a work which has probably done more than any other purely scientific work to revolutionise our scientific notions—the 'Principia' of Newton. In the subsequent history of the thought of this century, the next chapter will deal with the part that the Newtonian ideas have played throughout the whole period. We have now to turn our attention to the state of science in Great Britain during the period when Paris academicians and German professors combined to define and carry the spirit of modern scientific thought into the several mathematical, physical, and biological branches of research.

Considering that the great scientific institutions of the Continent—the Paris Institute, the scientific and medical schools in Paris, and the German universities—have done so much for the furtherance of science and the diffusion of the scientific spirit, it is natural that we should ask, What have similar institutions done in this country? 2. Similar institutions in Great Britain.
These institutions are, indeed, mostly older than the academies and modern universities of the Continent. The Royal Society, if not older than the French Academy, is certainly older than the Paris Academy of Sciences.[1]

[1] The actual dates are as follows: The first Academy devoted to the pursuit of science seems to have been the "Academia Secretorum Naturæ," founded at Naples in 1560. Several societies devoted to the culture of literature and art existed in Italy, such as the Academy "della Crusca" (founded at Florence in 1582). The great French Academy, devoted exclusively to the study of the French language, dates from 1629, and received its charter in 1635. The Royal Society, though not the first scheme of its kind which was started in this country—for the establishment of a Royal Academy was discussed as far back as 1616—actually started (1645) in the private meetings described in 'Dr Wallis's Account of Some Passages of his own Life' (quoted by Weld, 'Hist. of the Royal Society,' vol. i. p. 30). These meetings, according to him, were suggested by a German, Theodore Hank, then resident in London. The members were "persons inquisitive into natural philos-

The universities of Oxford, Cambridge, Edinburgh, Dublin, and Glasgow[1] are older than most of the German universities which have done the great scientific work of this century. So far as wealth is concerned, no institution on the Continent could compare with the two older English universities, and the Royal Society had in the beginning of this century long emerged from the poverty which characterised her early history during the lifetime of Newton.[2] Let us look at the subject from a

ophy, . . . and particularly of what hath been called the New Philosophy or Experimental Philosophy." It formed a branch at Oxford in 1649, and received a royal charter in 1662, four years before the "Académie des Sciences" at Paris—which had also previously existed as a private gathering of savants at the houses of Mersenne, Montmort, and Thévenot—was formally installed in the Bibliothèque du Roi. The "Accademia del Cimento" at Florence was established in 1657; but it only lasted ten years. Very irregular were also the life and labours of the "Academia naturæ Curiosorum" (later called A. Cæsarea Leopoldina), founded at Vienna in 1652. The Accademia del Cimento printed an important volume of Transactions in 1666. The Royal Society published its first volume in 1665. The first volume of the 'Journal des Savants' is of the same year. Very complete information will be found on all foreign Academies in the 'Grande Encyclopédie,' art. "Académie."

[1] Although the dates of the foundation of Oxford and Cambridge are uncertain, they were certainly more than a century—probably two centuries—older than Prague, the first German university, founded by the Emperor Charles IV. in 1347. The older Scotch universities were founded in the course of the fifteenth century, about the same time that Leipsic appears to have had its origin through a secession from Prague. The German universities—Halle, Göttingen—which were the seat of modern erudition, have a much later date, as given in chap. ii. p. 159, above. Edinburgh was founded at the end of the sixteenth century, and Trinity College, Dublin, about the same time. Leyden, which exerted a great influence both on Scotch and German higher education during the seventeenth century, was somewhat older than Edinburgh.

[2] It appears from Weld ('History,' &c., vol. i. pp. 231, 241, 246, 316, 462, 473) that the financial position of the Royal Society was precarious, and frequently engaged the serious attention of the Council, during the whole first hundred years of its existence; that as late as 1740 the whole revenue of the Society was only £232 per annum. An effort was then made to get in the large arrears of subscriptions and other contributions. In the following year the income seems to have exceeded the expenditure by £297. Weld adds, "It is a painful task to record these periodical visitations of poverty, which threatened the very existence of the Royal Society;

3. English science in the early part of the century.

different point of view. England has during the early part of the century, in all but the purely mathematical sciences, a greater array of scientific names of the first order than Germany, and nearly as great an array as France. Black, Herschel, Priestley, Cavendish, Davy, Young, Dalton, Faraday, Rowan Hamilton, Brewster, Lyell, Charles Bell, are all identified with one or more novel ideas or definite branches of research.[1] Great Britain had thus no lack

there is, however, a proportionate amount of pleasure in witnessing the triumphant manner in which the small band of philosophers extricated their institution from serious difficulties, unassisted by Royal bounty and labouring alone on account of their love for science" (vol. i. p. 474).

[1] The following are the principal dates referring to the great discoveries made in this country during the half-century ending 1825:—

1774. Priestley (1733-1804) discovers oxygen and a variety of other gases.
1775. Black (1728-99), Memoirs on latent heat.
1775. Maskelyne (1732-1811) measures the Attraction of Mount Shehallien.
1775. Landen (1719-90) expresses the arc of an hyperbola in terms of two elliptic arcs.
1778. Benjamin Thompson (Count Rumford, 1753-1814) first experiments on heat by friction.
1781, 13th March, Sir William Herschel (1738-1822) discovers Uranus.
1784. Cavendish (1731-1810) discovers the composition of water.
1786-97. Caroline Herschel (1750-1848) discovers her eight comets.
1798. Cavendish determines the density of the earth.
1799. Davy (1768-1829), essay on heat, light, &c.
1800. Nicholson and Carlisle decompose water with the voltaic pile.
1801. Dalton (1766-1844), theory of evaporation.
1801. Young (1773-1829), first essay on the theory of light and colour.
1802. Dalton, law of expansion of gaseous fluids.
1802. Playfair (1748-1819), 'Illustrations of the Huttonian Theory.'
1802. Wollaston (1766-1829), on Iceland spar, and undulatory theory.
1802-3. William Herschel, observations on nebulæ and double stars.
1802-3. Young expounds the principle of "Interference."
1803-4. Dalton proposes the atomic theory.
1804. Leslie (1766-1832), experiments on heat.
1804. Wollaston discovers palladium and other kindred metals.
1806. Davy isolates the alkaline metals.
1807. Young introduces the word Energy (lect. i. p. 75).
1809. Ivory (1765-1842), on the attraction of ellipsoids.
1810. Young (in 'Quarterly Review') explains the different refractions in crystals.
1810. Davy discovers chlorine to be a simple body.

either of great men of science or of great institutions, and yet—in spite of these—we read in the course of the first third of the century about the decline of science in England. That such could be seriously said of a country which within fifty years had in astronomy discovered a new planet (the first addition to the number known to the ancients), had discovered oxygen, latent heat, and the decomposition of water, applied the galvanic current for isolating the most refractory metals, laid the groundwork for the undulatory theory of light, established the atomic theory, put forth in statics and dynamics two of the most important modern generalisations,[1] and introduced in the treatment of electric and

4. Alleged decline of science in England.

1810. Brown (1773-1858) publishes his 'Prodromus Floræ Novæ Hollandiæ,' &c.
1811. Charles Bell (1774-1842) asserts the difference of sensory and motor nerves.
1813. Brewster (1781-1868) begins his experiments on refraction and dispersion.
1813. Davy discovers iodine.
1813. Wollaston publishes his synoptical scale of equivalents.
1814. Wells (1757-1817), essay on dew.
1815. William Smith (1769-1839) publishes his work on 'Strata.'
1815. Brewster gives his law for determining the polarising angle.
1815. Leslie (1766-1832) experiments on radiant heat and temperature of the earth.
1816. Prout (1785-1850), Memoir on the position of hydrogen.
1817. Young (in a letter to Arago) suggests transverse vibrations of light.
1819. Kater (1777-1835) measures the length of the seconds-pendulum.
1821. Faraday (1781-1867) discovers the rotation of a coil round a fixed magnet.
1821. Brown, monographs on botanical subjects.
1821. Sabine (1788-1883) experiments on the dip of the magnetic needle.
1823. Rowan Hamilton (1805-65) presents his paper on Caustics to the Irish Academy.
1823. Faraday condenses chlorine and other gases.
1824. Sir J. Herschel (1792-1871), observations of double stars.
1825. Sir J. Herschel, on the parallax of fixed stars.

[1] The two important generalisations I refer to are contained in:
1. George Green, 'An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism,' published at Nottingham by private subscription in 1828. The term "potential function," to denote the sum (V) obtained by adding together the masses of all the particles of a system, each divided by its distance from a given point, or in mathematical language $V=\int\frac{dm}{r}$, occurs there for the first time. See Green's mathematical papers, ed. Ferrers, 1871, p. 22. The function had before that time been used by Legendre and Laplace, but Green was the first to give a general mathematical theory of it. His essay remained unknown to the mathematical world, and the principal theorems were independently published by Gauss in his celebrated essay 'Allgemeine Lehrsätze über die im verkehrten Verhältnisse des Quadrats der Entfernung wirkenden Anziehungs- und Abstossungs-Kräfte,' 1839.
2. W. Rowan Hamilton's memoirs in the 'Philosophical Transactions' of 1834 and 1835, preceded by his theory of systems of rays in the 'Transactions of the Royal Irish Academy,' 1828. In these papers is contained his celebrated principle of varying action, which is a development of Maupertuis's principle of least—or stationary—action. A great deal has been written on this principle, which is now considered to be the most general principle of dynamics, as well for its mathematical usefulness in calculations (see Kirchhoff, 'Vorlesungen über mathematische Physik,' vol. i. pp. 28, 29), as from a physical point of view (Helmholtz, in 'Journal für Mathematik,' vol. 100). It has gained this importance since the conception of energy, or power to do work, has been placed at the base of the theory of all physical processes.

magnetic phenomena novel conceptions, the value of which other fifty years have hardly sufficed to realise —is, indeed, an extraordinary fact well worthy of careful examination. Certainly the language in which Cuvier with truth congratulates the French nation on the pre-eminence which it has attained in all branches of science contrasts strangely with the repeated attacks made in periodical literature, and in special pamphlets, on the state of science in England. And these not by persons ignorant of the great names and signal achievements just mentioned, but by men of note, occupying all but the very first places among the scientific men of this country.

It will suffice to give only two out of many examples of this criticism.

5. Criticisms of Playfair.

One of the earliest complaints regarding the culture of higher mathematics in this country will be found in an

excellent review of Laplace's 'Mécanique céleste' by Playfair in the 'Edinburgh Review' of 1808.[1] "In the list of the mathematicians and philosophers to whom the science of astronomy for the last sixty or seventy years has been indebted for its improvements, hardly a name from Great Britain falls to be mentioned.[2] . . . Nothing prevented the mathematicians of England from engaging in the question of the lunar theory, in which the interests of navigation were deeply involved, but the consciousness that in the knowledge of the higher geometry they were not on a footing with their brethren on the Continent. This is the conclusion which unavoidably forces itself upon us. . . . We will venture to say that the number of those in this island who can read the 'Mécanique céleste' with any tolerable facility is small indeed. If we reckon two or three in London and the military

[1] 'Edinburgh Review,' vol. ii. p. 279, &c. John Playfair (1748-1819) was a native of Forfarshire, and Professor of Mathematics, and later of Natural Philosophy, at the University of Edinburgh. "Playfair was struck with the backwardness of the English mathematicians in adopting the results of the Continental analysts. While they boasted of Newton, they were unable to follow him, and the mantle of Newton had indeed passed over to France, where it rested ultimately on the shoulders of Laplace. Playfair accordingly set himself to diffuse among his countrymen a knowledge of the progress which science had been making abroad. This he did in a variety of ways,—by his articles in the 'Encyclopædia Britannica,' by his papers in the Transactions of learned societies, by his articles in the 'Edinburgh Review,' and by his class-teaching. As David Gregory introduced the Newtonian philosophy, so Playfair introduced the Continental methods into the studies of the University of Edinburgh" (Sir A. Grant, 'The Story of the University of Edinburgh,' vol. ii. p. 302).

[2] Playfair here excepts his countryman, Colin Maclaurin (1698-1746), "in whose time the teaching of mathematics at Edinburgh reached a point which it cannot be said to have yet surpassed" (ibid., vol. ii. p. 299; cf. also vol. i. p. 271, where a programme published in 1741 is given of the mathematical and physical lectures at Edinburgh, which surpassed probably at that time the teaching of any other English or Continental university). Playfair might have excepted also Ivory and the Englishman Landen, both of whom were well known among Continental mathematicians.

schools in its vicinity, the same number at each of the two English universities, and perhaps four in Scotland, we shall hardly exceed a dozen, and yet we are fully persuaded that our reckoning is beyond the truth."

6. Babbage's criticisms.

The other opinion I am going to quote dates from more than twenty years later, and is contained in a pamphlet by Charles Babbage,[1] who with Herschel and Peacock had done much to introduce at the University of Cambridge that knowledge of Continental mathematics which, according to the Edinburgh Reviewer, was so much needed. His 'Decline of the State of Science in England' (1830) was directed mainly against the Royal Society, as the review

[1] Charles Babbage (1792-1871), a native of Devonshire, well known all over Europe through his calculating machine, was a very remarkable and original man. He lived during the age when the application of machinery to manufactures, trades, and arts produced the great reform in the industrial system of this country, and his talents, which might well have been employed in promoting pure science, were largely spent in solving problems of practical interest. An account of these several pursuits and schemes is given in his 'Passages from the life of a Philosopher,' London, 1864. Of his analytical machine we shall have occasion to speak hereafter (see p. 248). Of the beginnings of the new school of mathematics at Cambridge he gives the following account (p. 27). Having purchased for seven guineas a copy of Lacroix's 'Differential and Integral Calculus,' he went to his public tutor to ask the explanation of one of his difficulties. "He listened to my question, said it would not be asked in the Senate House, and was of no sort of consequence, and advised me to get up the earlier subjects of the university studies." Repeated experience of this kind had the effect that he acquired a distaste for the routine studies of the place, and devoured the "papers of Euler and other mathematicians scattered through innumerable volumes of the Academies of Petersburg, Berlin, and Paris." He then perceived "the superior power of the notation of Leibniz." It being an age for forming societies for printing and circulating the Bible at Cambridge, Babbage conceived the plan of a society for promoting mathematical analysis, and to parody one of the many advertisements he proposed to call it a society for promoting "the Principles of pure *d*'ism (*d* being Leibniz's symbol) in opposition to the *dot*-age (*dots* being Newton's notation) of the university." The most important result of this movement was the publication in 1816 of a translation of Lacroix's treatise, and of two volumes of examples in 1820.

of Playfair was against the English universities.[1] "That science has long been neglected and declining in England is not an opinion originating with me, but is shared by many, and has been expressed by higher authority than mine."[2] The author then proceeds to give extracts from the writings of Davy, Herschel, and others on this subject. "It cannot," he says, "have escaped the attention of those who have had opportunities of examining the state of science in other countries, that

[1] Some of the causes of the decline as given by Babbage are interesting, the more so if we remember that they were written at the period which marked the culmination of *Wissenschaft* in another country (p. 10): "The pursuit of science does not in England constitute a distinct profession, as it does in many other countries. . . . Even men of sound sense and discernment can scarcely find means to distinguish between the possessors of knowledge merely elementary and those whose acquirements are of the highest order. This remark applies with peculiar force to all the more difficult applications of mathematics; and the fact is calculated to check the energies of those who only look to reputation in England." In 1794 Professor Waring of Cambridge wrote: "I have myself written on most subjects in pure mathematics, and in these books inserted nearly all the inventions of the moderns with which I was acquainted; . . . but I never could hear of any reader in England, out of Cambridge, who took the pains to read and understand what I have written;" and "he then proceeds to console himself under this neglect in England by the honour conferred on him by d'Alembert, Euler, and Lagrange" (see Todhunter, 'History of the Theory of Probability,' p. 453). Babbage remarks (p. 13) that "in England the profession of the law is that which seems to hold out the strongest attraction to talent," that science is pursued as a favourite pastime, and that mathematics "require such overwhelming attention that they can only be pursued by those whose leisure is undisturbed by other claims." "By a destructive misapplication of talent we exchange a profound philosopher for but a tolerable lawyer" (p. 37).

[2] One of the causes given by the Edinburgh Reviewer of 1822 (vol. xxxvii. p. 222) is the following: "In Cambridge there must always be a great number of men devoted to scientific pursuits; but from the want both of the facilities and the excitements furnished by such an association, apt to lose the spirit of original investigation,—a remark peculiarly applicable to those young men who yearly distinguish themselves in the favourite studies of the University, and who, after the laborious course of discipline by which they have attained the first object of their ambition, are prone, if left alone, to become the mere instruments for enabling others to pursue the same course."

in England, particularly with respect to the more difficult and abstract sciences, we are much below other nations, not merely of equal rank, but below several even of inferior power."

"It is," says the Edinburgh Reviewer of 1816,[1] "certainly a curious problem with respect to national genius, whence it arises that the country in Europe most generally acknowledged to abound in men of strong intellect and sound judgment should for the last seventy or eighty years have been inferior to so many of its neighbours in the cultivation of that science which requires the most steady and greatest exertions of understanding, and that this relaxation should immediately follow the period when the greatest of all mathematical discoveries had been made in that same country."

7. Foreign opinions on English science.

It must be said that these opinions, expressed as they were by men of the highest attainments, did not remain unchallenged at home or unnoticed abroad. It will be interesting to see how they have been met. Let us first hear what Cuvier says in his Éloge of Sir Joseph Banks in 1821[2] regarding the work of the Royal Society during the period of forty-one years of his presidency: "During this period, so memorable in the history of the human mind, English philosophers have taken a part as glorious as that of any other nation in those labours of the intellect which are common to all civilised peoples: they have faced the icy regions of both poles; they have left no corner unvisited in the two oceans; they have increased tenfold the catalogue of the kingdoms of nature; the

[1] 'Edinburgh Review,' 1816, vol. xxvii. p. 98.

[2] See Cuvier, 'Éloges historiques,' vol. iii. p. 79.

heavens have been peopled by them with planets, with satellites, with unheard-of phenomena; they have counted, so to speak, the stars of the Milky Way: if chemistry has assumed a new aspect, the facts which they have furnished have mainly contributed to this change: inflammable air, pure air, phlogisticated air, are due to them; they have discovered how to decompose water; new metals in great number are the outcome of their analysis; the nature of the fixed alkalis has been demonstrated by none but them; mechanics at their call have worked miracles, and have placed their country above others in nearly every line of manufacture." Another foreigner, Professor Moll of Utrecht, remarked in his reply to Mr Babbage's pamphlet[1]: "If Mr Herschel and some of his friends

[1] The pamphlet was entitled 'On the alleged Decline of Science in England.' By a Foreigner. London, 1831. It was by Dr Moll of Utrecht, and was introduced by a few lines from Faraday, who, without taking any side in the question, remarked that "all must allow that it is an extraordinary circumstance for English character to be attacked by natives and defended by foreigners." In the discussion on the subject by this writer, as also by Babbage, Herschel, Playfair, Whewell—*pro* and *con.*—a good many points of importance are brought out: some of them are still interesting, others refer to defects which have since been remedied. I will mention a few of them. Playfair, in the 'Edinburgh Review' (vol. xxxi. p. 393, 1819), thinks that the "very extensive dissemination of general knowledge, which is so much the case over the whole of this kingdom," is against the advancement of the higher branches of mathematics. This refers probably to the absence of periodicals devoted to special sciences, such as the 'Annales de Chimie et de Physique,' published by Arago and Gay-Lussac in France. In the absence of these special organs, memoirs of original value, which marked an era in special researches, were scattered in general literary reviews, as Young's on Light and Hieroglyphics in the 'Quarterly,' Herschel's and Airy's in the 'Encyclopædia Metropolitana'; and much good mathematics was buried in the 'Ladies' Diary' among poetry of the "worst taste" and "childish scraps of literature and philosophy" ('Edin. Rev.,' vol. ii. p. 282, 1808). Another point is that "the researches of English men of science have been too much insulated from each other and from what is doing in other countries" (Whewell to Vernon Harcourt, 1831; see Life by Todhunter, vol. ii. p. 126). The British Association, which was founded very much as a result of this agitation,

have such a poor opinion of the English scientific journals, a different judgment is entertained abroad, as is well proved by the eagerness with which the German journalists seize upon every article issuing from the presses of their British colleagues. The value which is set in Germany upon the scientific pursuits of the English, the rapidity with which translations are made in Germany of whatever English philosophers of some reputation publish, shows abundantly that in that country at least, in *docta Germania*, a far greater value is set upon the productions of English science than is done by Mr Herschel and his friends."[1]

has remedied this defect; and special periodicals exist now in multitudes; but who could say that a third point has been sufficiently attended to—*viz.*, "the ignorance of foreign languages, which prevails both in England and in France: in England the number of those who acquire a smattering of French is very small, and still smaller is the number of those who know enough of German to read a book in that language without considerable trouble" (Dr Moll, *loc. cit.*, pp. 7, 8). A fourth defect existing at that time is worth mentioning, as we have long left the age of such drawbacks; it "is the high price in England of foreign books, in consequence of an importation duty." The paper duties were repealed in 1861.

[1] Moll, *loc. cit.*, p. 7. Another passage is of interest, as bearing upon the difference between the culture of science in England and in France: "At the time of the French Revolution it so happened, by the exertions of d'Alembert, Clairault, Condorcet, and others, that of all sciences mathematics were the most fashionable. . . . With this view the Ecole Normale was founded, which, though of short duration, was perhaps of more utility towards the extension of mathematical knowledge than all the universities of Europe together. It was there that Laplace, Lagrange, and Monge were lecturers, and men like Lacroix among the hearers. The study of classics having been in a great measure abolished by the French Revolution, mathematics were studied in its stead; and it thus happened that a number of mathematicians, unusually great, were scattered over the soil of France, and every one thought himself capable *de faire les x*, as they themselves called it, upon any given subject. But most of these investigations were all theoretical, and practical applications were foregone in almost every instance" (p. 11). "Mechanics in particular do not seem accessible, according to the tenets of the French school, to any man not well versed in sublime analysis. . . . Hence it arises that many have acquired a profound knowledge of the higher branches of mathematics, whilst the more elementary part of mathematics, which leads to the

8. English replies to Babbage, &c.

The answers to the challenges of Babbage and the Edinburgh Reviewer given by English writers themselves cannot on the whole be said to be very reassuring. One of them counts the scientific periodicals in England and in France, but omits to weigh the merit of their respective contributions. Another points to the 'Ladies' Diary,' in which many curious mathematical problems, far beyond the mere elements of science, are often to be met with. A third, whilst in general admitting the correctness of Babbage's strictures, draws attention to the 'Penny Magazine' and the 'Cabinet Cyclopædia' as counterparts in England of the Reports of Cuvier and Berzelius abroad. The true position was probably recognised by the founders of the British Association for the Advancement of Science about 1830,[1] who saw that, be-

9. Foundation of the British Association.

most useful applications, is far less diffused in France than in England" (p. 12). "The principle of the division of labour [in science] is more acted upon in France than in England" (p. 14).

[1] The movement, which originated in the circle to which Babbage belonged, was — as stated above, p. 42 — to some extent copied from the German Association founded by Oken in 1822. The latter acquired a kind of European renown through the exertions of Humboldt in 1828, who succeeded in attracting a considerable number of celebrities—such as Gauss, Berzelius, Oerstedt, — who for themselves preferred a solitary to a "gregarious" mode of science. Babbage was a guest at this meeting at Berlin, and gave an account of it in an appendix to the 'Decline of Science.' A good account of the character and gradually declining influence of these German meetings will be found in Bruhns' 'Life of Humboldt' (vol. ii. p. 127, &c., translation). They "degenerated after the usual German fashion into the unintellectual form of feasting." The British Association for the Advancement of Science, founded shortly afterwards on the 27th September 1831 at York, was the immediate outcome of a suggestion thrown out by Brewster at the end of a review in the 'Quarterly' of Babbage's 'Decline of Science.' He fully endorsed the latter's opinion, and was even more severe upon the universities, maintaining "that the great inventions and discoveries which have been made in England during the last century have been made without the precincts of our universities. In proof of this we have only to recall the labours of Bradley, Dollond, Priestley, Cavendish, Maskelyne, Rumford, Watt, Wollaston, Young, Davy, and Chevenix; and among the living to mention the names of Dalton, Ivory, Brown, Hatchett, Pond, Herschel,

sides a number of separate societies, "concentration was needed in one association in order to give more systematic direction to scientific inquiry, and that the first thing needed would be to procure reports on the state and the desiderata of the several branches of science." Babbage, at the Oxford meeting in 1832, "expressed the general feeling that meetings should be held in places likely to bring science into contact with that practical knowledge on which the wealth of the country depends." There is also no doubt that in the course of half a century the British Association has done a very extensive service to science in the direction of supplying the wants which its early founders clearly defined, and in bringing about that concerted action and scientific co-operation which so highly distinguishes the great academies and universities of France and Germany.[1] It has done so without altogether destroying that peculiar feature which characterises not only the scientific but all the forms of the higher mental work of this country. In no country has the voice of public criticism been so free to unveil the shortcomings which attach to all—even the highest—human effort. In England there has existed for a long time the habit of promoting advance in every department by the cultiva-

10. Characteristics of higher mental work in England.

Babbage, Henry, Barlow, South, Faraday, Murdoch, and Christie; nor need we have any hesitation in adding that within the last fifteen years not a single discovery or invention of prominent interest has been made in our colleges, and that there is not one man in all the eight universities of Great Britain who is at present known to be engaged in any train of original research" ('Quarterly Review,' vol. xliii. p. 327, 1830). He then suggests "an association of our nobility, clergy, gentry, and philosophers" (p. 342).

[1] The British Association has from the beginning had two features which did not exist in the German society — first, the Reports on the position of various branches of science, delivered by specialists of the highest ability; and, secondly, the Committees, which undertake to do special work requiring concerted action.

tion of party spirit, party criticism, and party shibboleths, as the easiest method of enlisting popular favour[1] and individual interest; for here there exists no central authority which can create powerful organisations or disburse public means without the distinctly and repeatedly expressed support of a large section of the people. But all this must not induce us, in our historical survey, to dwell on the defects rather than on the excellence of the British contributions to the growth and the diffusion of science. Brilliant is undoubtedly the array of British names which have during the first half of this century become immortal by scientific labours, and it would be narrow-minded simply to emphasise the fact that they have not done so by the same means and through the same organisations as the Continental nations have established and perfected. For we must not forget that these even, with all their rightly extolled universality and breadth of spirit, have sometimes failed to recognise merit or to encourage genius. In spite of the impartial dealings of the Institute, on which Cuvier congratulates the French people, there are several instances in which contributions of the first order lay unnoticed for many years.

11. Academies and universities not always impartial.

[1] Referring to the British Association itself, Charles Lyell wrote in 1838, after the Newcastle meeting, to Charles Darwin: "Do not let any papers, whether of saints or sinners, induce you to join in running down the British Association. I do not mean to insinuate that you ever did so, but I have myself often seen its faults in a strong light, and am aware of what may be urged against philosophers turning public orators, &c. But I am convinced, although it is not the way I love to spend my own time, that in this country no importance is attached to any body of men who do not make occasional demonstrations of their strength in public meetings. It is a country where, as Tom Moore justly complained, a most exaggerated importance is attached to the faculty of thinking on your legs, and where, as Dan O'Connell well knows, nothing is to be got in the way of homage or influence, or even a fair share of power, without agitation" ('Life, Letters, and Journals of Sir C. Lyell,' London, 1881, vol. ii. p. 45, &c.)

Fourier's great work on the theory of heat, which for the first time propounded a universal method applicable to the mathematical treatment of almost every physical problem, inasmuch as it, so to speak, follows nature into the marvellous composition of the many movements out of which all her phenomena are compounded, lay buried for fourteen years in the archives of the Institute. That great authority had failed to recognise its paramount importance.[1] Fresnel's first memoir, which established on a firm mathematical basis the undulatory theory of light, was for years left unpublished, whilst the whole scientific world was anxiously expecting the results of his inquiries.[2] In Germany we have examples of similar

12. Fourier.

13. Fresnel.

[1] Jean Bapt. Jos. Fourier (1768-1830), of humble origin, in his celebrated 'Théorie analytique de la Chaleur' (Paris, 1822), and in previous memoirs, carried further the mathematical treatment of physical phenomena and introduced wider conceptions of mathematical quantities and their dependence—*i.e.*, of a mathematical "function." His investigations have led to far-reaching applications in physical science (Ohm and Lord Kelvin), and to profound mathematical theories (Dirichlet, Riemann, &c.) The so-called "Fourier" series has thus a great applied as well as theoretical interest. Fourier's first memoir was presented to the Institute in 1807; an extract was published in 1808; a second memoir was presented in 1811 and crowned, but was not printed till 1824, two years after the great work itself had appeared. On the physical importance of Fourier's analysis see Helmholtz, 'Vorträge und Reden,' vol. i. p. 101, &c.; Sir W. Thomson, Mathematical and Physical Papers, *passim*, but especially vol. ii. p. 41, &c. On the purely mathematical interest that attaches to the Fourier series see especially Riemann, 'Mathematische Werke,' p. 218, &c. A very concise summary of the history of the series is also given by George A. Gibson in the 'Proceedings of the Edinburgh Mathematical Society,' vols. xi. and xii. We shall revert to this subject in a subsequent chapter.

[2] Augustin Fresnel (1788-1827) divides with Thomas Young the merit of having established the undulatory theory of light on a firm basis. His first memoir on Diffraction of Light was presented to the Academy in 1815, a more extensive paper in 1818; this was crowned in 1819, but not printed till 1826. Other papers of his were mislaid or lost. The delay in bringing before the world these important discoveries has been attributed to the opposition of Laplace and his party in the Institute, which even the influence of Arago could not overcome. See what Sir John Herschel says in 1827, referring to Fresnel's memoir of 1821 on

discouragement and neglect being thrown in the way of the growth of new ideas. 14. Plücker. Plücker of Bonn laboured for many years on the union of the geometrical and analytical methods in the treatment of geometry; but he found so little appreciation that he abandoned his investigations, and only resumed them when in after-years a similar line of thought was independently developed in England.[1]

Transverse Vibrations, which the Academy had recommended to be printed: "We are sorry to observe that this recommendation has not yet been acted upon, and that this important memoir, to the regret and disappointment of men of science throughout Europe, remains yet unpublished" ('Ency. Metrop.,' article "Light"). A full account of the opposition and difficulties which both Young and Fresnel had to encounter will be found in Whewell's 'History of the Inductive Sciences,' vol. ii. In earlier times Réaumur seems to have exercised a similar tyranny in the Academy of Sciences: see Maury, 'Les Académies d'autrefois,' vol. i. pp. 280, 123; also Huxley, 'Critiques and Addresses,' 1890, p. 112, &c.

[1] Julius Plücker (1801-68), professor at Bonn, equally known in England by his scientific co-operation with Faraday and by that with Cayley and Salmon, worked both in physics and geometry on independent lines. In the latter especially he brought about that union of purely geometrical and algebraic methods which has become so fruitful in the development of modern geometry and modern algebra. He had two periods of original geometrical work. The first began in 1826 (the year of the revival of mathematics in Germany), and closed in 1846. His mathematical researches were little noticed in his own country, whereas in France, and still more in England, his name was well known. After having published in 1846 a 'System of Geometry,' which contained his former results in a more methodical form, he dropped his mathematical researches for twenty years, during which time he devoted himself to physical investigations of great originality. By these, if he had not been a personal friend, he might almost have been called a rival of Faraday (G. Chrystal in 'Ency. Brit.') During a visit to England in 1864 he was agreeably surprised to meet with appreciative interest from English geometricians, who had independently worked on the same lines as he had done twenty years earlier. He was thus induced to resume his favourite studies, and to develop an idea which had already been expressed in his last-named work of 1846. This led to a new fundamental conception of geometrical forms, in which not the point but the line is the element of space. He was not spared to complete this line-geometry, but after his death his pupils found sufficient material to put his researches into a systematic form under the title, 'Neue Geometrie des Raumes, gegründet auf die Betrachtung der geraden Linie als Raumelement' (Leipzig, 1868 and 1869). See Clebsch on Julius Plücker, Göttingen, 1872. A very appreciative notice of Plücker, by George Chrystal, will be found in the 9th edition of the 'Encyclopædia Britannica.'

Grassmann, in his 'Ausdehnungslehre,' published in 1844, 15. Grassmann. is now generally admitted to have originated quite a novel way of considering geometrical relations.[1] It took twenty years, however, before he succeeded in attracting any attention, and his great work, of which the first edition had been sold as waste-paper, was later on reprinted in its original form—mathematicians having now begun to study and recognise its intrinsic value. Such cases of neglect have undoubtedly been much more frequent in England, where even at the present day no central organisation 16. Central organisation wanting in England. exists which annually collects and arranges the scattered labours of individual workmen, and where that historical and encyclopædic spirit is wanting which does its utmost to guarantee completeness and thoroughness of search and of research. Men of the greatest eminence, pioneers

[1] Hermann Grassmann (1809-77) was born, lived, and died at Stettin. He did not succeed till late in life, and fully thirty years after he had published his original investigations in geometry, in gaining for these the recognition and appreciation which they deserved. Neither he nor even Jacob Steiner at Berlin attained to positions worthy of their ability; the latter, in spite of his connection with other great mathematicians, never filled the chair of an ordinary professorship, whilst the former never entered the sphere of university teaching at all. The 'Ausdehnungslehre,' as a new branch of mathematics, appeared in 1844. It is a science of pure extension, the application of which to empirical space is geometry. Similar investigations, in which space of three dimensions is considered to be merely a particular case of pure extension of any number of dimensions, which are not necessarily determined by the same properties as our empirical space, have become familiar since the publication of Riemann's celebrated dissertation of 1854 (published in 1867), and since Helmholtz was led to similar investigations by considering the different dimensions or manifoldnesses of our sense perceptions (see his 'Vorträge und Reden,' in many passages). Grassmann, who at the end of his life witnessed the growing appreciation of his ideas, had filled up the interval with entirely different studies, the translation of the 'Rig-Veda' (Leipzig, 1876-77), and the composition of a dictionary to the same (1872-75). He seems to have been the only mathematician, besides Thomas Young, who combined the ability for exact mathematico-physical and for philological studies. Both can complain of having been very insufficiently appreciated by their contemporaries. See Victor Schlegel, 'Hermann Grassmann,' Leipzig, 1878.

in their line of thought and discovery, have to the present day remained popularly unknown to their countrymen, who have not only neglected but reviled them, allowing their great discoveries to be taken up as their own by
17. Thomas Young. foreigners. Such was Dr Thomas Young, whom many educated persons at the present day cannot distinguish from the author of 'Night Thoughts.'[1] The great founder

[1] Thomas Young (1773-1829), a native of Somersetshire, attained equal eminence by his discoveries in connection with the undulatory theory of light, in which he was the first to assert the principle of interference and that of transverse vibrations, and by his discovery of the key to the system of hieroglyphics. Of his discoveries and suggestions some were published in anonymous review articles (so especially his hieroglyphical papers); some in his Lectures on Natural Philosophy, delivered early in the century at the Royal Institution, and published 1807; some in the 'Transactions of the Royal Society' (from 1800 onwards); and some in various collective works, especially the 'Encyclopædia Britannica.' The remarkable fact that Young, of whom Helmholtz says ('Vorträge und Reden,' vol. i. p. 279) that he came a generation too soon, remained scientifically unrecognised and popularly almost unknown to his countrymen, has been explained by his unfortunate manner of expression and the peculiar channels through which his labours were announced to the world. His frequently unintelligible style, his obscure and inelegant mathematics, the habitual incognito which he preserved, his modesty in replying to attacks, and his general want of method in enunciating his ideas, contrast very markedly with the writings of some of his rivals, especially in France, where the qualities of style, method, and elegance were highly developed, and where recognised organs existed for the publication of works of genius. The historian of thought, however, must not omit to state that several great names contributed, by the authority they commanded, to oppose Young's claims to originality and renown. Lord Brougham, shielded by the powerful anonymity of the 'Edinburgh Review,' and ostentatiously parading the authority of Newton, submitted the views of Young to a ruthless and unfair criticism, the popular influence of which Young probably never overcame. The great authority on optics, Brewster, who has enriched that science by such a number of experiments and observations of the first importance, never really adopted the theories of Young and Fresnel. In the other great branch of research with which Young's name is now indissolubly connected, in the science of hieroglyphics, the authority of Bunsen decided against Young and for the Frenchman Champollion. But this decision, which did so much to obscure the merits of Young, was founded on an insufficient knowledge of the dates of Young's publications. Since these were collected by Leitch in the third volume of the 'Miscellaneous Works' of Dr Young (London, 1855), the chronology of his discoveries, which begin in 1814, has been well established. See Benfey, 'Geschichte der Sprachwissenschaft' (München, 1869, p. 729). Bunsen pronounced his verdict in his well-known work, 'Egypt's Place in Universal History,' published in 1845-57. On the whole, the words of Peacock, 'Life of Dr Young' (London, 1855), p. 472, are still correct: "His scientific works were rarely read and never appreciated by his contemporaries, and even now are neither sufficiently known nor adequately valued; whilst if justice was awarded more promptly and in more liberal measure by his own countrymen to his hieroglyphical labours, these also were singularly unfortunate, as far as concerned the general diffusion of his fame, by coming into collision with adverse claims, which were most unfairly and unscrupulously urged in his own age, and not much less so by some distinguished writers in very recent times."

of modern chemistry, who next to Lavoisier did more than any one else to introduce into this science mathematical ideas, John Dalton, grew old and infirm before 18. Dalton.
his countrymen sufficiently recognised and honoured him. Deprived of all but the very meanest apparatus for the proofs of his theories, and yet able to do what he did, what might not such a genius have accomplished if he had possessed the means of a Gay-Lussac or a Regnault?[1]

[1] John Dalton (1766-1844), a native of Cumberland, spent the greater part of his life in teaching elementary mathematics at Manchester, first at a college and then privately. In 1801 he propounded the law known under the joint name of Dalton and Gay-Lussac (who stated it six months later). In the years immediately following he elaborated his atomic theory, which was to account for the existence of those definite quantitative relations between the chemical constituents of bodies known already to Richter. It was published in 1805. But the man who did most to make known to chemists the ideas of Dalton was Thomas Thomson (1773-1852), Professor of Chemistry at Glasgow, who in 1807, in the 3rd edition of his 'System of Chemistry,' gave an account of the atomic theory based upon communications of Dalton. Two memoirs published in the 'Philosophical Transactions' of 1808—one by Thomson on "Oxalic Acid," and one by Wollaston on "Super-Acid and Sub-Acid Salts"—pointed to the great importance of the atomic theory, which (Wollaston prophetically added) would not stop short with the determination of the relative weights of elementary atoms, but would have to be completed by a geometrical conception of the arrangement of the elementary particles in all the three dimensions of solid extension. The real merit of having experimentally proved the theory of Dalton belongs to Berzelius, whereas Sir Humphry Davy opposed it for many years after it had been accepted abroad. Dalton himself by no means followed the development which his ideas underwent at the hands of others. For example, he opposed Gay-Lussac's law of volumes. He was on the whole more successful in working out his own

19. Faraday. Faraday, instead of being backed by a wealthy Academy and ample assistance, had during all the years when his great discoveries were being made, to keep alive, with an income scarcely exceeding a hundred pounds a-year, an institution which but for him the memory even of such names as Rumford, Young, and Davy would not have sufficed to preserve from utter ruin and collapse.[1] The author of one of the most suggestive treatises in the application of mathematics to physical phenomena,
20. Green. George Green, published it in 1828 at Nottingham by private subscription. Seventeen years later, William Thomson (Lord Kelvin) tried in vain to procure a copy

ideas than in comprehending those of others who, like Berzelius, Mitscherlich, Laplace, Liebig, and many later, contributed to the confirmation of the atomic theory. A good account of this is given in Henry's 'Life of Dalton' (1854) and in Kopp's 'Entwickelung der Chemie in der neueren Zeit' (München, 1873).

[1] Michael Faraday (1791-1867), though not a mathematician, introduced into the science of electricity those ideas which have since been developed into a mathematical theory approaching in completeness the mathematics of the undulatory theory of light. What the atomic theory has done for chemistry, Faraday's lines of force are now doing for electrical and magnetic phenomena. Dalton, though unacquainted with the higher mathematics of the French school, had essentially a mathematical or arithmetical mind. Faraday's peculiar ideas on the nature of electrical and magnetic action, though supported by an experimental knowledge many times surpassing in volume and accuracy that of Dalton, did not find much appreciation among his contemporaries. They were much more interested in his experimental researches than in his theories. In France and Italy Faraday's eminence was recognised early. Already in 1823 he was elected member of the Academies of Paris and Florence, almost before any society at home had received him. "The circumstances under which Faraday's work was done were those of penury. During a great part of the twenty-six years the Royal Institution was kept alive by the lectures which Faraday gave for it. 'We were living,' as he once said to the managers, 'on the parings of our own skin.' He noted even the expenditure of the farthings in research and apparatus. He had no grant from the Royal Society, and throughout almost the whole of his time the fixed income which the Institution could afford to give him was £100 a-year, to which the Fullerian professorship added nearly £100 more" (Bence Jones, 'Life and Letters of Faraday,' London, 1870, vol. ii. p. 344). See also Bence Jones, 'The Royal Institution,' p. 311.

of this document, of which he knew by a reference in another work. At last he got possession of a copy which had probably during all this time been buried in the library of a prominent mathematical tutor at Cambridge, with whom he had been in frequent intercourse. Thomson then took it with him to Paris, where Sturm and Liouville at once recognised its merits. He then published it in 'Crelle's Journal,' where it has ever since been referred to as a fundamental essay on the so-called potential theory.[1] One of the most original thinkers on mathematics, who introduced a novel principle into algebraical science, George Boole, never at- 21. Boole.
tained to a higher position than that of teacher at a remote Irish provincial College.[2] But perhaps the most signal example of the want of support which the

[1] See note 1 to p. 231; also Sir William Thomson, reprint of papers on "Electrostatics and Magnetism," 2nd ed., London, 1884, p. 2, note; p. 126, note.

[2] George Boole (1815-64), a native of Lincolnshire, was one of the few great and original mathematicians who, like Leibniz and Grassmann, and to some extent Gauss, looked at the logical as well as the purely arithmetical side of the language of symbols. Though his treatises on 'Differential Equations' (1859) and on 'Finite Differences' (1860) have become well-known text-books, and his 'Laws of Thought' (1854), in which he examined the foundations of the mathematical theories of logic and probabilities, remains a unique work, his principal services to science lie in the direction of the "calculus of operations." In this branch of mathematics, which is peculiar to England, the symbols indicating an arithmetical operation are separated from those denoting quantity and treated as distinct objects of calculation. In connection with these investigations, many of which have now penetrated into ordinary text-books, Boole was led to examine the conditions under which and the forms in which algebraical expressions, whilst undergoing changes and transformations, remain, nevertheless, unaltered (invariant) (1841). By introducing this point of view he has, so to speak, created modern algebra; founding the extensive and fruitful science of "Invariants." Of this we shall treat later on. I now only refer to the further development of this subject in the hands of Cayley and Sylvester, and to the valuable sketch of the history of this branch of mathematics by Dr F. Mayer in the first volume of the 'Jahresbericht der deutschen Mathematiker-Vereinigung,' Berlin, 1892.

wealthiest of nations has shown to scientific genius is 22. Babbage. to be found in the history of Babbage's calculating engine. Yet this machine was approved by all experts —English and foreign—during the inventor's lifetime; and the Report of a Commission of the British Association appointed specially to examine into the matter, concluded by stating that the scheme was perfectly feasible, and might, if carried out, mark an invention as great probably as that of logarithms.[1] Who among us who has been interested in the promotion of institutions for higher education has not a story to tell of pecuniary troubles, continued through many a long year, whilst the wealth of the country seemed to exert its influence only in the direction of making the demands on a struggling establishment more formidable, the expenses more difficult to defray?[2]

[1] On Babbage see p. 233, note 1. The history of the "difference engines" and the "analytical engine" is given by Babbage himself in his 'Passages from the Life of a Philosopher.' See also Weld, 'History of the Royal Society,' vol. ii. p. 369, &c.

[2] Like the Royal Society, which for a century had to struggle with poverty, the Royal Institution has a story to tell of want of funds through a long period of its early existence. See Bence Jones, 'The Royal Institution,' London, 1871, pp. 202, 281. The Royal Institution was founded by Benjamin Thomson, Count Rumford (1753-1814), and had originally not a scientific, hardly even a higher educational object. The scheme arose in the mind of its founder after he had successfully exerted himself at Munich under the patronage of the Elector of Bavaria in founding industrial workhouses, improving the state of the army, and putting down beggary and immorality in the capital and country. His principle was to make "vicious and abandoned people first happy and then virtuous" (p. 31). After leaving Munich in 1793 and spending two years in Italy, similarly occupied, he visited London in 1795 in order to publish his Essays, which appeared separately between 1796 and 1802. The first essay contained "a proposal for forming in London by private subscription an establishment for feeding the poor and giving them useful employment, . . . connected with an institution for introducing and bringing forward into general use new inventions and improvements," &c., &c. (p. 44). The first outcome of this was the formation of a society for encouraging industry and promoting

But it is hardly the duty of the historian of thought to record that which belongs more to the impediments of mental progress than to its promotion, were it not that in and through these peculiar circumstances the genius of the nation has developed its main features, its strong character. These are manifest as much in the department of science as they are in general literature and in the institutions of practical life. British science through all the centuries, since the time of Roger Bacon, and in spite of the efforts of his illustrious namesake, has 23. Characteristics of English thought.

the welfare of the poor. William Wilberforce was one of the original promoters; Thomas Bernard, the founder of many other charitable institutions, one of its most active members. To a committee of this Society Count Rumford submitted, in 1799, his proposals for forming the Royal Institution, and it was accordingly founded in February of that year on private subscriptions of fifty guineas each. It was described as a "public Institution for diffusing the knowledge and facilitating the general introduction of useful mechanical inventions and improvements, and for teaching by courses of philosophical lectures and experiments the application of science to the common purposes of life." In the course of a very few years the original character of the Institution entirely changed, the aim of influencing directly the condition of the poor was lost sight of, and little remained besides the result of "bringing science into some degree of fashion" and "affording a new employment and amusement to the higher classes of life." The interest of the Institution for the history of thought is the fact that in its laboratory Davy and Faraday conducted their researches, and that they, as well as Young, Coleridge, and Sydney Smith, there delivered their lectures. And the history of the Royal Institution is also typical of the history of other establishments for higher culture in this country: it has been in its main features repeated on a larger or smaller scale in many provincial societies, and notably in the colleges of Manchester, Birmingham, Liverpool, Newcastle, Leeds, Bristol, Nottingham, &c. Started by persons with large but nevertheless insufficient means, or by subscriptions and endowments of moderate extent, obliged to gain popularity and fashionable support in order to meet their growing expenses, these institutions have depended mostly on individual energy for their first successes, and have all had to pass through periods of great difficulty, till in course of years they have acquired a special character of usefulness and defined their peculiar sphere of action. The absence of a definite programme and a great waste of energy and funds over special departures are not uncommon features of these developments.

refused to congregate in distinct schools and institutions or to be localised in definite centres. The Royal Society, the Royal Institution, the British Association, and many other smaller societies, have all more or less started with the programme of Lord Bacon, and have failed to realise it: everywhere the schemes of co-operation or organised scientific research have encountered the opposition of individual pursuits or of local interests.

Newton could not secure the use of Flamsteed's observations, which on their part remained uncompleted and unpublished through the want of appreciation of others. Great schemes in practical life have been carried out by the unaided efforts of eminent persons, and great ideas have been put forward with all the power and

24. Absence of schools of scientific thought.

all the resources of individual genius,[1] but no great master in scientific research in this country can point to a compact following of pupils—to a school which undertakes to finish what the master has begun, to carry his ideas into far regions and outlying fields of research, or to draw their remoter consequences. Newtonianism was a creation of Voltaire; the school of Locke is to be found in France; the best realisation of Bacon's schemes are the Encyclopédie, the French Institute, and the foreign Academies.[2] Dr Young's discoveries in optics

[1] See Huxley, 'Lay Sermons, &c.,' edition of 1891, p. 43: "England can show now, as she has been able to show in every generation since civilisation spread over the West, individual men who hold their own against the world, and keep alive the old tradition of her intellectual eminence. But in the majority of cases these men are what they are in virtue of their native intellectual force, and of a strength of character which will not recognise impediments. They are not trained in the courts of the Temple of Science, but storm the walls of that edifice in all sorts of irregular ways, and with much loss of time and power, in order to obtain their legitimate positions."

[2] See above, pp. 34, 95.

and hieroglyphics were made known to the learned world through his French contemporaries. Dalton,[1] Charles Bell,[2] Faraday, Darwin, and Maxwell, no less than Bentley and Gibbon,[3] have furnished the text for lecture-courses in German universities, and created a whole literature of pamphlets and scientific memoirs.[4] English societies may sometimes honour and admire, but they do not support, their great representatives, and these themselves often refuse to be tied by exclusive academic duties, still more by official restrictions. Two characteristics have marked most of them: they have, at all expense and sacrifice, guarded their individual freedom

25. Individual character and practical tendency of English science.

of thought, and they have almost always shown a great desire to combine some application with their abstract researches, to take part in the great practical work of the nation. Continental thinkers, whose lives are devoted to the realisation of some great ideal, complain of the want of method, of the erratic absence of discipline, which is peculiar to English genius. The fascination which practical interests exert in this country appears to them an absence of full devotedness to purely ideal pursuits.[5]

[1] See above, p. 245, note.

[2] See above, p. 193, note.

[3] See above, p. 169, note.

[4] Germany may be said to have produced *Darwinismus* in this century as France created *Newtonianisme* in the last. Huxley writes ('Life of Darwin,' vol. ii. p. 186): "None of us dreamed (in 1860) that in the course of a few years the strength (and perhaps I may add the weakness) of *Darwinismus* would have its most extensive and most brilliant illustrations in the land of learning." Quite recently Prof. Boltzmann at Munich, and M. Poincaré, have published courses of lectures on Maxwell's electric theories.

[5] What appears irksome to an English genius—the red tape of academic restrictions, the barriers of officialism, and the duties of the teacher—melted away in the glow of enthusiasm and love of truth which animated the great leaders and founders of university culture abroad; as Goethe has told us that the rigid form of the sonnet melts in the fervour of the love-song:

"Das Allerstarrste freudig aufzuschmelzen
Muss Liebesfeuer allgewaltig glühen."
—Sonette No. 14.

The English man of science would reply that it is unsafe to trust exclusively to the guidance of a pure idea, that the ideality of German research has frequently been identical with unreality, and that in no country has so much time and power been frittered away in following phantoms, and in systematising empty notions, as in the Land of the Idea; but he would as readily admit that his own country is greatly deficient in such organisations for combined scientific labour as exist abroad, and that England possesses no well-trained army of intellectual workers.

26. English peculiarities more pronounced during early part of the century.

These differences between English and Continental science were most pronounced in the first half of the present century, when Germany developed her university system, when France clearly defined the exact scientific methods, and when the encyclopædic view—peculiar to the historical and philosophical pursuits of the earlier years—gradually became dominant in the exact sciences also. Since then the intercourse of the different nations has done much to destroy these national peculiarities. The reform of the universities, in which Germany was engaged in the early years of the century, did not touch the English universities before the middle of the century. In the meantime quite different demands had sprung up all through the civilised world; and as nothing repeats itself in history, it will be impossible to reach in this country the same broad organisation for purely intellectual work as Germany can rightly boast of during the period we are dealing with. Some persons doubt whether it will be maintained in Germany. It appears still more doubtful whether such an organisation could now be

created in the face of the industrial spirit of our age. Ever since the latter half of the eighteenth century schemes for a general education of the masses have attracted the thought and the attention of philanthropists and statesmen in many countries of Europe. But the directions taken by these educational efforts have been characteristically different in the different countries, and their success, so far as the great masses of the people are concerned, has been very partial indeed. It is true that during the first thirty years no country possessed such distinguished schools of science as did France in the great scientific and medical institutions of her capital. It is also true that no country equalled Germany in her system of universities and higher schools, which had come under the influence of classical learning and philosophical ideals. England, which at that time took no part in the educational movements of the Continent,[1] possessed, neverthe-

[1] This statement requires two qualifications. Firstly, both Milton and Locke have had great influence in spreading enlightened views regarding the principles and the object of education in general—especially in the direction of enlarging the idea of education, so as to make it comprise something more than merely instruction and pedantic teaching. I cannot find, however, that in England, either in the direction of higher university education or of a general system of popular education, their influence has been very marked. Locke's influence abroad, through his psychological analysis of the mind, has been very considerable. Secondly, in the direction of practical education, of the endeavour to reach large numbers of the people by educational institutions, we must look with admiration to the early work done in Scotland, which in this respect somewhat resembles Switzerland. The Scotch system of parochial schools, and their influence on the education of the people, has been too little studied abroad, though rightly extolled at home. It is true that, with the exception of Calvin, none of the great Continental educationalists—such as Fénelon, Rousseau, Pestalozzi, or W. von Humboldt—have had any direct influence on Scotland; nor has the educational work of Scotland produced any great educational literature like that which Switzerland can boast of, and which has brought the theory of education so prominently before the world. But nevertheless there it stands, this creation of John Knox and the early Reformers. "Civilised

27. Unique character of English universities.

less, something peculiar in her two great universities. It was neither the scientific, nor the classical, nor the philosophical spirit exclusively which reigned there; if any or all of them had ruled, we should not meet with those repeated complaints that higher mathematics were absent in Cambridge, that no philological studies were cultivated in either of the universities, and that philosophy was represented merely by Aristotle, Butler, Locke, and Paley.[1] According to the representatives of the university

Europe has never witnessed a nobler spectacle than the first Protestants of Scotland in the assembly of the nation demanding that from the funds before abused by a licentious superstition one-third should be devoted, not to increase the revenue of the Reformed Church, but to the education, the universal education, of the youth in all departments of instruction, from the highest to the lowest" ('North Brit. Rev.,' 12, p. 483).

[1] As to the deficient mathematical teaching at Cambridge, see p. 233, note, &c. The complaints regarding the teaching of other subjects are frequent, but belong to a later date, the middle of the century, when the Royal Commission of Inquiry, which was appointed under the Government of Lord John Russell on the 31st August 1850 and expired with the presentation of its report on the 30th August 1852, attracted the attention of the public to university reform, and gave rise to a very full discussion of the whole subject in the various literary papers and reviews. The two older universities are called "citadels of political prejudice and sectarian exclusiveness, instead of being the temples of liberal arts and the repositories of science" ('Brit. Quart. Review,' 1860, July, p. 205). Theology is stated to be "the last thing taught at Cambridge" (ibid., p. 221); there was no professor of Latin, none of English literature, of logic and metaphysics, of modern languages (p. 225). In 1849 Cambridge had no laboratory; the universities took no part in the legal training of lawyers ('Edin. Rev.,' April 1849, p. 511); Oxford afforded no training in natural science (ibid.) Cambridge "sacrificed to the monopoly of a severe geometry every other exercise and attainment of the human mind. There was no theological study, no study of history, none of moral science, none of chemistry, none even of experimental philosophy" (ibid., p. 514). These criticisms were fully justified by the Reports of the Commissions published in 1852. See on the teaching of Theology at Cambridge, Report, pp. 89, 102; Evidence, pp. 88, 168, 190, 216: on the teaching of Latin, Rep., pp. 98, 102; Evid., pp. 165, 176, 289: on the teaching of English, Evid., pp. 124, 136: of modern Languages, Rep., pp. 26, 101; Evid., pp. 165, 216, 300: of Law, Rep., pp. 35, 182; Evid., pp. 123, 190: of Natural Sciences, Evid., p. 115, &c. In 1874 the 'Edinburgh Review' could point out that during twenty years, whilst the examination for the Indian Civil Service had been thrown open, the English universities had practically contributed no

system, what England did possess was the ideal of a *liberal education.* But none of these three forms of intellectual training—neither the scientific in Paris, nor the classical in Germany, still less the liberal in England—touched the great masses of the people. They all did good work in their respective lines; but they left, or would by themselves have left, the country in darkness. The beginnings of general popular education are to be traced independently in Switzerland, in Scotland, and in many of the small States of Germany.[1] The great scientific 28. Ideal of Liberal Education.

candidates to the competition (April 1874, p. 342). "Nothing about university life was more striking" to the Edinburgh Reviewer "than the contrast between the efforts and the high aims of the few, the culture and solid result achieved by them—and the utter uselessness of it to the many" (p. 354). The 'Quarterly Review' of June 1826 notes "a growing taste for the cultivation of physical science as characteristic of the state of the public mind in England" (p. 159), and refers to the "measures which have been carried into effect throughout the country with great harmony of design, although chiefly by the unassisted exertions of private individuals, . . . the recent establishment of numerous literary and philosophical institutions in our metropolis and many of our provinces" (ibid., p. 154).

[1] The great Reformers—Luther, Melanchthon, Zwingli, and Calvin—alike took a great interest in education, which they intended to be universal and popular. But their success, so far as the education of the people was concerned, remained everywhere very partial. A real organisation of primary schools was not attained. They prepared for it by introducing the vernacular languages, the reading of the Bible, the popular hymns. Their main efforts lay in the training of good teachers for church and schools in the reorganisation of what were called the Latin schools. In the course of the sixteenth and seventeenth centuries the smaller Protestant States of Germany—especially Saxony, Würtemberg, Brunswick, the northern cities Hamburg and Lübeck—received under various forms what was called "Eine Kirchen- und Schulordnung." Luther's tract of the year 1524, addressed to the "burgomasters and councillors of all towns of the German land, that they should found and maintain Christian schools," was the beginning of this movement. In Scotland burgh schools, also grammar (or Latin) schools and lecture schools, "in which the children were instructed to read the vernacular language," existed long before the Reformation. But to John Knox is due the scheme for popular education contained in the 'First Book of Discipline.' The parochial schools were started in many instances by voluntary or ecclesiastical assessment through the efforts of the Reformed clergy. The foundation of the subsequent system of parochial schools was laid

schools of France trained the civil and military engineers in that country, and produced text-books for the

in the statute of 1696. It must not be forgotten, however, that the "Order of Jesus" (founded 1540), whose higher educational work has found so much appreciation from men like Sturm—the Protestant educationalist—Lord Bacon, and Descartes (see the quotations in Schmidt's 'Geschichte der Pädagogik,' 4th ed., vol. ii. p. 248), was also active in the direction of popular and primary education. In emulation of the Protestant movement, it had introduced "school regulations" in many Catholic countries, and even founded a special order—the "Patres piarum scholarum" (1600)—for the education of the poorer classes (ibid., p. 253). Whether the statute of 1696 is the earliest official document referring to popular education and providing the means of maintaining an adequate number of schools (one in 1000 of population) to teach the lower classes, I cannot say. It appears that Duke Ernest of Gotha, in the course of the seventeenth century, established a general system of primary education in his territory which was "quite unique, at first an object of ridicule, but then very soon of emulation" (ibid., p. 333). The regulations were certainly most wise and liberal, and attendance was made compulsory. The question of popular education was taken up on a much larger scale by Frederick the Great in the middle of the eighteenth century. The year 1763, which marks the end of the Seven Years' War, is also the year of an edict which forms the basis of the regulation of popular education for the whole monarchy: it establishes village schools with compulsory attendance. It met with much opposition, and its ends were only slowly realised, and only as training-schools, where a sufficient number of teachers were educated, sprang up, and as popular school and story-books were provided. Campe, with his edition of 'Robinson Crusoe,' marks an epoch in this direction. In fact, the cause of universal popular education remained in the hands of private persons, frequently of men of great insight and organising ability—such as A. H. Francke (1663-1727), the indefatigable friend of the poor and of orphans; Basedow (1723-90), the founder of the Philanthropin and populariser of Rousseau's ideas; Von Rochow (1734-1805), the friend of the country-folk and founder of village schools; Von Felbiger (1724-88), the adviser of Maria Theresa and Joseph II., the organiser of the popular educational system in Austria (1770-80): or else it was dependent on the casual favour of enlightened princes and sovereigns. At length, in the middle of the eighteenth century, training-schools for teachers, so-called "seminaries," were founded all over Germany. A beginning had been made by Duke Ernest of Gotha (1601-75), but had been neglected like many other beginnings. But in the second half of the eighteenth century no less than thirty-three seminaries were founded all over Germany, including Austria. For details on this important and interesting subject, see the third volume of Schmidt's 'Geschichte der Pädagogik.' Freytag's 'Bilder aus der deutschen Vergangenheit' also contains many interesting details; but above all I would recommend for the countries of the west and south of Germany the valuable researches of C. T. Perthes contained in his 'Politische Zustände und Personen

higher scientific training of the whole of Europe;[1] but no serious effort was made, during the brilliant days of the First Empire, to secure for the nation the blessing of a popular education. This state of things continued under the Restoration; the real beginnings of an organised primary system are to be found in Guizot's celebrated law of 1833. In Germany the influence of Pestalozzi and Zschokke in the south; of Basedow, Francke, and the school of Kant and Herder, and, later, of Herbart in the north,—stimulated many Governments to establish a system of popular schools for the education of the masses, and a system of seminaries for the training of a popular teaching staff. This movement was chiefly carried on independently of the reform of the universities and higher schools, over which the ideal of *Wissenschaft* exercised a powerful spell. Under the latter were trained the leaders and higher teachers of the nation, as well as the members of the learned professions. The educational influence of this ideal on the more gifted among the student class was the very highest and best; but it hardly

in Deutschland zur Zeit der französischen Herrschaft,' 2 vols., Gotha, 1862 and 1869. As unfortunately this work, with its collection of interesting and not easily accessible facts referring to the inner history of the German people, has no index, I give the following references: Compulsory education in Kur Trier in 1712, vol. i. p. 225; in Kurmainz, 1750, vol. i. p. 19; popular education in Baden, vol. i. p. 411; in Bavaria, vol. i. pp. 436, 467; in Würtemberg, vol. i. p. 537; and the chapter on Joseph II.'s school reform, vol. i. pp. 153-170. The seminary or training-school being thus the centre and beginning of national education in Germany, as it has also, with a different constitution, become the centre of scientific work (see p. 214, note), it is interesting to note that Scotland, so far advanced in educational work, had no real training-school for teachers before Stow started his Normal School in Glasgow (see 'Chambers's Encyclopædia,' art. "Education"), and that the "seminary" for higher scientific work has to this day not yet been introduced into this country.

[1] See above, p. 44, note.

reached the multitude of less gifted minds, who always gave themselves to bread-studies; and it must necessarily fail yet more when not only the future teachers and leaders, but the masses of the nation, flock into the halls of the universities. Imperceptibly a differentiation has taken place in Germany between the educational work which was meant to reach the people at large and the intellectual instruction of a select few. But it is exactly this differentiation of education and higher instruction which the champions of a *liberal education* in England have desired to avoid.[1] In France, very soon after Rousseau's time, dis-

29. Union of education and instruction.

[1] The two developments in Germany start from different centres. The purely educational movement began in Switzerland with Pestalozzi (1746-1827). His forerunner was Martin Planta (1727-1772), his successors were legion, all over Europe, including sovereigns, statesmen, and philosophers. He created an enthusiasm for education, which was to begin at home, not in the school; to depend on the influence of the mother; to be founded on a religious spirit; to direct itself to the development of the body as much as of the mind; to rest primarily on observation and experience, not mainly on memory and learning; and then to absorb the whole mind and the entire man, not exclusively the intellect. It was to begin from below, not from above, with the people, the poor, the unfortunate and deserted; on the part of the teacher it was to be a sacrifice, an end in itself, not a profession. The greatest followers of Pestalozzi were Von Fellenberg (1771-1844), the founder of Hofwyl and other industrial schools for poor and deserted children among the peasant population of Switzerland; Johannes Falk (1760-1826), the founder of a great number of houses for the poor and the fallen, of the "Society of Friends in Need"; J. H. Wichern (1808-1881), the founder of the "Rauhe Haus" near Hamburg; lastly, the celebrated Fröbel (1782-1852, a native of Thüringen), the founder of the Kindergarten. The other—not to say opposite—development was centred in F. A. Wolf, in whose school the ideal of *Wissenschaft* with its enormous influence on universities and high schools was elaborated. In the history of this development, with which our second chapter dealt, the name of Pestalozzi does not occur. The term "popular" was for a time banished as identical with the *βαναυσία* of the ancient Greeks. The two movements find a connecting-link in the extra-academical, the classical literature of Germany, notably of Herder and Goethe, to whom we must add Fichte and Schleiermacher. The present age is working towards a fusion of both interests, of the educational and higher scientific, the bridging over of the gap which had been left; it is trying to remove the estrangement which existed in the middle of the century. We may say that no educational scheme can be permanently satisfactory that does not regard with equal favour, and does not find equal room for, the two ideals of Pestalozzi and Wolf. It is interesting, however, to note that neither in Switzerland nor in Scotland, the two countries in which popular education has been longest at home, do we find a really great development of the higher institutions and centres of learning; the universities in these two countries have always stood somewhat in the relation of higher schools to the rest of the educational establishments; but both countries have produced and reared some of the greatest geniuses of all time—geniuses who have given to German and English literature and science a fame over the whole world and for all ages; they would have sufficed, had they stayed at home, to form academies and universities of the first order.

cussions on educational matters confine themselves to the ends and means of general or higher instruction;[1] in

[1] Compare chapter i. pp. 112, 142, &c. We are indebted to France for three great educational influences which have left indelible traces over the whole domain of European thought. These proceed from the Paris University, the model of higher education; the great school of Port Royal, that model of secondary education; and the 'Émile' of Rousseau, which gave to the educational aspirations of Basedow, of Kant, and of Pestalozzi a definite direction. It has, however, frequently been stated that the valuable side of Rousseau's ideas was developed outside of France. "C'est une chose remarquable," says M. Compayré, "que l'influence du philosophe de Genève se soit surtout exercée à l'étranger, en Allemagne et en Suisse" ('Histoire critique des Doctrines de l'Éducation en France,' 5me ed., 1885, vol. ii. p. 101). "Il y avait, chez Rousseau," says M. Bréal, "un côté généreux et vivifiant: l'amour de l'humanité et particulièrement de l'enfant, la confiance dans ses facultés et le respect de son activité intellectuelle. Cette partie là, qui était le germe de vie déposé dans les œuvres de Rousseau, nous l'avons laissée aux étrangers." In French writers a great deal of discussion is to be found on the difference between education and instruction. Duclos (1704-72) in his celebrated 'Considérations sur les mœurs de ce siècle' (1751), in the second chapter, which treats of Education and Prejudice, says: "On trouve parmi nous beaucoup d'instruction et peu d'éducation. On y forme des savants, des artistes de toute espèce; chaque partie des lettres, des sciences et des arts y est cultivée avec succès, par des méthodes plus ou moins convenables. Mais on ne s'est pas encore avisé de former des hommes, c'est à dire, de les élever respectivement les uns pour les autres, de faire porter sur une base d'éducation générale toutes les instructions particulières," &c. When the successive Governments of the Revolution took up the question of a national education, the formula of Condorcet quite inevitably became more and more the leading principle. Condorcet distinguished "instruction"—*i.e.*, knowledge positive and certain, truths of fact and calculation—from "education"—*i.e.*, "political and religious beliefs." He gives the State the power to extend the former, whilst he denies it the right to direct and dispense the latter (see Hippeau, 'L'Instruction publique en France pendant la Révolution,'

Germany, education and higher instruction present independent developments; in England alone the genius and language of the nation have refused to admit of any curtailment of the original sense of the word. This continued to imply a discipline of the character as well as of the mind, practical as well as intellectual training. So much has been said in this country and abroad regarding the shortcomings of the English universities and higher schools, that I feel it a duty to point to the positive gain which this ideal of a liberal education[1] has

1881, vol. i. p. xvii; also Compayré, *loc. cit.*, vol. ii. p. 280, &c.) Every Government which has attempted to systematise, to centralise education, has been forced also to secularise it, to reduce it to instruction, leaving out what many consider the central problem of education, the training of the character and the discipline of the feelings and the heart. Considering the large organisations which have been developed in England by the unaided efforts of working men, such as the trade-unions and the co-operative societies, and looking at the amount of self-government, self-control, and self-denial which they demand from their members, one might be tempted to say that England is the best educated, though it may be the worst taught and the least informed, of the three nations now under review.

[1] The term "liberal education" has acquired a peculiar significance in the history of English culture and thought. It cannot be translated into French or German with any certainty that the real significance of the term or the subject which it denotes is conveyed. It is interesting to note how each of the three nations has given to special words of the once common Latin language a peculiar pregnancy, denoting a peculiar form of thought or culture which they have especially elaborated. Thus "science" in the modern sense is a product of French thought, *Wissenschaft* a product of German thought. England has reserved to itself the elaboration of a "liberal education." I am at a loss how to translate it into French, unless I am permitted to use simply the word education in its contrast to instruction and *enseignement*, not as this was defined by Condorcet, but as it is understood in the writings of modern French educationalists, such as Gréard, Bréal, Compayré, and others. To convey the meaning of "liberal education" to a German, I would revert to the Greek phrase, the *ἐλευθέριος παιδεία* of the post-classical age. The fact is that down to the middle of the century the Germans in discussions on the work of universities and high schools always talk of *Wissenschaft*, English writers always talk of "liberal education." To a German scholar's heart *Wissenschaft* is dear beyond anything; to an English university man it is "liberal education." The former will sacrifice everything to *Wissenschaft;* the latter will not part with "liberal

been. For it is the principal object of this work to attempt to portray the actual progress of thought, the valuable contributions of each of the three nations to the

education." In Germany, the real home of the educationalist or *Erzieher* has not been the university; the home of the man of science has not been and is not the university in England. The German educationalist can point to a special creation of his own, the *Volksschule.* The English man of science has no organisation to point to except it be the select society of a dozen great names of world-wide fame, corresponding to the solitary and unconnected heights of Homer, Sophocles, Dante, Shakespeare, and Goethe in literature. To descend, however, from generalities to the real thing, I give here some extracts referring to English university life, chosen from among hundreds, all variations on the same theme. Dr Thomas Young, who knew both German and English universities, having studied at Göttingen and taken his degree at Cambridge, was not indebted to any university for his position or his knowledge; yet he significantly defends the English universities against the criticism of the Edinburgh Reviewer: "We do not intend to imply a censure of the system adopted by our universities; . . . for it must be remembered that the *advancement* of learning is by no means the principal object of an academical institution: the *diffusion* of a respectable share of instruction in literature and in the sciences among those classes which hold the highest situations and have the most extensive influence in the State is an object of more importance to the public than the discovery of new truths. . . . We think that we have observed numerous instances, both in public life and in the pursuit of natural knowledge, in which great scholars and great mathematicians have reasoned less soundly, although more ingeniously, than others, who, being somewhat more completely in the possession of common-sense, . . . were still far inferior to them in the refinements of learning or of science" ('Quarterly Review,' May 1810, reprinted in Miscellaneous Works, vol. i. p. 235, &c.) I shall now give a quotation from an entirely different source, from one who in his department was equally well acquainted with German and English thought and life. In 1830 E. B. Pusey attempted to give his friend, Prof. Tholuck of Halle, a sketch of what had been "recently done in English theology." He begins by referring to the "practical character of the nation" and "the different condition of the universities," and then continues as follows: "Few, if any, of our writings have originated in an abstract love of investigation: our greatest and some immortal works have arisen in some exigencies of the times. . . . A German writes because he has something to say; an Englishman only because it is, or he thinks it is, needed" ('Life of Pusey,' vol. i. p. 238). The man who did most for the widening of the circle of university studies in England during the first half of the century was William Whewell (1794-1866), whose influence at Cambridge extended over more than a generation. In the beginning he assisted the movement begun by Babbage, Herschel, and Peacock, and published several text-books on mechanics and dynamics, in which the influence of Continental, especially

general stock of ideal possessions, not merely to criticise the shortcomings and failures of separate schools of thought, or separate sources of mental development. Only in the aggregate of these different ideals is to be found the inventory of the intellectual possessions, the outcome of the higher work of the century.

30. Educational organisations in England.

When the modern scientific methods and their impelling force, the mathematical spirit, made their way from France to Germany during the first quarter of the century, they there met with a powerful intellectual organisation, the German university system, in which classical and philosophical studies had elaborated the ideal of *Wissenschaft* —of science in the larger sense of the word. Gradually, and not without opposition, the exact or mathematical spirit was received into this system, and has since become an integral portion of it. In England the older traditions which clung to the two great universities, and the higher

French models, can be clearly traced. Between 1830 and 1850 his influence exerted itself in two directions, firstly by the publication of his 'History of the Inductive Sciences' (3 vols. 1837; a second edition appeared in 1847, a third in 1857), and, secondly, by a series of papers and pamphlets referring to university education. As the ideal and definition of this Whewell adopts the term "liberal education." The first of these papers appeared in the 'British Critic' (No. 17, 1831, "Science of the English Universities"). Then followed in 1836 "Thoughts on the study of Mathematics"; "Additional Thoughts," 1836; "On the Principles of English University Education," 1837; "Of a Liberal Education in General" (Part 1, 1845; Part 2, 1850; Part 3, 1852). The second part of the little work on Liberal Education gives a history of the various changes previous to 1850 through which the University of Cambridge tried to meet the growing demands of the times for a wider and more liberal programme of higher scientific work. In these various writings the work of education and "original research" (a term introduced by Whewell—see Todhunter, 'Life of Dr Whewell,' vol. i. p. 50), the nature of "permanent" and "progressive" studies at the university, of "university" and "college" education, of "tutorial" and "professorial" teaching, are fully discussed. In the course of thirty years the university of Cambridge added to the examinations for mathematical honours the "Classical" Tripos (1822), the "Moral Sciences" Tripos and the "Natural Sciences" Tripos (1848); also a "Board of Mathematical Studies" (1848). Dr Whewell's great influence declined when in 1850 Royal Commissions were appointed to "inquire into the state, discipline, studies, and revenues of the universities of Oxford and Cambridge." He "regarded the Commission as an unwarranted and undesirable intrusion into the affairs of the university." The results of this inquiry belong to the second half of the century. Although this movement, which was brought about by many influences, has somewhat changed the issues, the central idea which in England tries to assimilate the higher work and thought of the nation is that of education. The term liberal education, which for twenty years, from 1830 to 1850, formed the banner of university reform, has since somewhat yielded to "scientific," and more recently to "technical," education; the influence of the universities has gone out in the work of university extension in the provincial towns; still the whole movement can be defined as an educational movement. Whereas in Germany about a generation earlier the term *Wissenschaft* gained the upper hand and governed the intellectual life of the nation, purely educational movements being separated from it, in England the purely scientific interest has never gained the upper hand, and can still complain of having nowhere a full and complete representation. Around the writings of Whewell as a centre may be grouped those of A. Sedgwick ('A Discourse on the Studies of the University of Cambridge,' 1833, 5th ed., 1850); Sir Wm. Hamilton (articles in the 'Edinburgh Review,' reprinted in 'Discussions on Philosophy, &c.,' 1853); Sir John Herschel ('A Preliminary Discourse on the Study of Natural Philosophy,' 1831); the criticisms of Lyell ('Travels in North America,' 1845), and of the 'Edinburgh,' 'British Quarterly,' and 'Westminster' Reviews ('Edin. Rev.,' Ap. 1849, Jan. 1874, 'Brit. Quart.,' Nov. 1850, 'West. Rev.,' Jan. 1855). Whoever desires to gain an insight into the different, frequently diametrically opposite, considerations which moulded and governed the reconstruction of the German university system on the one side, and on the other side widened in England the older ideas of university education, should compare the documents relating to the foundation of the University at Berlin in the beginning of this century (collected by Rudolf Köpke, 'Die Gründung der Königlichen Friedrich-Wilhelms-Universität zu Berlin,' Berlin, 1860) with the writings referred to in this note, and centering in Whewell's pamphlets and essays. The personification of the German scheme was Wilhelm von Humboldt, of whom Böckh said in his 'Logos epitaphios': "He was a veritable statesman, penetrated and led by ideas—a statesman of a Periclean greatness of spirit. Philosophy and poetry, eloquence, historical, philological, linguistic erudition, were fused in him into undisturbed harmony and wonderful symmetry." The reforming and revolutionary ideas of Fichte, the classical ideals of Wolf, the historical interests of J. Müller the historian, the literary interests of Schlegel, the philosophical interests of Schleiermacher, were combined by Humboldt into a realisable scheme. Stein said of him in 1810: "Prussia has intrusted the management of her educational and scientific institutions to a man possessed of a remarkable intellect and of great firmness of character, and who utilises these qualities in his sphere of action with glorious loyalty" (ibid., pp. 61, 62).

practical interests of a select class which upheld those traditions, prevented any of the Continental ideals, be it the philological of F. A. Wolf, or the philosophical of Fichte, or the scientific of Laplace and Cuvier, from establishing themselves in the older seats of learning. And they were, after all, the only organisations for higher culture which possessed a historical character and continuity. Around these centres, partly in a friendly, more frequently in a hostile spirit, other institutions, other centres of culture and learning, had grown up. Let us rapidly survey these more recent institutions. It is hardly necessary again to mention the Royal Society, which was an early offspring of the older universities, a kind of overflow of the scientific interests from them into

31. The Royal Institution. the capital. More recent was the Royal Institution, the creation of that extraordinary man, Benjamin Thompson, Count Rumford. Like the Royal Society, it was dependent upon private subscriptions and on the popular interest created by its lectures. These were very promiscuous, exhibiting no plan or unity. In the early years Dr Young and Davy lectured there, as well as Coleridge and Sydney Smith. Later it became the home of Faraday, and through him, and many other illustrious lecturers, has done much to spread a taste for natural, especially experimental, science, in the higher and cultivated classes. It has been a means of diffusing the scientific taste, more perhaps than the exact scientific spirit, in the stricter sense of the word. Whilst its lectures may have kindled in many a young listener the love of scientific work, the Institution did not fulfil the early intention of its founder, nor did its laboratory play

the part of some of the great laboratories of Paris or of Germany, in turning out a large number of well-trained experimentalists. Davy may be said to have educated Faraday, though he was suspected of having become jealous of him, and Faraday declared he received only one valuable suggestion from any member of his audience during the whole course of his lecturing. It is the strongly marked individuality of all these great men, expressed in their persons, their lives, and their works, rather than the character of the institution itself, which has given celebrity and historical importance to the Royal Institution. John Dalton's [1] position in the Literary and Philosophical Society of Manchester was similar to that of Davy and Faraday in the Royal Institution; and as Faraday can in some sense be called a pupil of Davy, so can Prescott Joule [2] be termed a pupil of Dalton, whom

32. Manchester Literary and Philosophical Society.

[1] See note, p. 245.

[2] James Prescott Joule (1818-89), a native of Salford, "received from Dalton his first inducement to undertake the work of an original scientific investigator." He was one of the first who tried to measure electrical action in terms of the units of well-known mechanical or chemical changes. His publications began in 1840. Weber's 'Electrodynamische Maasbestimmungen,' that great monument of exact measurement, was published in 1846. Mayer's first publication, containing a calculation of the mechanical equivalent of heat, bears the date 1842. But the great publication of Gauss, in which he measures magnetic action in ordinary mechanical (or absolute) units, dates from 1832: 'Intensitas vis magneticæ terrestris ad mensuram absolutam revocata' (Comm. Societ., Götting., 1832, &c.) Joule in 1843 published the first of his accurate determinations of what is termed in physical science "J" or "Joule's equivalent of heat." He read successively papers on this subject before the meetings of the British Association, first at Cork (1843), giving the constant "J" as 838, then as 770, then as 890 in 1845 (Brit. Assoc. at Cambridge), lastly at Oxford (1847) as 781·5. From this meeting dates the acquaintance and scientific co-operation of Joule and Thomson (Lord Kelvin) and the gradual recognition of the importance of the subject by other men of science (see Thomson's address on Joule, 1893, in 'Popular Lectures and Addresses,' vol. ii. p. 558 *sqq.*) Helmholtz's memoir, "Ueber die Erhaltung der Kraft," which was theoretical—as Joule's were experimental—dates also from 1847.

he succeeded as president of the Society. These names are identified with some of the greatest work in experimental science. Some of them may be said to be identified with quite original theoretical ideas which have governed the development of great departments of research ever since. Dalton's atomic theory in chemistry, however, received a tardy recognition in England, and was firmly established only by foreign research, while Faraday's "lines of force" remained a mystery to electricians,[1] till William Thomson and Clerk Maxwell made them the groundwork of our most recent conceptions. It is well to note that neither Young, nor Davy, nor Faraday, nor Dalton, nor Joule belonged to the circle of Cambridge men, and that probably none of them received any inspiration from that official school of English mathematics.[2] In the early years of the century that

[1] See Helmholtz on Faraday's ideas in 'Vorträge und Reden,' vol. ii. p. 277. "Since the mathematical interpretation of Faraday's theorems has been given by Clerk Maxwell in methodically elaborated scientific formulæ, we see, indeed, how much definiteness of conception and accurate thought were contained in Faraday's words, which seemed to his contemporaries so indefinite and obscure. And it is indeed remarkable in the highest degree to observe how, by a kind of intuition, without using a single formula, he found out a number of comprehensive theorems, which can only be strictly proved by the highest powers of mathematical analysis. I would not depreciate Faraday's contemporaries because they did not recognise this; I know how often I found myself despairingly staring at his descriptions of lines of force, their number and tension, or looking for the meaning of sentences in which the galvanic current is defined as an axis of force, and similar things. A single remarkable discovery can indeed be brought about by a happy chance, . . . but it would be against all rules of probability that a numerous series of the most important discoveries, such as Faraday produced, could have had their origin in conceptions which did not really contain a correct, though perhaps deeply hidden, ground of truth."

[2] Young resided at Cambridge to take his medical degree on his return from Göttingen; but though his biographer has inserted a chapter on Cambridge in the 'Life of Young,' and though Young's first great discovery, that of the interferences of waves of sound and light, fell within that period, there is no evidence that his scientific studies were promoted by Cambridge influences; in fact he makes a disparaging remark regarding British as compared with Continental mathematics. See Peacock's 'Life of Dr Young,' p. 127.

centre had, indeed, to receive aid from a still more secluded and unacademic quarter. Undergraduates of Cambridge used to migrate from the seat of teaching which has been immortalised by Newton to the remote Yorkshire village of Sedbergh, where John Dawson,[1] one of the few British analysts who held their own against the great foreign authorities, taught the higher mathematics for five shillings a-week. 33. John Dawson of Sedbergh.

During the latter part of the eighteenth century a formidable rival to the learning of Oxford and Cambridge had sprung up in the Scotch universities. These were teaching centres, more after the manner of the foreign universities. They had been started on the model of the University of Paris or of the older Italian universities; some had their origin in the educational movement which, especially in those countries where the doctrines of Calvin prevailed, accompanied the Reformation.[2] All through the 34. The Scotch Universities.

[1] John Dawson (1734-1820), the son of a poor "statesman" of Garsdale, tended his father's sheep till he was twenty. He studied mathematics with innate love and ability, inventing a system of conic sections out of his own brain. By teaching he gained a little money. In 1756 he instructed three young men—of whom Adam Sedgwick's father was one—before they went up for their Cambridge studies. He then became assistant to a surgeon at Lancaster. Having saved £100 he walked to Edinburgh and studied medicine there. His funds spent, he returned to Sedbergh, where he practised as a surgeon. When he had saved a larger sum he proceeded with this to London. After taking his degree in 1767, he settled in his native county to practise his profession and teach the higher mathematics to Cambridge undergraduates. They flocked to him in the summer, and between 1781 and 1794 he numbered eight senior wranglers among his pupils. In 1797 and subsequent years he counted four more. In 1812 he ceased teaching. He wrote papers on the "precession" and the lunar theory, and followed the development of higher mathematics on the Continent. See 'Life and letters of Adam Sedgwick,' by J. W. Clark and T. M'K. Hughes, 1890, vol. i. p. 61, &c.

[2] Details referring to the foundation of the Scotch universities are given by Sir A. Grant in the first volume of his 'Story of the Univer-

seventeenth and eighteenth centuries they stood in intimate relations with such Continental centres of study as Paris, Geneva, and the Dutch universities. Adam Smith and David Hume were in direct and very intimate intercourse with French thought, the former having obtained in France a knowledge of the novel views of the great political economists of the pre-revolutionary period. Edinburgh became in the first half of the last century, under the influence of John Monro and his son Alexander (1697-1767), who was a pupil of Boerhaave, a medical school of great importance, rivalling London in its foreign rep-

sity of Edinburgh,' 2 vols., 1884. Three of them—St Andrews, Glasgow, and Aberdeen—were founded in the century preceding the Reformation; St Andrews about 1411 by Bishop Wardlaw, because Scotch students had been unpopular and "molested" at Oxford. The University of Glasgow was founded in 1450, reference being made to the University of Bologna in the Bull of Pope Nicholas V.; but it has also been observed that "the customs and technical phraseology showed an imitation of the institutes of Louvain, then and for all the following century the model university of Northern Europe, of which a Scotchman, John Lichton, had been Rector" (p. 21). Aberdeen was started by Bishop Elphinstone, who had studied in Glasgow and Paris, and been professor, both there and at Orleans, of canon and civil law. In the preamble to the Bull of Pope Alexander VI. the Universities of Paris and Bologna are referred to (p. 29). But the universities seem not to have flourished previous to the Reformation, when they were "purged" and a new spirit and order infused into them. St Andrews was to have four faculties, named as in foreign universities—Philosophy, Medicine, Law, and Divinity (p. 63). Glasgow and Aberdeen were to have two faculties, of which the first was to be Philosophy (or Arts), the second to comprise Law and Divinity. The 'Book of Discipline' contained a very complete scheme of higher graded education; but this was only gradually and partially realised; secondary schools being wanting, the "colleges" had to descend to elementary teaching (p. 67). A jealousy also existed on the part of those in power regarding the older universities, these being—as the King of France declared when refusing to grant to the Academy of Geneva the rights of a university—hotbeds of heresy (p. 125). Accordingly the latest academic creation in Scotland was the foundation by the "Town Council and ministers of the city" of the College of Edinburgh (pp. 99, 121, 127) between the years 1561 and 1578, King James's charter dating from 14th April 1582. "But it did not, like the older universities, commence with a blaze of success and then collapse. It started from a humble beginning and steadily expanded into greater things" (p. 158).

utation.[1] Edinburgh had also one of the earliest chairs of chemistry. It grew into an independent centre of original scientific work when in 1783 the Royal Society of Edinburgh was incorporated. Ever since the foundation of the Scotch universities, mathematics had been studied independently in Scotland, where John Napier of Merchiston had at the end of the sixteenth century invented logarithms. "Whether we consider the great originality of the idea, the difficulty of carrying it into effect in the state in which algebraical analysis then was, or the immense practical and theoretical value of the invention, we shall have little difficulty in claiming for Napier the honour of a discovery unsurpassed in brilliancy in the whole history of mathematics."[2] From that time the

35. The Royal Society of Edinburgh.

[1] "In 1738 the foundation-stone of that building which was till recently the Royal Infirmary of Edinburgh was laid, and a great public enthusiasm on the subject was manifested. Drummond, the greatest Ædile that has ever governed the city of Edinburgh, and Monro, were appointed the Building Committee, and they paid the workmen with their own hands. All classes contributed: landowners gave stone; merchants gave timber; farmers lent their carts for carriage of materials; even the masons and other labourers gave one day's work out of the month gratis, as it was a building for the benefit of the poor" (Sir A. Grant, *loc. cit.*, vol. i. p. 306).

[2] Quoted by Sir A. Grant (*loc. cit.*, vol. ii. p. 293) from Chrystal's unpublished Inaugural Address, 'John Napier, Baron of Merchiston' (1550-1617). The 'Mirifici Logarithmorum Canonis Descriptio' appeared in 1614. The 'Logarithmorum Chilias prima' of Henry Briggs (1556-1630), professor at Oxford, contains the first table of common or decimal logarithms. Kepler (1571-1630) received the invention with great enthusiasm as of immense importance to astronomy. "The more one considers the condition of science at the time, and the state of the country in which the discovery took place, the more wonderful does the invention of logarithms appear. . . . It is one of the surprises in the history of science that logarithms were invented as an arithmetical improvement years before their connection with exponents was known. It is to be noticed also that the invention was not the result of any happy accident. Everything tends to show that it was the result of many years of labour and thought undertaken with this special object; Napier succeeded in devising, by the help of arithmetic and geometry alone, the one great simplification of which they were susceptible — a simplification to

science was cultivated at the different Scotch universities, which supplied Oxford with a Professor of Astronomy (preferred to Halley), in the person of David Gregory. "David Gregory not only introduced the 'Principia' to Edinburgh students, but he also brought them to the notice of Englishmen."[1] The Philosophical (afterwards called the Royal) Society of Edinburgh was much indebted to Colin Maclaurin,[2] who almost alone with Landen and Ivory maintained the reputation of British mathematicians during seventy years, whilst the Continental school was revolutionising that science. A successor to Maclaurin in the mathematical chair at Edinburgh, John Playfair,[3] introduced the Continental methods into the studies of the Scotch universities about the end of the last century. He was one of the early contributors to

36. The 'Edinburgh Review.'

the 'Edinburgh Review,' which in politics, literature, and science inaugurated a new kind of criticism, and led a powerful attack upon all those traditional forms of government, taste, and learning which prevented the free expansion of ideas and the progress of science and practical interests. Though not always judiciously used, the

which the following two hundred and eighty years have added nothing" (Glaisher in 'Ency. Brit.,' 9th ed., article "Napier").

[1] David Gregory (1661-1708) has "the honour of having been the first to give public lectures on the Newtonian philosophy. This he did in Edinburgh five-and-thirty years before these doctrines were accepted as part of the public instruction in the university of their inventor" (Sir A. Grant and Chrystal, *loc. cit.*, vol. ii. p. 296). Cambridge writers, headed by Whewell, are loath to admit any reluctance on the part of their university in accepting the Newtonian philosophy, in spite of Whiston's testimony to the contrary. See on this Whewell's 'History of the Inductive Sciences,' 3rd ed., vol. ii. p. 149, &c.

[2] Colin Maclaurin (1698-1746) published, 1742, a 'Treatise on Fluxions,' 2 vols. 4to. In 1740 he shared with Daniel Bernoulli and Euler the prize of the French Academy for his 'Essay on the Tides.'

[3] John Playfair (1748-1819) was Professor of Mathematics and then (from 1805) of Natural Philosophy.

influence of that review must have been very powerful in rousing the older English universities out of a state of stagnation, and especially in stimulating younger minds in the direction of the long-delayed reform of studies. An important step in this direction was taken by three undergraduates of Cambridge—Herschel, Babbage, and Peacock—who in 1812 formed the Analytical Society, with the distinct object of introducing the more modern and powerful analytical methods developed mainly by Euler and Lagrange, and deposited in their numerous Memoirs in the publications of the foreign academies.[1] In harmony with them worked Whewell, Airy, and Sedgwick, who did much to enlarge the programme of mathematical and scientific studies, though they very staunchly upheld that the real object of university education could not be identified with any special method or school of thought, but was expressed in the specific ideal peculiar to England, that of a liberal education.[2]

37. The Analytical Society of Cambridge.

38. University life in Scotland.

The universities of Scotland, unlike those of England, instead of nursing an exclusive spirit, and encouraging only scanty intercourse between teachers and students of different centres, lived in constant exchange of professors and ideas—much in the same way as has always been the custom on a larger scale among German and other Continental universities. Though this is destructive of that individual character of the university or the college which

[1] See note 1 to p. 233; also for many details Rouse Ball's 'History of the Study of Mathematics at Cambridge,' 1889, p. 120, &c.

[2] On Whewell and his writings on university education see note to p. 261. Sir George Biddell Airy (1801-1891) published in 1826 'Mathematical Tracts' (2nd ed., 1831) on the lunar and planetary theories, &c., for the use of students in the university.

is so highly prized by many English fellows, it is certainly more conducive to the progress of studies and of research, and it is the cause why in the early history of recent science the universities of Scotland have played so much more important a part than those of England. Whilst in England modern science was cultivated outside the pale of the universities by Priestley, Davy, Wollaston, Young, Dalton, Faraday, and Joule, to whom we may even add Green and Boole, all eminent Scotch men of science, such as Gregory, Simson, Maclaurin, Playfair, Black, Thomson, Leslie, Brewster, and Forbes, were university professors, many of whom did not confine their labours to one centre, but spread the light of their ideas and researches all over the country.[1] Whilst England has been great in single names, Scotland has certainly in proportion done more

[1] Napier of Merchiston remained outside the pale of the universities. At that time the College of Edinburgh had no mathematical professor; but Glasgow had, and so had Aberdeen. James Gregory was educated at Aberdeen, was then professor at St Andrews, and subsequently at Edinburgh. Colin Maclaurin was educated at Glasgow, then professor at Aberdeen and at Edinburgh. Playfair was educated at St Andrews, and lectured there before coming to Edinburgh. Leslie was trained at St Andrews, and was then professor first of mathematics and afterwards of natural philosophy at Edinburgh. Black was educated at Glasgow and Edinburgh, and was professor at both universities. Brewster studied at Edinburgh, and was subsequently principal of St Andrews and then of Edinburgh. Forbes, as student and professor, belongs exclusively to Edinburgh, and so did in earlier times Robert Simson, the great mathematical professor. Adam Smith belongs exclusively to Glasgow, though he had lectured in Edinburgh before he was appointed professor at Glasgow. But the contrast between England and Scotland becomes still more prominent if we look at the medical sciences and note the great array of celebrated professors at Edinburgh, Cullen, Brown, Gregory, Alison, Hamilton, Syme, Simpson, Christison, and Charles Bell, whereas the equally great names of John and William Hunter, of Jenner, of Astley Cooper and Bright, have no connection with the English universities; Sydenham was only slightly connected with Oxford and Cambridge, and even Harvey never occupied a prominent position at Oxford. Through situation or constitution the English universities were unable to open a field of activity for these celebrated men.

to diffuse modern scientific knowledge. The great publishing firms of Edinburgh have also for more than a century done much through Cyclopædias, Reviews, and Magazines to spread general information of all kinds;[1] whilst Hume, Adam Smith, and the subsequent Scotch school of metaphysicians have exerted their influence during the whole of this century, not only in Great Britain, but over the whole of Europe.[2] In the more circumscribed domain of scientific thought a powerful influence has again been exerted from Scotland as a centre, and through the larger instrumentality of the University of Cambridge, on the study of mathematical and experimental physics, and what we may term the spirit and method of these sciences. This influence be-

[1] The most popular Cyclopædia, that of Chambers, had its origin in Edinburgh in 1860. It was founded on the tenth edition of Brockhaus's 'Conversations-Lexicon.' The more important 'Encyclopædia Britannica' was published there also in 1771, 3 vols.; 2nd ed., 1777. The 'Edinburgh Review' was established in 1802 by Jeffrey, Scott, Horner, Brougham, and Sydney Smith; it was the first successful "Quarterly," carried on independently of the booksellers, after several unsuccessful attempts had been made in a similar direction by Adam Smith and Hugh Blair in 1755, and after Gilbert Stuart and William Smellie had issued from 1773 to 1775 the 'Edinburgh Magazine and Review.' No such periodical ever attained to the circulation of the 'Edinburgh Review,' of which at one time 20,000 copies were sold. The first high-class monthly Magazine was also printed in Edinburgh by Blackwood in 1817, with Scott, Lockhart, Hogg, Maginn, Syme, and John Wilson as contributors. 'Tait's Edinburgh Magazine' was the first shilling magazine. The brothers William and Robert Chambers, in 1832, started the Journal named after them. They also brought out many popular works of sterling merit, mostly written by Robert Chambers, than whom none did more to introduce a knowledge of nature into popular reading, and to give a healthy tone and moral influence to the cheap literature which has become such an important factor in modern culture.

[2] Whilst Locke exercised the greatest influence on French philosophy, Kant starts more directly from Hume. The literature of the Restoration in France again attaches itself to the Scotch metaphysicians, notably Reid. It is interesting that both Kant and the greatest representative of the French "Ideology," De Tracy, were of Scotch descent.

longs to the second half of the century, and is centred in the two names of William Thomson (Lord Kelvin) and James Clerk Maxwell, who may be said to have jointly revolutionised natural philosophy. It began with the appearance of George Stokes's and William Thomson's important contributions to mathematical physics, and with the publication of that suggestive and stimulating—but unfortunately unfinished—work by Thomson and Tait on Natural Philosophy. It was represented to the fullest extent in Clerk Maxwell's activity in the Cavendish Laboratory at Cambridge. But the consideration of this subject belongs to a later chapter of the present work, and is only mentioned here in connection with the intellectual intercourse and exchange which has existed all through this century between the invigorating spirit of the north and the more conservative spirit of the southern

39. The Dublin Mathematical School.

portion of the island. Besides Scotland another centre —the Dublin School—has gained European renown through a series of mathematical labours of the highest importance, some of them of an originality hardly yet sufficiently recognised. This school is represented by the names of Rowan Hamilton,[1] MacCullagh, Sal-

[1] Of Rowan Hamilton's dynamical "principle of varying action" I have spoken in a note to p. 231. William Rowan Hamilton (1805-65) cannot with the same certainty as Kant and De Tracy be claimed as of Scotch descent. Indeed he seems to belong distinctly to Ireland. See Tait's article in the 'North British Review,' September 1866, and Perceval Graves's reply in 'Life of W. R. Hamilton' (3 vols., 1882-89, vol. i. p. 5). He was one of the few quite original mathematicians who, like Gauss, led the way into new channels of thought and succeeded in breaking through the traditional forms of this science, which more than any other is hampered in its development by transmitted customs and habits of representation. Thus, after ten years of research and thought in connection with the representation of extended algebraical forms by means of the different directions in space, he succeeded in establishing the fundamental principle of his theory of

mon;[1] nor should we forget the suggestive writings of George Boole.[2] The influence of these men originated outside of Cambridge, and a history of mathematics at that university does not contain their names,[3] though the ideas of which they have been the bearers have largely entered into the text-books and the teaching of the Cambridge school.

So far I have mainly dealt with one side only on which the progress of science depends, namely, the methodical use of experiment, measurement, and calculation: this

quaternions — complex quantities which are compounded of a purely algebraical or quantitative element and three distinct elements corresponding to the three directions or dimensions of space. He was the first to work out this calculus, and the labour occupied twenty years of his life. In Hamilton's calculus of quaternions, distance (or length) and direction are introduced as they naturally present themselves when we deal with geometrical or physical problems, instead of all quantities being reduced to lengths, as was the case in the Cartesian geometry. Hamilton thus broke through the conventionalism of the latter and showed how the consideration of directions in space forces us to extend the original operations of arithmetic. It is interesting to note how simultaneously Grassmann (see p. 243, note 1) in his 'Ausdehnungslehre' (1844) and Von Staudt in his 'Geometrie der Lage' (1847), quite independently worked at similar extensions of our arithmetical and geometrical conceptions, and how subsequently quaternions, in which Hamilton had seen a powerful method for solving geometrical and physical problems, present themselves as a special form of the extended algebra and geometry elaborated from these different beginnings. Whilst the practical usefulness of the calculus has been demonstrated by some extensive applications, as, for example, to spherical trigonometry, the ideas contained in it—frequently without Hamilton's notation—are gradually finding their way into text-books, and the strangeness which for half a century prevented the labours of Hamilton, Grassmann, and Von Staudt from being generally appreciated, is disappearing. A popular exposition of the relation of quaternions to general arithmetic is given in O. Stolz, 'Grössen und Zahlen,' Leipzig, Teubner, 1891.

[1] The excellent treatises of Salmon on 'Higher Algebra,' 'Higher Plane Curves,' 'Geometry of Three Dimensions,' and 'Conic Sections' have in their German translations by Fiedler done a great work in systematising and popularising modern conceptions in algebra and geometry. See Gino Loria's treatise on the "Principle Theories of Geometry" in the German translation by Schütte, Leipzig, 1888, p. 25, &c.

[2] See p. 247, note 2.

[3] See Rouse Ball, 'A History of the Study of Mathematics at Cambridge,' 1889.

side had been very largely developed by the great French naturalists and mathematicians in the beginning of our period. The change in the higher branches of science which took place during the first half of the century is greatly owing to them, and to the later German school, which was much influenced by them. If we compare the contributions of British science in these branches, they are indeed inferior in bulk, and still more so in methodical arrangement; but among them is a small number of works of the first order which are embodiments of scientific ideas of the very highest importance. Introduced into the great edifice of scientific research which was being planned and erected on the Continent, they mark the very corner-stones of the building, standing out in bold and conspicuous prominence. But it is a fact that no Academy existed in this country which was zealous in collecting and arranging all the best labours of scattered philosophers, no university which was anxious to attract and train promising intellects, no comprehensive text-books and hand-books, ensuring right guidance, correctness of knowledge, and completeness of study, no historical and philosophical traditions guaranteeing that novel contributions should make their appearance under favourable conditions, or supplying the most appropriate *mise en scène* for new ideas.

40. Importance of British contributions to science.

41. Diffusion of scientific knowledge on the Continent.

It is the French Institute, in the earlier years of the century, and the German university system, with its many local ramifications and literary organs, during the whole of the century, which have done the great work of systematising and diffusing scientific knowledge, and of introducing the exact spirit of research. There is

something casual and accidental about the great ideas which British men of science contributed during the first half of the century. Each of them chooses an isolated position, a special form of delivery, frequently a language and style of his own. They attach little or no importance to the labours of others, with which they are frequently unacquainted.[1] Important papers are lost or buried, as in the case of Cavendish and Green. Novel ideas are communicated in unintelligible language and symbols, and accordingly neglected. This was the case with Dr Young's writings, and to a certain extent with Faraday's. The greatest discoveries were unduly postponed through the absence of assistance, as seems to have been the case with Adams's discovery of Neptune,[2] perhaps with Stokes's anticipation of spectrum analysis.[3]

42. Isolation of English men of science.

[1] This is correct of most of the great men referred to in the course of this chapter. Among them, however, Rowan Hamilton forms an exception. Though working on quite original lines, he took a great interest in the labours and suggestions contained in the writings of his forerunners and contemporaries, as the historical notices in the preface to his 'Lectures on Quaternions' (1853) prove; likewise his correspondence with De Morgan (see 'Life of Sir W. R. H.,' vol. iii.)

[2] The story of the discovery of Neptune has been frequently told. The first publication of the elements of the suspected planet, which enabled a search to be made, came from Leverrier to the Paris Academy of Sciences on the 1st July and the 31st August 1846. In consequence of this publication, Galle at Berlin, requested by Leverrier to search in the neighbourhood of δ Capricorni, and comparing his observations made on the same night on which he received the request, 23rd September 1846, with Bremiker's map, actually found the planet. Subsequently it became known that Adams of Cambridge had already communicated his elements in September and October 1845 to Challis and Airy, and that the former had actually seen the planet on the 4th and 12th of August 1846, but—for want of equally detailed maps—had not compared the observation and established the discovery. See Whewell's 'History of the Inductive Sciences,' third ed., 1857, vol. ii. p. 460, &c.; also Wolf, 'Geschichte der Astronomie,' p. 537, &c.

[3] It appears from a communication of Sir William Thomson (Lord Kelvin) to Kirchhoff immediately after the latter had published in 1859 his explanation of the identity of the dark lines in the solar spectrum with the bright lines in the spectra of coloured flames, that Stokes, soon after the publication

What might not these great minds have accomplished had they attached the same importance to style and form as most of the great French men of science, or had they been called upon to teach a number of eager pupils, anxious, not to take honours and degrees, but to understand and further elaborate the suggestions of their masters, as has been the custom and tradition in Germany? The history of English science during the first half of the century consists of a series of biographies, or of monographs on single ideas and points of view. We are struck by the individual greatness of the minds which produced them, their originality or the suddenness of their appearance. An *éloge* by the permanent secretary of the Academy has usually been considered sufficient to satisfy the historian of science in France; the life of every great philosopher in Germany is identical with the history of a phase of thought or with a school of research; in England alone the person of the thinker has nearly always claimed the

by Miller in 1845 and by Foucault in 1849 of observations relating to this subject, had suggested in the course of conversation that there is a correspondence between emission and absorption of the same kind of light by the vibrating molecules of the same body, according as it is used as a source or a screen for light. Had this idea of Stokes's, which suggested the presence of sodium in the atmosphere of the sun, been followed out at the time, the discovery of spectrum analysis would have taken place ten years earlier. Actually, the various publications, beginning with Fraunhofer's description of the dark lines in the solar spectrum in 1814 and proceeding through the observations of Herschel, Talbot, Drummond, Miller, Angström, Plücker, Swan, and Balfour Stewart on the absorption and radiation of heat, found their consummation when Bunsen and Kirchhoff settled the main point in question—*viz.*, "that the bright lines of an incandescent gaseous body depend on the chemical constituents of the same." Then at length spectrum analysis became possible. See on this matter Kirchhoff's own historical *résumé* of the year 1862, reprinted in 'Gesammelte Abhandlungen' (Leipzig, 1882), p. 625, &c.; also Sir William Thomson's 'Baltimore Lectures,' shorthand notes, 1884, p. 100, and Stokes's translation of Kirchhoff's first paper in 1860 ('Philos. Magazine,' March 1860).

greater share of popular attention.[1] His mental labours have preserved an individual character, shutting them out during his life from common contact, and limiting their fertilising power, like that of an oasis in the desert, to a narrow circle of casual visitors. Minds like Newton and Faraday, full of new life, but modestly content with deepening and strengthening their secluded vigour, refrained from boastful publicity or ostentatious parade, working for all ages rather than for a special school or a passing generation. It is the individualism of the English character, the self-reliant strength of natural genius, which comes out most strongly in its great examples of scientific work. In characters of smaller breadth, in intellects of lesser power, these tendencies show themselves in ways which we cannot always admire or commend: in the emulation for place and position, in the competing for

43. Individualism of the English character.

[1] This explains the remarkable richness of English literature in biographies, containing copious collections of correspondence, and the almost total absence of such literature in France, which, on the other side, is rich in memoirs, written by statesmen and authors themselves. As the students of nature have usually little time for autobiography, we possess of the long list of great names in modern French science hardly any personal records such as are so plentiful in English literature. What we miss in many of these elaborate and frequently gossiping narratives is a just appreciation of the position of the subject of the biography in the history of science, literature, and thought, a definition of the exact place and importance which belongs to him and his work. This is what is given in such a masterly and condensed form in the better *éloges* of Fontenelle, of Cuvier, of Arago, and other secretaries of the French Academies. In Germany biographical literature is less developed than in this country, and memoirs are almost absent—those of Varnhagen von Ense and of Perthes, among literary men, being remarkable and rare exceptions. Similarly the great correspondence carried on by Goethe through nearly sixty years is a unique monument of his genius and his influence, comparable only to that of Voltaire during the last century. R. Haym in his biographies of Hegel, Wm. von Humboldt, and Herder, which combine the biographical with the historical and critical elements, has done a great work, and these books are invaluable contributions to the history of thought. Justi's 'Winckelmann' is of equal importance; but Dilthey's 'Schleiermacher' is unfortunately unfinished.

honours and championships—in all the noble and ignoble forms of racing, where much energy, which might more usefully have been merged in co-operative action, is sacrificed for the sake of individual distinction. But where the height of genius forbids emulation, where the towering intellect has distanced all records, this individualism has produced single specimens of the greatest work, examples of the highest moral worth. It is not in the courses of scientific work alone that we shall have occasion to mark the peculiarity of British, especially of English, thought; but it is interesting to note how even in this sphere, which more than any other seems to bear an international and cosmopolitan character, the genius of the nation strongly asserts itself, baffling every effort to control it or to lead it into more conventional channels.

44. Changes during the last fifty years.

The last fifty years have done much to destroy the peculiarly national customs, the idiosyncrasies of the different peoples. English institutions have been copied in France, and German customs introduced into England; it has recently been stated that the older type of scientific amateur which existed in this country is dying out, being rendered impossible by the more complicated machinery of science, the manifold conditions on which progress depends. It seems to me doubtful whether this view is correct. Surely the advance of the highest kind of thought will always depend upon the unfettered development of the individual mind, regardless of established habits, of existing forms of expression, or of adopted systems; just as the diffusion and wholesale application of single discoveries will depend on a ready and efficient machinery and organisation; whilst their influence on general thought and literature will depend on the cultivation of a perfect form, of an expressive and elegant style. The French alone in the beginning of the century could boast of the last; the Germans have most successfully developed the second; whilst England, the country of greatest individual freedom, has been the land most favourable to the growth of genius as well as eccentricity, and has thus produced a disproportionate number of new ideas and departures. Nor is it to be desired that the reliance of genius on itself should be in any way curtailed, as it is impossible to foretell whence the new light will come which is to illuminate future ages. This individualism of the English mind presents other accompanying features, and these are of great interest to the historian of thought. They manifest themselves in the province of science as much as in other provinces. We will now study them more closely; in the sequel we shall meet with them in other departments also.

Hitherto our observations on English science have nearly all referred to only one side of modern scientific work,—the side on which lie the experimental, measuring, and calculating sciences; those sciences which abroad are termed "exact"; in which mathematical notions and methods, be it of measurement or of calculation, obtain. But these sciences cover only one side of reality. We noticed how in France, during the great scientific epoch, the other side of nature, that which exhibited and was filled by the phenomena of life, was simultaneously explored with equal originality and equal success. As Laplace was the great representative of the one, so Cuvier was the great representative of the other. We have also seen how in

Germany this latter department of research was specially cultivated, how all the mathematical, experimental, and philosophical sciences combined to organise the one great science of physiology or biology, with its central and crowning problem—the problem of consciousness. We also noted how this science worked a great reform in the whole domain of medical theory and practice. Let us now return to the question, What has Great Britain done during the first half of this century in this great department of scientific thought? Single great names, like those of Harvey,[1] marked in former centuries discoveries in the natural sciences equal to those of Newton in the mathematical; the name of Ray[2] is still preserved in the

45. British contributions to biology.

[1] William Harvey (1578-1657), a native of Kent, received his medical education in Italy, especially in Padua, under Fabricius of Acquapendente. The discovery of the circulation of the blood belongs to the year 1616, and is almost contemporary with Napier's invention of logarithms. This discovery is contained in the manuscript of Harvey's lectures preserved in the British Museum, but the publication did not take place till 1628 ('Exercitatio anatomica de motu corporis et sanguinis in animalibus,' published at Frankfort). Although Harvey was drawn into long controversies by his publication of this work, he had the satisfaction of seeing his discovery generally recognised. Descartes abroad took Harvey's part in his letter to Beverwijck in 1637, and in his 'Discours de la Méthode,' published in the same year; and it is noteworthy that—as has been the case with many subsequent English discoveries—the first great acknowledgment came from the Continent, notably Holland. The acceptance in France by the faculties of Paris and Montpellier was less rapid, and in England it is well known that Lord Bacon took no notice either of Harvey's discovery or of Napier's invention. See James Spedding's preface to the "De interpretatione Naturæ Prooemium" in works of Lord Bacon, vol. iii. p. 507, &c.; also Harvey's own opinion on Bacon, ibid., p. 515. Hobbes, on the other hand, "was eager to accept Harvey's revolutionary discovery" (Croom Robertson, 'Hobbes,' p. 123), and refers to Harvey in the dedication of the 'De Corpore' (1655) as "the only man I know that, conquering envy, hath established a new doctrine in his lifetime" (ibid., p. 187 n.) On Harvey's other works, notably on the work 'De Generatione,' see, *inter alia*, Huxley, 'Science and Culture,' 1888, p. 333, &c.

[2] John Ray, or Rajus, as he is called abroad (1628-1705), a native of Essex, was a Cambridge man; he, however, gave up his fellowship in 1662, feeling himself unable to subscribe to the Act of Uniformity of 1661. He was one of the first great classifiers of plants; he collected a vast amount of information, beginning with the neighbourhood of Cambridge and extending it in travels over Great Britain and the Continent with Willoughby. The 'Historia Plantarum'—describing 18,625 species of plants—appeared from 1685 to 1704 in 3 vols. The first volume contains a chapter on the anatomy and physiology of plants, which was much extolled by Cuvier and recommended for republication. The "Ray Society," started in 1844 "for the publication of works on Natural History," brought out among many other excellent and celebrated works (such as Darwin's 'Monograph of the Family Cirripedia'), Memorials (1844) and Correspondence (1848) of John Ray: it also translated that eccentric specimen of the "Naturphilosophie" Oken's 'Elements of Physio-philosophy,' 1847. A contemporary of John Ray was Nehemiah Grew (1628-1711), one of the first to make extensive use of the microscope (invented in Holland between 1590 and 1600) for the examination of the anatomy and physiology of plants. After Oldenburg he was Secretary of the Royal Society together with Hooke. The Society printed his 'Anatomy of Plants.' About the same time it seems to have exhausted its funds in printing Willoughby's 'Historia Piscium,' so that it was unable to carry out its design of defraying the cost of printing the 'Principia.' This was generously done by Halley. See Weld, 'History of the Royal Society,' vol. i. p. 309, &c.

Society called after him: in more recent times Hutton formed a school in geology which was opposed to that of Werner, emanating from Germany.[1] Hunter, the anato-

[1] Beneath the strife of the Wernerians and Huttonians, or the Neptunists and Plutonists as they were termed, the real merits of Robert Jameson (1774-1854) and James Hutton (1726-97) have sometimes been overlooked. Both were ardent naturalists who spent their lives in observation and study of nature. They made Edinburgh for some time the centre of geology in this country. Jameson was fifty years Professor of Natural History, founded the first school of Natural History in this country (see Cossar Ewart's address, quoted by Sir A. Grant, 'Story of the University of Edinburgh,' vol. ii. p. 444), trained a number of eminent naturalists, among whom are Edward Forbes and Grant (*N.B.*—The name of Darwin must be added with caution, see his 'Autobiography,' vol. i. p. 44, &c.), founded the Edinburgh Museum of Natural History, which includes the Huttonian collections, and founded the Wernerian and Plinian Societies of Natural History. James Hutton, though not a teacher like Jameson, exerted a great influence through John Playfair, who popularised his views in his 'Illustrations of the Huttonian Theory of the Earth' (1802). It is termed by Geikie a "classical contribution to geological literature." Though the opposition of Hutton's theoretical views to those of Werner gave him a great reputation as a theorist, it is claimed for him that he first among geologists disclaimed the intention of investigating the origin of things, and thus put an end to the cosmogonies of the eighteenth century. Such had been promulgated in all the three countries by the most illustrious philosophers and naturalists, by Burnet, Buffon, and Leibniz. On Hutton's great merits see especially Huxley, "Essay on Geolo-

mist, acquired a world-wide reputation in the latter part of the eighteenth century.

Many other students of nature could be added to this list. Perhaps none has acquired greater popular celebrity

46. Jenner. than Jenner.[1] This he acquired through his extraordinary discovery, by which he grappled successfully with one of the most prevalent and distressing epidemics from which

47. English love of nature. former generations had to suffer. The study of animated nature, the observation of the sky and the heavens, have always been favourite occupations of Englishmen. The love of travels abroad and of the country at home has favoured a close intercourse with nature. A fickle and humid climate invited the superior skill of the agriculturist and the gardener, and rewarded them with heavier crops and more luxuriant verdure.[2] The chill of the long winter

gical Reform" (1869. Reprinted in 'Lay Sermons and Addresses,' No. 11). He is there considered as the first representative of "Uniformitarianism" against the older "Catastrophism." Uniformitarianism has been followed by "Evolutionism."

[1] Edward Jenner (1749-1823), one of the greatest benefactors of mankind, spent twenty years on the farms of Gloucestershire, following the advice of his friend and master John Hunter, "Don't think, but try," before he undertook the first inoculation of cowpox on the 14th of May 1796. About the end of the century the process of vaccination, which dispelled the older process of inoculation—introduced into England by Lady Mary W. Montagu in 1721—had become generally known in Europe. The governments of the Revolution in France and the Academy of Sciences had at the end of the century occupied themselves a good deal with the cure of smallpox, both Voltaire and d'Alembert having taken great interest in the subject.

[2] The yield of an acre in wheat is in England about 30 bushels or one ton of grain; next comes Belgium, then Germany, then France; the average yield in the United States of America is barely one-half of that in England. The yield of an acre in Scotland exceeds slightly that in England. In Scotland farming is carried on with much skill and enterprise, and, in spite of the severe climate, gardening is probably further developed there than in any other country. It appears that the first voluntary organisation for the improvement of agriculture was the "Society of Improvers in the Knowledge of Agriculture in Scotland" formed in 1723, of which the Earl of Stair was one of the leaders. Though it counted 300 members, it was short-lived: its 'Select Transactions' were published by Maxwell in 1743.

stimulated active exercise and outdoor sport; the abundant rains, which fed the many rivulets with a constant supply of fresh water, suggested the cultivation of that pastime of which Izaak Walton had left a classical description, long before Rousseau in France made the love of nature a fashionable sentiment. Lord Bacon pointed to the study of natural phenomena as the only source of knowledge. Evelyn wrote a treatise on forest-trees, and the old-fashioned English flower-garden is immortalised in Bacon's 'Essays,' in the "Winter's Tale," in Cowper's "Task," and in the works of many other poets. Through the literature of the eighteenth century there runs a vein of increasing love and knowledge of natural objects and natural scenery, beginning in Thomson and Gray, widening and deepening in Erasmus Darwin and Cowper, and attaining full vigour and originality in Burns and Wordsworth, as also in the school of English landscape-painting. William and Caroline Herschel com-

Next came the Bath and West of England Society, 1777; the Highland Society, 1784; and the National Board of Agriculture, 1793. The 'Farmer's Magazine' was started in 1800. About the same time that Lawes and Gilbert in England and Liebig in Germany gave such an impetus to scientific farming through their experiments and publications, "Mr John Finnie at Swanston, near Edinburgh, having suggested (1842) to some of his neighbours the desirableness of obtaining the aid of chemistry to guide farmers in many departments of their business, the hint was promptly acted upon, and these Mid-Lothian tenant-farmers had the merit of originating an Agricultural Chemistry Association (the first of its kind), by which funds were raised, and an eminent chemist engaged" ('Ency. Brit.,' article "Agriculture," vol. i. p. 305). There is probably no country where farming is such a favourite pursuit of gentlemen of leisure and wealth as Great Britain, or where the intelligence of higher society and of the universities is so liberally transferred to the benefit of the country, of its population, its crops, and its live-stock. Among many examples of the past and present I mention as an outcome of this spirit the little volume by Sir Thomas Dyke Acland, 'On the Chemistry of Farming' (London: Simpkin & Co., 1891), and his liberal patronage of agriculture in the west of England.

menced the long line of amateur star-gazers of this country; Luke Howard's study of clouds drew from the kindred spirit which lived in the great Goethe a loving memorial;[1] and John Dalton was induced by the mists and fogs of his native lake country to join in the foundation of the modern science of meteorology.

48. Union of individualism and naturalism in England.

We now discover the reason why the strong individualism of the English character, which prompted new departures and inspired new ideas in science, as it produced adventures and novel enterprise in life and arts, has not more frequently led to discouraging failures in the latter, or to eccentricity and dreaminess in the former; why it has, on the whole, alike in practical work and in scientific study, been rewarded by signal success. The rare genius, gifted with the power of original thought, who found no academy ready to call him, no schools where he could be trained, no university eager to nurse and develop his

[1] Luke Howard (1772-1864), a member of the Society of Friends, was one of the many lovers of nature and amateur naturalists of this country in whom new sciences—like that of meteorology—are nursed during their unpretentious infancy. He himself gave a simple narrative of his life and doings to the great Goethe, who, attracted by his attempted classification of clouds (about 1802, published in his 'Climate of London'), had addressed some lines to him, accompanying them by a statement in verse of Howard's description of the stratus, cumulus, cirrus, and nimbus:—

"Er aber, Howard, giebt mit reinem Sinn
Uns neuer Lehre herrlichsten Gewinn:
Was sich nicht halten, nicht erreichen lässt,
Er fasst es an, er hält zuerst es fest;
Bestimmt das Unbestimmte, schränkt es ein,
Benennt es treffend!—Sey die Ehre Dein!
Wie Streife steigt, sich ballt, zerflattert, fällt,
Erinnre dankbar Deiner sich die Welt."

Goethe subsequently tried to get some information about Howard's way of life, "so that I might see how such a mind is formed, what opportunities, what circumstances, have led him into ways of looking at Nature naturally, have taught him how to devote himself to her, so as to find her laws and to prescribe these again to her in a natural human manner." In his autobiographical narrative (reprinted in the last volume of Goethe's Works) Howard refers to the meteoric phenomena of 1783, mentioned also in Cowper's Letters (13th June 1788), and White's 'History of Selborne.'

talent, did not retire into the depths of his own consciousness, or surround himself with the artificial atmosphere of erudition. The result of such a process can be abundantly traced in other countries and other literatures. In England the isolation from society and the solitariness of genius threw him into the arms of Nature, and she has in many instances, in science, in poetry, and in art, rewarded and refreshed him by a novel inspiration—she has lifted her veil to his loving eye and revealed to him one of her secrets. The individualism of English science has been tempered by its naturalism. A type of this peculiar form of the naturalist was Gilbert White, the natural historian of Selborne.[1]

[1] A long list might be given of these retired nature-loving souls, among whom Charles Darwin will always rank as the greatest and most conspicuous. I give here a few names in addition to those mentioned in the text.

John Gough of Kendal (1757-1825) might, according to John Dalton (see his Life by Henry, pp. 9 and 10), "be deemed a prodigy in scientific attainments. . . . Deprived of sight in infancy by the smallpox, . . . possessing great powers of mind, he bent them chiefly to the study of the physical and mechanical sciences. It was he who first set the example of keeping a meteorological journal at Kendal; . . . he knew by the touch, taste, and smell almost every plant within twenty miles; he could reason with astonishing perspicuity on the construction of the eye, the nature of light and colours, and of optic glasses," &c., &c. For about eight years Dalton and he were intimately acquainted.

George Edwards (1694-1773) of Stratford, Essex, was the author of the 'History of Birds,' which he published between 1743 and 1764 in six volumes. He had journeyed through France and other countries, and gave engravings of six hundred subjects not before delineated by naturalists.

Still more remarkable was Thomas Edward (1814-86), the shoemaker of Banff, who, having been turned out of three schools for his zoological propensities, without friends, without a single book on natural history, not knowing the names of the creatures he found, gained a knowledge unique in its freshness and accuracy. At the University of Aberdeen, where he exhibited his collections, he was told by the professors that he came "several centuries too soon," as they had then no chair of Natural History. His life has been written by Smiles, 1876.

Edward Forbes (1815-54) of Douglas, Isle of Man, a born lover

Not long after Ray and Linnæus had attempted the artificial and logical classification of living beings, and about the same time that Buffon in France infused into the literature of his country a somewhat pretentious love of
49. White of Selborne. nature, Gilbert White, in a simpler and more healthy style, betook himself to describe the aspect that nature presented when viewed from the quiet home of an English country parson. He may be said to have represented that other

of nature, "led an unusually full life, occupied in promoting science and arousing enthusiasm and awakening intelligence in others. To almost every department of biology he rendered much service, especially by connecting various branches together and illustrating one by the other. Though his published works have been few, his ideas have been as the grain of mustard-seed in the parable" ('Dictionary of National Biography'). After holding various badly paid offices in London and elsewhere, he succeeded Robert Jameson as Professor of Natural History at Edinburgh (see 'Memoir of E. Forbes,' by G. Wilson and A. Geikie, 1861).

Hugh Miller (1802-56), the self-taught stonemason of Cromarty, combined the soul of an artist with that of a naturalist. His writings occupy a place by themselves in English Literature. "The principal scene of his own investigations was the Cromarty district, where he ransacked every wrinkle of the hill-side, and traced every stratum sawn through by the watercourse, and where on the beach at ebb, in indurated clay of bluish tint and great tenacity, belonging to the old Red Sandstone formation, he discovered and dug out nodules which, when laid open by a skilful blow of the hammer, displayed organisms that had never been seen by the human eye." In September 1840 there appeared in the 'Witness' a series of articles entitled "The Old Red Sandstone." They formed the nucleus of a book of this title which established the reputation of Miller as an original geologist, as a practical thinker and fascinating writer. 'My Schools and Schoolmasters' is a masterpiece of the English language. "In an age prodigal of genius, yet abounding also in extravagance, glare, and bombast, the self-educated stonemason wrote with the calmness and moderation of Addison." "The fossil remains seem in his glowing pages to live and flourish, to fly, swim, or gambol, or to shoot up in vegetative profusion and splendour, as in the primal dawn of creation" (Carruthers, quoted by Peter Bayne in 'The Life and Letters of Hugh Miller,' 2 vols., 1871).

David Robertson, the naturalist of Cumbrae in the Firth of Clyde (born in 1806), was a farm-labourer till he was twenty-four, then took to the study of medicine, and had afterwards for many years a china and hardware shop in Jail Square, Glasgow. He gained a sufficient independence to be able to retire in 1860 to Great Cumbrae, where he devoted the rest of his life to a study of nature. Especially in "the marine section, by his own unaided efforts, he opened up in a remarkable degree the zoology of the Firth of Clyde.

side of natural science, which does not try to comprehend nature through the artificial arrangement or classification of a museum, but in those connections, among her own animate and inanimate objects, which constitute reality, and are the characteristics of life and development. It was the real, not the artificial, Jardin des Plantes, where he and his successors tried to study natural objects and the habits of living beings.[1] Another re-

Many animals, till then accounted rare, are now known to exist as common objects, while the annals of science have received many important additions of animals altogether new to natural history records — discoveries which have caused the Firth of Clyde, and more particularly the Cumbrae Islands, to become one of the best explored and most widely known districts of Britain" (Gray, Secretary of the Glasgow Natural History Society, quoted by Thomas R. R. Stebbing in his 'Naturalist of Cumbrae,' London, 1891).

William Pearson (1767-1847) of Borderside, Crosthwaite, near Kendal, was a self-educated yeoman, who after many years spent in a bank at Manchester retired to a small patrimonial estate on the southern border of Westmorland. He possessed a choice collection of books, representing fully the English poets of all ages, and in translation the best German authors. "Of the habits of birds and other native creatures around him he was a watchful observer, and he described them in purest English with a charm that suggested no disadvantageous comparison with White of Selborne" (see Groves, 'Life of Hamilton,' vol. iii. p. 15). He was a friend of Wordsworth.

To this list, which could be indefinitely extended, I might add another, beginning with Thomas Bewick (1753-1828), the reviver of wood-engraving in England, who lent his art and life to the delineation of nature. 'British Birds' (1797-1804) is a standard work on the borderland of art and science, in which many other British artists have, in humbler or more extensive fields, laboured with so much faithfulness and success.

[1] The 'Complete Angler' and the 'Natural History of Selborne,' are types of a class of literature peculiar to this country. In these classical productions we are introduced into the nursery of English thought, poetry—nay, of science itself. These, as the nation draws ultimately its wealth from the produce and culture of the land, on their part receive valuable ideas from a study of nature. The purity and originality of English art and poetry have their home in the same region. Gilbert White (1720-93) was born and lived in the little Hampshire village of Selborne. He was one of five brothers, all of whom, in various positions and vocations of life, followed the study of nature in its minute and local aspects, combining with it an antiquarian taste. He may not only be classed with the naturalists, but belongs also to that class of writers, peculiar also to England, who devote their time to the compilation of local records, of county histories, and to the preservation of the relics and memorials

action against the theorising methods which had come over from the Continent led to the formation of the Geological Society in the year 1807. At that time the war of the Wernerians and Huttonians, or, as they were also called, the Neptunists and Plutonists, was raging in the northern metropolis. The Geological Society of London was established with a view to "multiply and record observations, and patiently to await the result at some future period—that is, its founders resolved to apply themselves to descriptive geology, thinking the time not come for that theoretical geology which had then long fired the controversial ardour of Neptunists and Plutonists."[1] Fifty years after the formation of this society

50. The Geological Society.

of country life in bygone centuries. The series of letters written between the years 1765 and 1787 containing "the observations of forty years," and published, 1789, with the title 'The Natural History and Antiquities of Selborne,' had the object "of laying before the public his idea of parochial history, which, he thinks, ought to consist of natural productions and occurrences as well as antiquities." To him "nature is so full that that district produces the greatest variety which is the most examined." He early insists on the necessity of monographs in natural history; suggests the usefulness of a "full history of noxious insects"; gives in a series of letters a faithful and minute description of the swallow tribe as they are found in his country; traverses the Downs of Surrey with a loving eye a hundred years before they became celebrated through the greater Darwin; makes valuable observations about "earthworms," suggesting a monograph on them; suggests, in an age which was governed by the systematising mania, that "the botanist should study plants philosophically, should investigate the laws of vegetation, should promote their cultivation, and graft the gardener, the planter, and the husbandman on the phytologist," as "system should be subservient to, not the main object of, pursuit."

[1] "The one point the catastrophists and the uniformitarians agreed upon when this society was founded was to ignore it [*viz.*, geological speculation]. And you will find, if you look back into our records, that our revered fathers in geology plumed themselves a good deal upon the practical sense and wisdom of this proceeding. As a temporary measure I do not presume to challenge its wisdom; but in all organised bodies temporary changes are apt to produce permanent effects; and as time has slipped by, altering all the conditions which may have made such mortification of the scientific flesh desirable, I think the effect of the stream of cold water which has steadily flowed over geological speculation within these walls has been of doubtful beneficence" (Huxley on "Geological Reform," Address to the Geological Society, 1869; reprinted in 'Lay Sermons,' &c., 1891, p. 207).

the author from whom I quote, Dr Whewell, in the third edition of his 'History of the Inductive Sciences,' could still say that "their task was not yet finished, their mission not yet accomplished—that they had still much to do in the way of collecting facts; and in entering upon the exact estimation of causes, they have only just thrown open the door of a vast labyrinth which it may employ many generations to traverse, but which they must needs explore before they can penetrate to the Oracular Chamber of Truth."[1] One of the many individuals in this country who "had long pursued his own thoughts without aid and without sympathy"[2] was William Smith. "No literary

51. William Smith.

[1] See Whewell, 'History of the Inductive Sciences,' 3rd ed., vol. iii. pp. 428, 518. Lyell, 'Principles of Geology,' 3rd ed., vol. i. p. 102, &c.

[2] Whewell, *loc. cit.*, vol. iii. p. 427. William Smith (1769-1839), a native of Oxfordshire, has been called the Father of English Geology. He was—like so many other naturalists of this country—an amateur in his scientific studies, which were conducted on the occasions of his elaborate surveys of Oxfordshire, Warwickshire, and Somersetshire in connection with the engineering of several canals. He initiated in England the science called on the Continent "Stratigraphy," observed the successive layers in the geological structure of the country, and in 1799 prepared a tabular view of the order of the strata and their organic remains in the neighbourhood of Bath. For many years after this he was occupied in preparing his Geological Map of England and Wales, which appeared on the five miles to the inch scale in 1815 in fifteen sheets. He was popularly known as "Stratum Smith," but remained almost unknown abroad, as he himself also seems to have taken little notice of Continental geology or prevailing theories. Though he began earlier than Cuvier and Brongniart, they anticipated him by publishing in 1811 their mineralogical description of the Paris Basin, thus becoming the founders of the science of palæontology (see Peschel, 'Geschichte der Erdkunde,' München, 1877, p. 714, &c.) Of the Geological Map Lyell says ('Principles of Geology,' vol. i. p. 101) that it "remains a lasting monument of original talent and extraordinary perseverance; for he had explored the whole country on foot without the guidance of previous observers or the aid of fellow-labourers, and had succeeded in throwing into natural divisions the whole complicated series of British rocks."

cultivation of his youth awoke in him the speculative love of symmetry and system; but a singular clearness and precision of the classifying power, which he possessed as a native talent, was exercised and developed by exactly those geological facts among which his philosophical task lay. Some of the advances which he made had been entered upon by others who preceded him; but of all this he was ignorant, and perhaps went on more steadily and eagerly to work out his own ideas from the persuasion that they were entirely his own." In what he did and published, beginning with the year 1790, "we see great vividness of thought and activity of mind unfolding itself exactly in proportion to the facts with which it had to deal."[1]

About the same time that geological studies received a great impetus in this country from two distinct centres—the philosophical teaching in the Scotch metropolis, and the more empirical labours of the Geological Society—a signal discovery in another line marked a great step in
52. Charles Bell. anatomy and physiology. This was Charles Bell's discovery, in the year 1807, of the difference between sensory and motor nerves, "doubtless the most important accession to physiological knowledge since the time of Harvey."[2]

[1] Whewell, *loc. cit.*, p. 423.

[2] This statement, taken from Dr Henry's 'Report of the British Association,' vol. vi., and repeated by Whewell (*loc. cit.*, vol. iii. p. 352), probably requires a correction, since Du Bois-Reymond and others have placed in their true historical position the great merits of Descartes, who by the discovery of the principle of "reflex action" "did for the physiology of motion and sensation that which Harvey had done for the circulation of the blood, and opened up that road to the mechanical theory of these processes which has been followed by all his successors" (Huxley in his address to the British Association at Belfast, 1874; reprinted in 'Science and Culture, &c.,' p. 200, &c.) The first enunciation of the principle of reflex action had been variously ascribed to Joh. Müller, Prochaska, Willis, till Du Bois-Reymond in his most interesting 'Gedächtnissrede auf Joh. Müller' (Berlin Acad., 1859) showed how the merit of enunciating it is due to Descartes, whose tract on 'Les Passions de l'Âme' was published in 1649. Both Du Bois-Reymond and Huxley give full extracts from the writings of Descartes. There seems, however, to be some doubt to what extent Descartes substantiated his mechanical view of the action of the nervous system by actual experiments. Richet in his 'Physiologie des Muscles et des Nerfs' (Paris, 1882, p. 505, &c.) refers to this, and while giving Descartes his due, also says that practically from the time of Galen to Charles Bell no marked progress had been made in the knowledge of the nervous system, and that this belongs almost entirely to the nineteenth century (pp. 502, 507, 514). Huxley, who takes a much higher view of the merits of Descartes, says he was not only a speculator, but also an observer and dissector (*loc. cit.*, p. 201), and actually places him at the head of modern physiology (p. 334, &c.)

Bell's career was a unique one. He had early severed his connection with the great medical schools of Edinburgh, where his brother taught. He lectured and practised privately in London, where he gained a considerable reputation; but in his case also it was on the Continent that his greatness was more generally recognised. As in Dalton's case, his countrymen were slow to do him justice.[1] In France he had so great a name that a celebrated

[1] Charles Bell (1774-1842) was born at Edinburgh. His elder brother, John Bell (1763-1820), who was a lecturer of great repute in the extra-mural School of Surgery at Edinburgh, first drew his attention to the medical profession. It was only late in life, and after he had gained his European renown, that he was appointed to the Chair of Surgery at the University of Edinburgh, which had been created in 1831, and it does not appear that he was at all sufficiently appreciated in this position: he used to say, "I seem to walk in a city of tombs," being unknown in the city of his birth (see Sir A. Grant, 'University of Edinburgh,' vol. ii. p. 453). Whilst Charles Bell established the difference of sensory and motor nerves, and dispelled "the confusion which prevailed up to that time in the minds of anatomists and physiologists regarding the functions of the various nerves," the merit of proving by strict experiment the correctness of Bell's theorem belongs to Johannes Müller (1831), who showed it in the frog, and to Magendie and Longet, who succeeded in exhibiting it in warm-blooded animals. Up to the date of Müller's experimental proof nobody regarded "Bell's doctrine as more than an ingenious and indeed plausible, but nevertheless not sufficiently demonstrated, idea" (see Du Bois-Reymond, 'Reden,' vol. ii. p. 176, &c.; also Henle's description of the demonstration given by Müller in Paris on the 13th September 1831 to Humboldt, Dutrochet, Valenciennes, and Laurillart, in 'Jacob Henle,' by Merkel, 1891, p. 83).

anatomical professor, when Bell visited his lecture-room, dismissed his class with the words, "C'est assez, messieurs, vous avez vu Charles Bell."

In Germany one of the great achievements of Johannes Müller, through which he acquired European celebrity, was his actual experimental proof of Bell's thesis, with which he had occupied himself for many years.

Instances might be indefinitely multiplied, showing the individual greatness, but also the isolation, of English men of science and their discoveries; how the latter emanated so frequently from the depths of original genius in intimate communion with nature; how they as frequently lacked those social advantages, that organisation for development, which the great schools and establishments of the Continent all through the century have possessed in so eminent a degree. Not only in the study of nature has this individual character of British research

53. Historical geography.

shown itself, though it is here most conspicuous. In the exploration of foreign lands and the monuments of bygone civilisations—in the historical branches of research, we meet with similar pioneer work. Who does not recall the names of Dr Young and of Layard? I will mention only one instance of this kind, where individual ability joined to fortuitous circumstances laid the foundation of a new branch of research on the borderland of natural and political history, the geography of ancient and modern Greece—the exploration of the land which produced the most remarkable, and perhaps the most intense, culture which the world has yet seen. Note what Ernst Curtius [1]

[1] See his essay in the 'Preussische Jahrbücher,' vol. 38, on M. W. Leake, and his discourse, "Der Wetteifer der Nationen in der Wiederentdeckung der Länder des Alterthums" (1880), both reprint-

says,—the man to whom we are most indebted for the systematic historical and artistic study of this remarkable country; whose mind has better than any other succeeded in representing to itself the natural and ideal features of that country and that bygone race, and who has drawn in his writings a series of pictures, reproducing that past glory in unequalled perfection. In tracing the beginnings of the modern science of archæology or historical geography, he assigns to England and Englishmen a foremost place as pioneers. "In England there was no mediæval tradition which suggested expeditions to the East, nor did there exist any external occasion or public interest, but it was a free and purely human attraction which led Britons to the classical soil, and private means have made all the sacrifices that were required in order to satisfy a craving of the soul.[1] . . . England became the

ed in that valuable collection, 'Alterthum und Gegenwart,' 3 vols., Berlin, 1882 and 1889. In the rediscovery of the countries of ancient civilisation, Italians made the beginning with Cyriacus of Ancona (from 1412 to 1442). Then follow the French—Jacob Spon of Lyons, a German by birth, being among the earliest (1675). The generation that succeeded the age of Scaliger produced the first maps of Greece (Paulmier). Then follows England, where the name of Arundel has acquired a doubtful celebrity through that wholesale acquisition of ancient relics which Mr (afterwards Sir William) Petty and John Evelyn carried on in his name in Greece and Asia Minor. It is interesting to note here the position that Germany holds in the growing science of archæology, of which Winckelmann may be considered the founder. "The Germans possessed no advantages and resources by which they could take part in the contest of nations over the rediscovery of the countries of ancient history. . . . Whilst in Italy it was national feeling, in France political relations with the East, in England the love of collecting and travelling common among the aristocracy, which established the connection of the Old World with the New, in Germany it was the workroom of the professor" (Curtius, *loc. cit.*, vol. ii. p. 229).

[1] E. Curtius, *loc. cit.*, vol. ii. p. 226. "In the year 1742 Stuart and Revett wandered among the ruins of Rome, and recognised that in its relics they beheld only later and degenerate forms of ancient art. Six years later they set sail for Greece. It was, after Cyriacus of Ancona and Jacob Spon of Lyons, the third journey of exploration; but it was the first in scientific importance" (p. 227).

treasury of the wonders of the East, and whilst the Continent was closed to her, her travellers flocked to Hellas, registering with marvellous patience, watch in hand, on the back of the slowly marching mule, piece by piece, the remains of antiquity. . . . The political mission, headed by Martin William Leake, was as such quite unsuccessful; for science, it was of priceless value: from the moment that Leake trod on classic soil the reminiscences of Homer and Herodotus were kindled, and he saw clearly his life-work before him. Under the powerful impressions produced by the great table-land of Asia Minor with the solitary snow-peak Argaios, deeply moved by the deserted places, marching over Grecian inscriptions, over sarcophagi and temple ruins, he felt the irresistible charm of the attempt to explore and to understand these homes of ancient culture.[1] . . . The scientific result was a lasting gain for the civilised world, and the travels which he made from 1805 to 1807 mark an epoch in our knowledge of Grecian antiquity."[2]

54. Martin William Leake.

But the labours of the pioneer in science, life, or art, which form so conspicuous an element of this country's mental work during the first two-thirds of the century, must be supplemented and carried further by a great army of patient and trained explorers. Original ideas must be cast into an appropriate and elegant form; new discoveries must be extended and criticised by strict methods of research; erudition and philosophy are required to guarantee completeness and depth. In the large domain of the historical sciences these labours of

[1] E. Curtius, *loc. cit.*, p. 307.
[2] Ibid., p. 312.

the school and the study are even more important than in the exploration of nature, and thus it is not surprising that in these especially the bulk of the work, though frequently begun by Englishmen, has been carried on by the great schools and academies of the Continent. In the regions of exact science, with which we are at present more immediately concerned, there will always be a much greater inducement for original minds to forsake the beaten track, the recognised method or system.

The genius gifted with a larger field of vision and a keener glance will always feel the longing to return to Nature herself, and the practical man will be allured by the prospects of application of science in the arts and industries. Both will find their reward; nor is it likely that the works of Faraday and Darwin should be the last illustrious examples of great and far-reaching ideas sprung from the living intercourse of original genius and nature without the support of any school; or that the practical success of the Atlantic cable will be the last fruit of the rare combination of highest mathematical genius with industrial and commercial enterprise. The historian of thought is forced to admit that such rare combinations are most likely to spring up amongst a people who have always opposed the rule of systems and methods, of schools and academies; who have nursed and cherished an intimate communion with nature; and for whom practical interests and adventures have always preserved an irresistible attraction.

Living in an age when the foundation in England and in Germany of institutions similar to the Académie Fran-

çaise has been seriously discussed,[1] when the British Association has been copied abroad,[2] and when scientific men of eminence are joined in conference as to the advisability of founding a professorial university in London, in imitation of the great University of Berlin, it seems appropriate to recall the various ways and means by which, mainly in this century, the exact spirit of research, the mathematical method of investigating nature and reality, has been established and diffused.

55. Work of the three nations compared.

France was the country in which the modern scientific methods of measurement, calculation, and classification were first practised on a large scale, reduced to a system, and employed for the investigation of the whole of nature. The Academy of Sciences, together with the High Schools of Science, the Natural History collections, and Medical Institutions, all in close connection, furnished an organisation of the highest intelligences of the nation, by which

[1] See Matthew Arnold's essay on 'The Literary Influence of Academies,' and Du Bois-Reymond, 'Uebereine Kaiserliche Akademie der deutschen Sprache,' 1874, reprinted in 'Reden, &c.,' Leipzig, 1886, vol. i. p. 141, &c. On the other side see Huxley in 'Critiques and Addresses,' ed. of 1890, p. 113, &c.

[2] The British Association, itself established somewhat on the model of the German "Naturforscher-Versammlung," founded by Oken and Humboldt (see *supra*, p. 238) in the year 1831, has become the model of the younger "Association française pour l'Avancement des Sciences," founded in 1872 under the presidency of Claude Bernard. It held its first public meeting at Bordeaux in 1874. In the opening addresses of the president, M. de Quatrefages, and the secretary, M. Cornu, the elder sister in England is referred to. A characteristic passage in M. Quatrefages' address as regards the results achieved by the British Association is the following: "Grâce à elle une partie de la population a été transformée. Les fils de ces chasseurs de renards, qui, pour se délasser de leurs rudes passetemps, ne connaissaient que des joies également violentes et matérielles, sont aujourd'hui des botanistes, des géologues, des physiciens, des archéologues" ('Comptes Rendus,' Ière session, p. 40). Following the resolutions carried in 1885, the French Association amalgamated in 1886 with the older "Association scientifique de France," founded by Leverrier in 1864. See 'Compte Rendu de la 16me Session,' vol. i. p. 1, &c.

a systematic exploration of the heavens and the earth, the inanimate and the living world, could be undertaken. At the same time, the methods of measurement and calculation were submitted to closer study; new sciences were created by the application of these methods; and problems were attacked for the first time, with which, at the end of the century, the scientific world is still occupied. It was in France also that the discoveries of the laboratory were first applied so as to contribute to the revolution of arts and industries. In all its different expressions—in the production of works of classical perfection in substance and in form, in its application to the problems of life and society, and in its influence on general literature—we find the scientific spirit, as we know it, fully established in France in the beginning of the century. About three decades later we find this spirit domiciled in Germany, the study of the exact sciences having been gradually accepted at the German universities as an integral part of the university cycle. It there met the philosophical and classical spirit, which had organised the German university system and the teaching of the higher schools, and had revolutionised historical, especially philological, studies. What might have been wanting at times in French science, historical completeness and philosophical criticism, was added in Germany. Germany has in the course of this century not only become the country where the most faithful and exhaustive record is kept of the scientific labours of the whole world, but it has also become the country where mainly those problems have been attacked which lie on the borderland of natural science and philosophy, the problems of

life and consciousness. Modern physiology, especially psychophysics, is claimed as essentially a German science.

Meanwhile England, where the introduction of the scientific spirit as an established canon of systematic and methodical research was later than in other countries, has all through this century, as before, continued to do pioneer work in many isolated branches of science: individual, as opposed to corporate effort, has here been rewarded by a succession of brilliant discoveries, which have revolutionised practical life or opened out new views into the hidden recesses of nature. For the want of organisations of research and teaching, such as other countries possessed, these ideas of English thinkers have frequently lain dormant or been elaborated by foreign talent; but this want of a recognised system, and of a standard course of study, has forced original minds into a closer communion with nature and with life, whence they have frequently returned to the laboratory with quite novel revelations. The largest number of works perfect in form and substance, classical for all time, belongs probably to France; the greatest bulk of scientific work probably to Germany; but of the new ideas which during this century have fructified science, the larger share belongs probably to England. Such seems to be the impartial verdict of history. During the second half of the century a process of equalisation has gone on which has taken away something of the characteristic peculiarities of earlier times. The great problems of science and life are now everywhere attacked by similar methods. Scientific teaching proceeds on similar lines, and ideas and discoveries are cosmopolitan property. So much more

interesting must it be for those who have been born members of this international republic of learning to trace the way in which this confederation has grown up what have been the different national contributions to its formation, and how the spirit of exact science, once domiciled only in Paris, has gradually spread into all countries, and leavened the thought and literature of the world.

CHAPTER IV.

THE ASTRONOMICAL VIEW OF NATURE.

1. The scientific spirit in the first and second half of the century.

So far I have only treated of the scientific spirit, or of the method of exact research, in a general way; showing how it was firmly established and developed in France, how it spread into Germany, and received there larger and more systematic application, and how in this country it gradually and almost imperceptibly grew out of the older experimental philosophy. This growth, as we have seen, took place partly under the influence of foreign science, but still more through the individual and unaided labours of a small number of native intellects of the very highest order, to each of whom was for a time allotted the enunciation of some specially fruitful idea. The period referred to in this survey was mainly the first half of our century; in it were most clearly marked the characteristic differences between the three great civilisations of France, Germany, and England. A step further in time would lead into the midst of our own period—into the age which has largely reaped the benefits of those earlier labours, both in theory and in practice, fully realising in many directions the predictions and even the ideals

of the pioneers of science. One of these benefits, and perhaps that which to an external beholder marks the greatest difference between the first and the second half of the century, is the greatly increased intercourse which now exists as compared with the earlier years of our century. This intercourse has reacted on the domain of thought, and produced that exchange of ideas which promotes more rapid progress. It hardly belongs to the history of thought to analyse[1] the different steps by which the great change has been brought about. Still, a very superficial glance will suffice to show how the work of bringing about an international exchange of ideas has been very characteristically divided among the three nations in which we are specially interested. It was not in the interest of thought, of science, or of literature, but rather in that of commerce and of industry, that the modern facilities of intercourse and exchange were invented and introduced.[2] We shall therefore expect to

2. Science become international.

[1] The principal dates of the introduction of steam-engines and telegraphs for facilitating communication are as follows:—

1802. The tug Charlotte Dundas, built by Symington, was tried on the Forth and Clyde Canal.

1812. Henry Bell built the Comet with side paddle-wheels. It ran on the Clyde as a passenger steamer.

1829. George Stephenson's Rocket was tried on the Stockton and Darlington Railroad, which had been begun in 1821. In the year 1829 the Liverpool and Manchester Railway was inaugurated.

1838. The first steamboats, Sirius and Great Western, crossed the Atlantic.

1833. A comprehensive system of railways was planned by the French and Belgian Governments.

1835. The first German railway was opened between Nürenberg and Fürth. The first electric telegraphs for public use were almost simultaneously constructed in England, Germany, and the United States—the first successful line being probably that constructed by Wheatstone and Cooke between 1836 and 1840. The first Atlantic cable was begun in 1857, and after repeated failures, which were in the main corrected by the scientific investigations of William Thomson (Lord Kelvin), telegraphic communication with America was permanently established in 1866.

[2] This remark applies fully to the railway system, but scarcely to the development of the electric telegraph, which was first actually used for scientific purposes by Gauss and

find them originate mainly in that country in which those larger spheres of practical work had grown unchecked and flourished—in Great Britain and its extensive dependencies. To Germany, on the other side, with its fully developed system of learning, we are indebted mainly for the complete recording, registering, and analysing of the scientific labours of the whole world. To France

Weber at Göttingen in the year 1833. The documents referring to this interesting application have recently been published in H. Weber's biographical notice of Wilhelm Weber, Breslau, 1893, p. 25, &c. We read there that soon after 1830 Gauss had been occupied with reducing his magnetical measurements to an absolute scale, having laid his celebrated paper, "Intensitas vis magneticæ ad mensuram absolutam revocata," before the Göttingen Society in December of 1832. He had induced Weber to take up similar investigations at the Physical Institute, which was situated about a mile distant from Gauss's Observatory. This distance was found to be an inconvenience, and in order to overcome it, the first longer telegraphic line in which galvanic currents were used, and which had two wires, was carried overhead between the two buildings, and the instruments and signalling arrangements perfected in the years 1833 to 1836. Both Gauss and Weber were well aware of the importance of their invention for practical purposes. The former wrote to Olbers on the 20th November 1833: "I do not know whether I have already written to you regarding a magnificent arrangement which we have made here. It is a galvanic chain between the Observatory and the Physical Institute, carried by wires in the air over the houses, up the Johannis tower and down again. The whole length will be about 8000 feet. . . . I have devised a simple arrangement by which I can instantly reverse the direction of the current, which I call a commutator. . . . We have already used this contrivance for telegraphic experiments, which succeed very well with whole words and short sentences. . . . I am convinced that by using sufficiently strong wires one might telegraph instantaneously in this manner from Göttingen to Hanover or from Hanover to Bremen" (see Schering's address on the occasion of Gauss's centenary, Göttingen, 1877, p. 15, &c.) To Schumacher, 6th August 1835, Gauss wrote as follows: "With a budget of 150 thalers [£22, 10s.] annually for Observatory and Magnetic Institute together, really extensive trials cannot of course be made. But could thousands of thalers be bestowed thereon, I think that, for instance, electromagnetic telegraphy might be carried to a perfection and to dimensions at which imagination almost starts back." Gauss estimates that fifteen millions sterling of copper wire would suffice to reach the antipodes, and he says significantly, "I do not think it impossible to invent a mechanism by which a despatch could be played off almost as mechanically as a musical-box plays off a tune when it is once fixed on a roller" (see 'Briefwechsel zwischen Gauss und Schumacher,' ed. Peters, vol. ii. p. 411, &c.)

we owe the first beginnings of a general and international system of units and measurements, which, like the common Latin tongue in former centuries, or like the universal languages of algebra or of music, enables us to express the results of scientific research in formulæ intelligible everywhere and at all times, without laborious translations and time-absorbing reductions.

3. Disappearance of national differences.

The effect of these international labours has been to destroy the clearly marked differences of national thought. At least in the domain of science the peculiarities of the French, the German, and the English schools are rapidly disappearing. The characteristics of national thought still exist; but in order to find them in the present age we should have to study the deeper philosophical reasonings, the general literature and the artistic efforts of the three nations. These aspects of the thought of our century belong to later portions of this work. I hope there to take up many of the threads which I here break off, as for the present purpose they cannot be profitably continued. To separate the scientific work of the second half of the century according to countries and nations would lead to unnecessary repetition. The second half of the century sees everywhere in the domain of science the dying out of national restrictions—in every country the introduction of foreign methods and foreign models, foreign institutions and foreign apparatus. The establishment of an observatory or a laboratory in our age lays under contribution almost every civilised country in the world, and the most international of sciences—that of electricity—fixes its units by the names of discoverers of many countries.

I therefore look upon the spirit of exact research as thoroughly domiciled in the leading countries of Europe during the second half of the century, and intend in the sequel to explain more precisely the different views, the leading ideas, under which this research is everywhere conducted. These leading ideas have themselves been more clearly brought out and recognised during this period.

4. Special scientific ideas.

The narrow spirit of the Baconian philosophy which reigned in England, the vagueness of the philosophy of nature which reigned in Germany, during the earlier decades of the century, have disappeared in favour of the more comprehensive and the stricter methods taught by Lavoisier, by Monge, by Laplace, and by Cuvier in France. New ideas of extensive bearing have been added, and in the light of these the powers and the limits of science have been more correctly recognised.

5. Philosophy of science.

To some of my readers well-known names will occur which might serve as guides to fix these leading ideas, under the influence of which the march of science has proceeded: Sir John Herschel, Auguste Comte, John Stuart Mill, and Whewell[1] have indeed done much to

familiarise the unscientific public with the progress of science and its canons of thought. And it would thus appear natural to resort to their teaching and their explanations. But this is not the road I propose to follow. Whewell's 'History of the Inductive Sciences,' being the first attempt to compass a large subject, will, like Montucla's earlier 'History of Mathematics,' always remain a standard work. It was, however, written at a time when the tendency of modern scientific thought was

[1] Of these writings the earliest is Sir John Herschel's "Preliminary Discourse on the Study of Natural Philosophy," which appeared in Lardner's 'Cabinet Cyclopedia' in 1831. The writings of William Whewell on the 'History' and 'Philosophy of the Inductive Sciences' were begun about the same time. They were planned to serve three distinct objects—to give, 1st, a philosophical history of astronomy, mechanics, physics, chemistry, and botany; 2nd, an analysis of the nature of induction and the rules of its exercise; and 3rd, to answer the question of applying inductive processes to other than material sciences—as philology, art, politics, and morals (see 'William Whewell,' by I. Todhunter, vol. i. p. 90). The 'History' appeared in 1837 in three volumes, a second edition in 1847, a third in 1857; the 'Philosophy' appeared in 1840 in two volumes, a second edition in 1847. In the course of its execution the original plan was not strictly adhered to—the scope of the History was enlarged considerably, and the Philosophy was broken up into different parts. Herschel stands mainly on the ground of Bacon's philosophy, whereas Whewell starts with the remark that "Bacon only divined how sciences might be constructed," but that "we can trace in their history how their construction has taken place"; that "though Bacon's general maxims still guide and animate philosophical inquirers, yet that his views, in their detail, have all turned out inapplicable." He accordingly aims at a "New Organ of Bacon, renovated according to our advanced intellectual position and office" (Preface to 2nd ed. of the 'Philosophy,' 1847). In the exposition of his views Whewell was greatly influenced by Kant's philosophy. He thus searches for the fundamental ideas which underlie all scientific reasoning; for "besides facts, ideas are an indispensable source of our knowledge." The historical portions of Whewell's works have met with great appreciation in England and Germany even from those who, like Herschel (see the review in the 'Quarterly,' June 1841) and Mill (see 'Autobiography,' p. 208), could not agree with his philosophy. The latter has been eclipsed by the bolder speculations of Auguste Comte, whose 'Philosophie positive' appeared in six volumes between the years 1830 and 1842 in France. Still more than Whewell did Comte emphasise the necessity of learning from the exact sciences how to treat economical and social problems in a methodical manner. Instead of the minute and frequently hesitating elaborations of Whewell, we find in Comte the bold generalisation of the three stages of knowledge—the theological, metaphysical, and positive,—which forms the groundwork of "Positivism." Of more permanent value than Whewell's and Comte's philosophies are the investigations of J. Stuart Mill, who in his 'System of Logic, Ratiocinative and Inductive' (1st ed., 1843), has laid the foundation for all subsequent treatises on this subject, and whose thoroughgoing empiricism is being more and more adopted by scientific thinkers. Like Whewell and Comte, to whom he acknowledges his obligations ('Autobiog.,' pp. 165, 209, &c.), his ultimate object was to solve the question "how far the methods by which so many of the laws of the physical world have been numbered among truths irrevocably acquired and universally assented to, can be made instrumental to the formation of a similar body of received doctrine in moral and political science" (Preface to 1st ed.)

not as clear as it has become since, and the work has also been superseded by more detailed labours, especially of German historians.[1] The 'Philosophy of the Inductive Sciences,' by the same author, was written with the object of doing something towards determining the nature and conditions of human knowledge, and had thus a philosophical rather than a historical object in view. The same can be said of Mill's 'Logic,' of Comte's 'Philosophie positive,' and of more recent works—such as Jevons's 'Principles of Science.' They form an important section of the philosophical literature of our century, and on future occasions I shall frequently have to refer to their teaching. At present I am not about to investigate the eternal principles of correct reasoning, and the particular methods adopted, consciously or unconsciously, by scientific writers of all times. What I desire to do is, to enumerate and analyse briefly the changing ideas, the general views, under the guidance of which scientific work has progressed in the course of this century. No doubt the same object was before

[1] Besides the works on the history of the special sciences contained in the Munich Collection, 'Geschichte der Wissenschaften in Deutschland,' which in many instances is not limited to German science and learning, there is the unique 'Geschichte der Chemie,' by Hermann Kopp (Braunschweig, 4 vols., 1843-47), the 'Geschichte der Physik,' by Rosenberger (Braunschweig, 3 vols., 1882-90), and Häser's 'Geschichte der Medicin' (Wien, 1875-82, 3rd ed.) In addition to the numerous works of German specialists, I must mention as of the first importance and value the histories by the late Isaac Todhunter of the 'Theory of Attraction and Figure of the Earth' (2 vols., 1873), the 'Calculus of Variations' (1861), the 'Theory of Probability' (1865), and the 'Theory of Elasticity' (continued by K. Pearson, 2 vols. in 3 parts, 1886-93). They supply the want of a good history of modern mathematics, which does not exist. Lastly, the "Deutsche Mathematiker-Vereinigung" have published in their Jahrbuch valuable histories of special branches of mathematics—notably the 'Theory of Invariants' by Franz Mayer, and the 'Modern Theory of Functions' by Brill and Noether.

6. Whewell's 'History,' and 'Philosophy.'

the mind of Whewell when, after writing his historical work, he attempted in the philosophical sequel to abstract the general ideas which have led scientific research; but it is instructive for our present purpose to note how, writing about the middle of the century, he hardly brought out any of those principles which in the course of its second half have turned out to be fruitful, and have almost become watchwords of popular science. In the year 1857, the date of the publication of the latest editions of Whewell's works, nothing was popularly known of energy, its conservation and dissipation,—nothing of the variation of species, and the evolution of organic forms,—nothing of the mechanical theory of heat or of that of gases—of absolute measurements and absolute temperature; even the cellular theory seems to have been popular only in Germany. And yet all the problems denoted by these now popular terms were then occupying, or had for many years occupied, the leading thinkers of that period. But we find no mention of them in Whewell's works.[1] So

[1] The dates of the birth of these leading ideas of the second half of our century are approximately as follows:—

Absolute measurements were started by Gauss about 1830, and the scheme published in 1833 in his memoir, 'Intensitas vis magneticæ terrestris ad mensuram absolutam revocata.' They were extended to electrical phenomena by Weber in his 'Electrodynamische Maasbestimmungen,' 1846. The absolute scale of temperature was introduced by William Thomson in 1848.

The cellular theory was propounded by Schleiden in 1838, and extended to animal structures by Schwann in 1839; the term "protoplasm" was introduced by Mohl in 1846.

The mechanical theory of heat dates from Mayer's and Joule's determinations of the equivalent of heat in 1842 and 1843.

The doctrine of the conservation of energy dates from Helmholtz's memoir, 'Ueber die Erhaltung der Kraft,' in 1847; that of dissipation of energy from William Thomson's paper "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy," 1852; it was prepared by Watt's and Poncelet's

little was the foremost champion of inductive thought able to discern the tendencies of his age: a warning to those who attempt to recognise the aims of contemporary thought.[1]

It is not, then, to the philosophical writers that I shall apply in order to trace the leading directions of scientific

definitions of horse-power and work (1826), which Whewell does not mention.

The mechanical theory of gases—not to mention the older speculations of Daniel Bernoulli — dates from Avogadro's and Ampère's hypothesis, published in 1811, "that all gaseous bodies, under the same physical conditions, contain the same number of units," from Herapath (1821) and Joule (1851).

On Whewell's position with regard to the question of the origin and variation of species, then already ventilated by Lyell, see 'History of Induct. Sci.,' vol. iii. p. 489, &c. (3rd ed.), and Huxley's remarks in the 'Life of Charles Darwin,' vol. ii. p. 192, &c. Wallace's essay 'On the Law which has regulated the Introduction of New Species' was published in 1858 along with Darwin's preliminary statement of his views.

We might form a whole catalogue of scientific terms, some of them by no means of recent origin, which are wanting in Whewell's books, but which now govern scientific progress: such are energy, work, action and efficiency, absolute measurement, to mention only physical terms. The general ideas upon which he himself lays some stress, such as those of polarity and symmetry, appear on the other hand to be vague generalisations, which have frequently led people astray.

[1] "It is a remarkable evidence of the greatness of the progress which has been effected in our time, that even the second edition of the 'History of the Inductive Sciences,' which was published in 1846, contains no allusion to the publication in 1843 of the first of the series of experiments by which the mechanical equivalent of heat was correctly ascertained. Such a failure on the part of a contemporary, of great acquirements and remarkable intellectual powers, to read the signs of the times, is a lesson and a warning worthy of being deeply pondered by any one who attempts to prognosticate the course of scientific progress" (Huxley in Ward's 'Reign of Queen Victoria,' vol. ii. p. 355). The same writer has pointed out how Auguste Comte was still more unfortunate in his opinions on contemporary science. "What struck me was his want of apprehension of the great features of science; his strange mistakes as to the merits of his scientific contemporaries; and his ludicrously erroneous notions about the part which some of the scientific doctrines current in his time were destined to play in the future" ("Scientific Aspects of Positivism," 'Lay Sermons,' 1891, p. 130). He then goes on to show how Comte treated the undulatory theory with contempt, extolled Gall, depreciated Cuvier, and spoke of the "abuse of microscopic investigations" (ibid., p. 134).

thought in our century: their position towards this thought is indeed instructive, but it is frequently unsafe.

7. Philosophy and science.

Philosophical reasoning either precedes or succeeds the labours of the scientific thinker; it rarely accompanies them. In the history of earlier times, during the first centuries of the modern period, we find some of the foremost philosophers, such as Descartes, Bacon, Leibniz, occupied in attempting to lay down the correct lines on which science should proceed, or to find general ideas which could serve as supreme principles of scientific truth. It is a rare thing to find that they have succeeded in either of these attempts. In more modern times, ever since Locke started on a different track, it has been, especially in this country, the endeavour of philosophers to abstract out of the existing volumes of scientific research the leading ideas which have proved so helpful, and to explain their origin, their bearing, and their value. Perhaps they have been more successful than their predecessors: it has, however, frequently happened to them, that whilst they were elaborately analysing some process of reasoning, or some prevailing scientific principle, science has meanwhile adopted some entirely different line, and presented an entirely unexpected development.

In this respect they resemble that school of historical politicians which in the middle of our century in Germany[1] attempted to read the signs of the times, and to

[1] This is the school represented by the historians Dahlmann and Gervinus. A good account, with a somewhat severe criticism of the aims of this school, will be found in Karl Hillebrand, 'Zeiten, Völker und Menschen,' vol. ii. pp. 205-290. "The State and Literature had grown in Germany alongside of each other without coming into contact, the former active, reticent, modest, the latter declaiming, noisy, pretentious. It appeared as if all our life had become intellectual; Gervinus himself thought so and blamed us. In reality it was

prescribe the lines on which the desired unification of the nation could be secured. Events took their own course, and the great statesman who was the central figure of the new era of European history may be excused the scorn with which he has sometimes treated these theoretical politicians.

8. Leading scientific ideas mostly very ancient.

The leading ideas which I select as marking the progress of scientific research in our century have, with few exceptions, hardly been discoveries or inventions of this age. Some of them are very old. The ideas of attraction, which in the hands of Newton and Laplace have led to such remarkable results, are of great age, and were familiar to the philosophers of Greece and Rome; the same can be said of the atomic theory, which in the hands of Dalton became such a powerful instrument. The principles of energy and its conservation can be traced back to the writings of Newton and Leibniz, and even to earlier thinkers. The same may be said of the modern ideas on heat, of the molecular theory of gases, and even of Lord Kelvin's vortices; whilst the views which through Darwin have revolutionised the natural sciences have been traced in the suggestions of much earlier writers.

not so. When the professors turned their backs on science in order to turn to politics, they imagined politics were now only beginning: with the wonted pride of learning they saw in the administrative class only labourers and clerks; for to them parliaments and freedom of the press were identical with politics. The mouthpiece of Germany was in the universities, as that of France was at the bar; they only heard each other: was it therefore unnatural if they thought the German professors composed the German nation, as the French lawyers formed the French nation? And indeed public opinion in Germany was that of the professors. . . . The learned newspaper writers imagined the spirit of the age spake in them; no wonder that th.y overestimated the importance of this spirit and of this so-called public opinion" (ibid., p. 254). See also Treitschke's 'Deutsche Geschichte,' vol. v. p. 408, &c.

Elaborate claims to priority have thus been set up for persons to whom it is said the credit of modern discoveries should be given. I do not intend to contribute to this controversial literature, except by a general remark, which will explain how it has come to pass that ideas and principles now recognised as useful instruments of thought and research have only recently attained this importance, while they have frequently been the property of many ages of philosophical thought, and familiar even to the writers of antiquity. It is the scientific method, the exact statement, which was wanting, and which raises the vague guesses of the philosophical or the dreams of the poetic mind to the rank of definite canons of thought, capable of precise expression, of mathematical analysis, and of exact verification. Obscure notions of the attractive and repulsive forces of nature have floated before the minds of philosophers since the time of Empedocles, but they did not become useful to science till Galileo and Newton took the first step to measure the intensity of those forces. Lucretius's poem introduces to us the early speculations on the atomic constitution of matter, but the hypotheses of his school only led to real knowledge of the things of nature when Dalton, following Lavoisier and Richter, reduced this idea to definite numbers; still more so when, through the law of Avogadro and Ampère, and the calculations of Joule, Clausius, and Thomson, the velocities, the number, and sizes of atoms became calculable and measurable quantities. Descartes, and after him Malebranche, filled space with vortices which were to explain the constitution of matter and the movements of its parts; but the notion was abandoned and ridiculed till Helmholtz

and Thomson approached the subject with mathematical analysis and calculated the properties of vortex motion.

Heraclitus proclaimed, six hundred years before the Christian era, the theory that everything moves or flows; but not till this century was the attempt made to work out the definite hypothesis of Daniel Bernoulli, and to explain the properties of bodies, apparently at rest—the pressure of gases, or the phenomena of elasticity—by assuming a hidden motion of the imperceptible portions of matter. The same fate of lying dormant for ages attaches to the suggestive ideas of many thinkers.

9. Mathematical spirit.

In every case the awakening touch has been the mathematical spirit, the attempt to count, to measure, or to calculate. What to the poet or the seer may appear to be the very death of all his poetry and all his visions—the cold touch of the calculating mind,—this has proved to be the spell by which knowledge has been born, by which new sciences have been created, and hundreds of definite problems put before the minds and into the hands of diligent students. It is the geometrical figure, the dry algebraical formula, which transforms the vague reasoning of the philosopher into a tangible and manageable conception; which represents, though it does not fully describe, which corresponds to, though it does not explain, the things and processes of nature: this clothes the fruitful, but otherwise indefinite, ideas in such a form that the strict logical methods of thought can be applied, that the human mind can in its inner chamber evolve a train of reasoning the result of which corresponds to the phenomena of the outer world. By such processes did Gauss and Leverrier succeed in tracing the lines in the heavens on which invisible

stars were speeding through the universe; without them these objects of nature would probably never have been seen, and if seen, they would not have been recognised. Similar, and still more intricate, reasonings permitted Mendeléeff[1] to arrange in geometrical order the several elements or simple substances out of which matter is compounded, and to point to the vacant places on the chart, some of which have since been filled up by new discoveries. Thus it has also been shown that the ranges of temperature cannot be extended indefinitely in both directions—*viz.*, those of heat and cold—but that the latter possesses a zero point, representing the complete absence of motion.[2]

[1] The periodic arrangement of the elements, according to which, with increasing atomic or combining numbers, the same properties—such as density, fusibility, optical and electric qualities, and formation of oxides, &c.—recur in periods which are at least approximately fixed, so that they can be represented by curves, dates from the year 1869, when D. Mendeléeff and Lothar Meyer published almost simultaneously their classification of the elements. Newlands seems to have indicated some of these facts as early as 1864. Mendeléeff predicted the properties of a missing element, found to be those of scandium, which Nilson discovered ten years later. The same applies to the two other elements which were subsequently discovered by Lecocq de Boisbaudran (1878, gallium) and Winkler (1886, germanium), and in 1894 the newly discovered element argon was found to fill a vacant place in the plan.

[2] The zero point of temperature was originally a purely mathematical quantity suggested by the formula which gives the expansion of air in the air thermometer as dependent on the temperature. The ideal, not realisable, temperature at which, according to the formula, the volume of air would be nothing, was fixed by calculation at 459°·13 Fahr. or 272°·85 Centigrade. The real physical, not merely mathematical, meaning of the absolute scale of temperature with its zero point was only revealed when, through Carnot and Thomson, it was established that every degree of temperature has an assignable value for doing work, and when a scale of thermometry was suggested by Thomson (1848) in which every one degree had the same dynamical value, 100° in it corresponding to the 100° Centigrade in the air thermometer. It was then found that the two scales—that of the air thermometer and that measuring the dynamical value of temperature—agreed almost exactly. The number 273° Cent. thus acquired a physical meaning (see Clerk Maxwell, 'Heat,' 8th ed., pp. 49, 159, and 215). Another

By drawing curves on paper which correspond to the thermal properties of various substances, the conditions have been defined beforehand under which gaseous bodies like oxygen, hydrogen, nitrogen, or common atmospheric air can be reduced to liquid and solid bodies, upsetting the notions of the last generation, which looked upon these substances as permanent gases.[1] If the mathematical formula has killed, or failed to grasp, the true life of nature, that which to the poet and the philosopher will always be the feature of supremest interest, it has on the other side given birth to that new life of ideas which in our reasoning minds serve as the images of things

example of a purely mathematical quantity which, suggested originally by a formula, acquired later a physical meaning, is that of the potential function, used first by Lagrange as a simplification in calculating the forces of a disturbing planet, and termed by Laplace "à cause de son utilité, une véritable découverte" ('Méc. cél.,' v. livre xv. chap. i.) This function, which has the property that by a simple differentiation the component of the force in any direction is found, acquired a physical meaning as the quantity, the change of which measures the work required to move a unit of matter from one point to another (see Thomson and Tait, 'Natural Philosophy,' vol. i. 2, p. 29). Other examples of purely mathematical quantities which reveal physical properties are Hamilton's "characteristic function" (see Tait, "Mechanics," 'Ency. Brit.,' 9th ed., p. 749), Rankine's "Thermodynamic function," called by Clausius "Entropy" (see Maxwell, 'Heat,' pp. 162, 189): it measures the unavailable energy of a system.

[1] Thomas Andrews (1813-85) took up the experiments begun by Cagniard-Latour in 1822, and explained how it comes about that a gas remains incondensable however great the pressure may be, provided the temperature exceeds what he termed the "critical temperature," which is different for different gases. He accompanied his statements, which were first published in the 3rd edition of Miller's Chemical Physics, by curves representing the behaviour of atmospheric air and of carbonic acid, the latter being a condensable gas, and he suggested in 1872 that the so-called permanent gases had a critical point far below the lowest known temperatures, and that this was the reason why their liquefaction had not yet been achieved. Two physicists, Cailletet and Pictet, took up these suggestions; after various trials they succeeded independently in 1877 in liquefying several of the permanent gases, notably oxygen and nitrogen. These have been followed by all the other permanent gases, including atmospheric air, of which large quantities can now be prepared in a liquefied form.

natural, and allow us to make them subservient to our purposes.

10. When first introduced into science.

Whoever grasps the significance of the change which the exact or mathematical treatment of knowledge has worked in our life and thought, will readily place that name at the entrance of a history of modern thought, which is identified with a few simple mathematical formulæ, by which ever since his time the progress of science has been guided. Though belonging to an earlier period, the full meaning of Newton's work has only been recognised in the course of our century. In fact the Newtonian philosophy can be said to have governed at least one entire section of the scientific research of the first half of this period: only in the second half of the period have we succeeded in defining more clearly the direction in which Newton's views require to be extended or modified. Newton's greatest achievement was to combine the purely mechanical laws which Galileo and Huygens had established with the purely physical relations which Kepler—following Copernicus and Tycho—had discovered in the planetary motions, and to abstract in so doing the general formula of universal attraction or gravitation. Newton looked upon the motion of the moon round the earth, or the planets round the sun, as examples on a large scale of the motion of falling bodies—studied by Galileo—on the surface of the earth. Delayed in the publication of this simple rule of planetary motion through the absence of correct measurements, and through the necessity of inventing a new calculus by which the mathematical results of the formula could be ascertained, Newton did not publish his 'Principia' till 1687. The

11. Newton's 'Principia.' work, however, was conceived in the highest philosophic spirit, inasmuch as the enunciation of the so-called law of gravitation required the clear expression of the general laws of motion. In the first and second parts of the work the discoveries of Galileo and Huygens were absorbed, generalised, and restated in such terms as have up to our age been considered sufficient to form the basis for all purely mechanical reasoning.[1] In the latter part the new rule, corresponding to Kepler's empirical laws, is represented as the key to a system of the universe. The great outlines of this system are boldly drawn, and the working out of it is left as the great bequest of Newton to his successors. At the end of the eighteenth century,

[1] The most recent historian of the subject is Prof. Ernst Mach of Prague, whose 'Mechanik in ihrer Entwickelung, historisch-kritisch dargestellt,' 2nd ed., 1889, I cannot praise too highly. It has been translated into English by M'Cormack (Chicago and London, 1893). Referring to Newton, he says: "Newton has with regard to our subject two great merits. Firstly, he has greatly enlarged the horizon of mechanical physics through the discovery of universal gravitation. Further, he has also completed the enunciation of the principles of mechanics as we now accept them. After him an essentially new principle has not been established. What after him has been done in mechanics refers to the deductive, formal, and mathematical development of mechanics on the ground of Newton's principles" (p. 174). "Newton's principles are sufficient without the introduction of any new principle to clear up every mechanical problem which may present itself, be it one of statics or of dynamics. If difficulties present themselves, they are always only mathematical, formal, not fundamental" (p. 239). "All important mathematical expressions of modern mechanics were already found and used in the age of Galileo and Newton. The special names . . . have sometimes been fixed much later. Still later came the adoption of uniform measures, and this process is even yet incomplete" (p. 252). In this country it is one of the great merits of Thomson and Tait's 'Natural Philosophy' that they "restored" the teaching of mechanics and placed it on the original foundations afforded by Newton's laws of motion, in his own words, as "every attempt that has been made to supersede them has ended in utter failure" (Preface), and, though they "are only temporarily the best," there does not exist, "as yet, anything nearly as good" (Tait in article "Mechanics," 'Ency. Brit.,' 9th ed., p. 749).

after many able mathematicians and observers had generally investigated the numberless problems contained in the 'Principia,' Laplace published his 'Exposition du Système du Monde,' followed in the course of the first quarter of this century by the 'Mécanique céleste';[1] and at the close of the present century the most learned astronomer of the age could say that the 'Principia' still formed the sole foundation of all investigations in that domain.[2]

It is interesting to see how in a simple formula the mathematician is able to condense an almost immeasurable volume of thought, bringing the theory and the observations of past ages to a focus from which new lines of thought diverge in many directions. Every mathe- 12. The gravitation formula.

[1] The 'Exposition du Système du Monde' appeared, 1796, in 2 vols. 8vo: the first and second volume of the 'Mécanique céleste,' 1799, 4to; the third, 1802; the fourth, 1805; the last, 1825. Before publishing this work, which has been termed a second edition of the 'Principia,' Laplace had himself during thirty years assisted in dispelling the last doubts as to the sufficiency of the doctrine of universal gravitation to explain all cosmical phenomena; and he had especially brought the investigations of Clairaut, Euler, d'Alembert, Lambert, and Lagrange to a final result by publishing in successive memoirs between 1773 and 1786 the doctrine of "the stability of the system of the universe," based upon the invariability of the major axes and the periods of revolution of the planetary orbits. He and his predecessors also extended the solution of the problem "to find the orbit of two bodies, acting under the law of mutual gravitation," which was given by Newton in such a way that the action of one or more third (disturbing) bodies could be taken into account, dealing thus with the case of nature, which had in the first instance presented itself in treating of the complex motion of the moon. Laplace himself, who in numberless passages of his works recurs to the discoveries of Newton, announced the object of the 'Mécanique céleste' to be the treatment of astronomy "as a great problem of mechanics, from which it was important to banish as much as possible all empiricism," and to perfect it so as "to borrow from observation only the most indispensable data" ('Méc. cél.,' vol. i. introd.)

[2] The late Professor Rudolf Wolf of Zürich, whose 'Handbuch der Astronomie, ihrer Geschichte und Litteratur,' 2 vols., 1890-93, as well as his earlier 'Geschichte der Astronomie,' München, 1877, I warmly recommend.

matical formula which expresses the existing relations of natural things acts in a similar way, but probably few, if any, subsequent discoveries have given scientific minds so much fruitful work to do as the gravitation formula. An analysis of it will serve us as a guide through a very large portion of the scientific work of our period; it will serve also as an example of the great service which the mathematical mode of dealing with conceptions renders to the progress of science and of thought.

The so-called law of gravitation states that every two portions of matter, placed at a distance from each other, exert on each other an attractive force,[1] which depends on the masses of each, and on their distance from each other. The attractive force varies in the direct proportion of the mass of each, and in the inverse duplicate ratio of the distance. Three distinct lines of

[1] The gravitation formula gives no indication of the actual or absolute amount of the force in question; it only establishes a relation. It was fully three-quarters of a century after the publication of the 'Principia' that experiments were suggested in order to determine the actual magnitude of the force of gravitation—*i.e.*, the constant c in the formula $f=c\frac{m.m'}{r^2}$. Michell in 1768 devised an apparatus, employed later (1797) by Cavendish, and Maskelyne made measurements towards the end of the last century. More and more accurate determinations were made all through the present century, and latterly by Prof. Boys. Few persons have an idea of the extreme feebleness of the force, which nevertheless, through the magnitude of the earth, acquires in our daily experience such ormidable proportions. As it is desirable, in accordance with one of the principal scientific tendencies of our age, to place the knowledge of absolute physical quantities in the place of merely relative numbers, I mention here that the force with which two units of matter (*i.e.*, 2 grammes) placed at unit distance (*i.e.*, 1 centimetre) apart attract each other is such that they would approach each other with a velocity of nearly 7 hundred millionths of a centimetre in the first second of time. As a pound is a more familiar quantity, we may also say that two masses, each containing 415,000 tons of matter, and situated at a distance of one statute mile apart, will attract each other with the force of 1 lb. (see Sir R. S. Ball, 'Ency. Brit,' 9th ed., art. "Gravitation"). See also Sir R. S. Ball, 'The Story of the Heavens,' p. 106, and Prof. Boys in 'Nature,' vol. 50, p. 330, &c.

scientific research are involved and opened out by this statement. 13. Lines of thought emanating from it.

First, There is the purely theoretical task of defining clearly what is meant by the different words which are used, and which in the formula are expressed in algebraic symbols. What is the definition of force, what of mass, what of distance? The 'Principia' give Newton's definitions.[1]

Second, The definitions must be given in such a way that they express definite measurable quantities; and in order to verify and apply the formula, methods must be devised for measuring these quantities as they occur in nature, and these measurements must be actually carried out.[2]

[1] It will be readily admitted that the definition of force as measured by change of motion, and the definition of mass as the quantity of matter, are definitions involving some difficulty. As to distance, it may be thought that this is a purely mathematical, not a physical quantity. So it would be if physical bodies were mathematical points, such as the planets in a first approximation may be considered to be. But in comparing the attraction of the earth upon a body at its surface with that on the moon, the dimensions of the earth could not be neglected, and the problem presented itself how the quantities of mass and distance, in the case of the earth and the body on its surface, had to be defined. It appears from a statement by Prof. Glaisher (see Rouse Ball, 'History of Mathematics,' p. 297, &c.) that the publication of the 'Principia,' containing the gravitation formula, was delayed, because Newton found it difficult to prove that in a sphere the different parts with their different distances from any point need not be considered separately, but that a quantity equal to the whole mass situated at the centre of the sphere may be substituted. Laplace showed a century later that this property of the sphere exists only for one decreasing function of the distance—*viz.*, that of the inverse duplicate ratio. It exists likewise for that function which increases in proportion to the distance, but for none other (see 'Principia,' 1st ed., pp. 198, 200; 'Mécanique céleste,' 1st ed., vol. i. p. 143). Hitherto the delay in publishing the 'Principia' was (see Brewster, 'Life of Newton,' vol. i. p. 290) always attributed to the erroneous figure of the moon's distance from the earth, with which Newton had been reckoning, and which did not satisfy the gravitation formula.

[2] Up to the beginning of this century the merit of carrying out accurate measurements of astronomical constants is about equally divided between France and Eng-

Third, the formula is a mathematical expression, and, as such, can be subjected to purely mathematical analysis: this analysis may refer to purely algebraical processes of

land; the former country having supplied the means and organised many expeditions (under Richer, Picard, Cassini, La Condamine, Maupertuis, and others), the latter having invented and furnished the greater portion of the delicate instruments, through Newton, Gregory, Ramsden, Dollond, Harrison, and others. The latter was a matter of personal, the former one of organised, talent. England did not take any great part in the repeated measurements of the arc of the meridian till, towards the end of the eighteenth century (1785-87), the French astronomer Cassini de Thury presented to the Royal Society a memorial on the uncertainty in the difference of longitude of Greenwich and Paris, and proposed that the English and French mathematicians in concert should determine, by geodetic operations, the distance measured along an arc of parallel. This was assented to, and the late Astronomer Royal (G. B. Airy) claims that it "may be said that in this as in other grand experiments, though we began later than our Continental neighbours, we conducted our operations with a degree of accuracy of which, till that time, no one had dared to form an idea." Since the beginning of this century Germany has, through the accurate measurements of Gauss and Bessel, and through the famous establishments of Fraunhofer, Steinheil, Repsold, and others, taken a leading position both in the theory and practice of measuring. So far as gravitational astronomy is concerned, the United States of America seem at the end of this century to eclipse all previous performances. But if we owe to English genius the invention of logarithms, the sextant, the reflecting and the achromatic telescope, the theodolite, and the chronometer, we owe to France the idea of an absolute system of measurements and the first approximation to it in the metrical system, which England has been tardy to adopt. A really absolute unit of measurement, as the ten-millionth part of the earth quadrant was intended to be—one which would be recoverable, if every actually existing pattern was destroyed—does not yet indeed exist; but the Government of the Revolution laid the foundation in 1790 of our present international decimal centigrade system. It does not appear that the idea of extending this system to all other forces and quantities in nature was then contemplated. A valuable contribution towards this desirable object was made by Fourier, who in his celebrated 'Théorie de la Chaleur' (1822, p. 152, &c.) laid down the doctrine of the "dimensions" of physical quantities which had to be measured and compared with each other. The first who reduced the measurement of other than purely mechanical phenomena to the standard of mechanical forces was Gauss (1832). In his investigations referring to the intensity of magnetic force at different points of the earth, he found it necessary to abandon the unit of weight, the gramme, and to adopt the unit of mass, inasmuch as the weight of the unit of mass varied at different points of the globe. He introduced the name "absolute" to signify that this standard is independent of local or relative influences (see

calculation, or to geometrical figures. These geometrical figures represent on paper, and on a small scale, the curves or orbits of bodies in space and time, and can be interpreted as such. Then, as in nature two bodies or portions of matter are never single gravitating points occurring alone, but are surrounded by the totality of existing things, the formula which reduces the action of gravitation to that of pairs of things, and to the elements of matter, requires to be extended to more than two—in fact to an infinity of elements. The infinitesimal calculus teaches us how to deal with such a progression from finite numbers and quantities to infinite numbers; or from relations which refer to infinitesimal elements to finite measurable quantities. We find very soon that our powers of calculation reach only a small way, and cover only a small extent of the ground which observation opens to our eyes. We are thus forced to deal with the element of error 14. Element of error.
which creeps into our calculations; to be satisfied with approximations;[1] and instead of certainty, probability is

Gauss, Werke, vol. v. pp. 85, 293, &c.) Of Weber's electrodynamic measurements I shall speak later on. Absolute measurements were used by William Thomson (Lord Kelvin) as early as 1851, and owing mainly to his influence the present system was gradually established in the course of the following twenty years (see William Thomson, 'Popular Lectures and Addresses,' vol. i. p. 83, &c.) Fourier's theory of dimensions was first brought prominently before the scientific and teaching world by Clerk Maxwell in his treatise on 'Electricity and Magnetism' (1st ed., vol. i. p. 2). There also we meet for the first time with the use of astronomical magnitudes and relations by which the usual three units, time, mass, and distance, can be reduced to two. This is also lucidly explained by Lord Kelvin (*loc. cit.*) It has been followed up in detail in two interesting papers by W. Winter in Exner's 'Repertorium der Physik' (vol. 21, p. 775, and vol. 24, p. 471).

[1] The history of astronomical calculations since the time of Newton, when the theoretical basis was once for all laid, is a history of gradual approximations. Mathematically a conic section is sufficiently defined if the position of the focus (the sun in our planetary system) and three positions of the moving star are known by observation. But it was a long time before even tolerably complete methods of observation

the best we can attain to in our results.[1] An entirely new branch of investigation springs up—*viz.*, the theory of error, the doctrine of probability, and the investigation

and calculation were invented to deal practically with the problem. Up to 1781, when the new planet Uranus was discovered by Herschel, the interest centred mainly in the determination of the orbits of comets, which were assumed to be parabolic. Halley was the first to calculate these by means of tentative methods given by Newton in the 'Principia.' After 1781 the necessity arose of determining closed orbits, and a first attempt was made to do so by assuming circular orbits (neglecting the ellipticity) and neglecting the inclination of the plane of the orbit to that of the earth. But in the first year of this century neither the parabolic nor the circular figure of the orbits seemed to answer in the case of the new planet Ceres, nor could the inclination of the orbit be neglected. It required all the skill of Gauss to tackle the entire, unabbreviated problem, and this was done in his fundamental work 'Theoria motus corporum cœlestium.' As the 'Principia' form the foundation of all physical, so does the 'Theoria motus' of all calculating astronomy. A similar fundamental work which should take the next important step, solving generally the problem of the motion of a body which is attracted from more than one fixed or movable centre (the problem of three bodies), would mark the next great era in calculating astronomy. Hitherto this problem has only been treated under the assumption that the third attracting body disturbs the real orbit which has been calculated. The necessity of solving the problem of three bodies has made itself felt in the theory of the moon and other satellites, which stand under the influence of the main planet as well as the sun, and where therefore the ellipsis of Kepler cannot even be taken as a first approximation. And here again the necessity of taking into account the volume and the figures of the attracting bodies still further complicates the problem. On them depend the precession of the equinoxes and the irregularity of the precession known under the name of nutation.

[1] According to Wolf ('Handbuch der Astronomie,' vol. i. p. 128 *sqq.*) the merit of having first considered the best methods of dealing with errors of observation belongs to Picard (1670) and Roger Cotes ('Aestimatio errorum in mixta mathesi,' 1722). The former seems to have first used the apparently so obvious rule of taking the arithmetical mean of a number of observations, the latter introduced the notion of attributing to each observation its value or weight. Cotes accordingly found that the centre of gravity of a number of weighted points distributed over a plane coincided with the position of greatest probability. Gauss suspected that Tobias Mayer had already employed modern methods in his calculation of long series of observations, and he himself used what is termed after Legendre the "method of least squares" as early as 1795. It was not published till 1806 by Legendre, in his memoir 'Nouvelles méthodes pour la détermination des orbites des comètes.' Gauss published his methods in 1809 in the celebrated 'Theoria motus corporum cœlestium.' This method of finding the most probable result when a larger number of equations is given than unknown quantities

of the degree of approximation which we can attain to. And this does not only refer to the methods of calculation which we adopt,—is not only a consequence of the limits of our mathematical powers; this element of error attaches likewise to our actual observations, to the imperfection of our senses and of our instruments. The many sources of mistake and inaccuracy which surround us may either combine to produce an absolutely useless result, or may be adroitly adjusted so as very largely to destroy each other.[1] The arrangement of instruments of observation and calculation, so as to minimise our errors, is a special branch of science. Before the time of Newton few minds

is the same as that of finding the centre of gravity of a number of weighted points. This centre has the property that the sum of the squares of its distances from these points is a minimum. After the method had been introduced, Laplace and Gauss independently tried to prove it by a variety of considerations. These have not always been accepted as conclusive, though it is remarkable that very different ways of attacking the problem all lead to the same result, and that the rule is confirmed by actual trials on a large scale. It has been shown that the method of least squares in the case of a series of observations of one and the same quantity is equal to taking the arithmetical mean,—a process which recommends itself to common-sense, though it is not easy to prove it mathematically to be the best. On the whole, the calculus of probabilities and the so-called law of error are attempts to put into figures and mathematical formulæ a few common-sense notions, and it is interesting to see to what complicated processes of reasoning a combination of these simple notions may lead. The literature of the subject, belonging almost entirely to this century, is very large, Laplace and Gauss heading the list. Encke has summarised the scattered discussions of Gauss and Bessel in his memoir on the subject, reprinted in Taylor's 'Scientific Memoirs' and in the 2nd vol. of Encke's 'Abhandlungen,' Berlin, 1888. De Morgan, Airy, and Jevons ('Principles of Science,' vol. i.) in England have done much to popularise the subject, and Bertrand ('Calcul des Probabilités,' 1888) has very fully discussed the principles of the whole matter and shown up the weak points. The application of the calculus to statistics will occupy us in a future chapter.

[1] Not only has every instrument its constant errors, but even every observer himself has what is called a personal equation—*i.e.*, he is subject to constant errors of observation, dependent on the peculiarity of his sense organs, or his temperament, &c. This was hardly recognised at the beginning of this century, when Maskelyne, the Astronomer Royal, dismissed an assistant whose observations showed a constant difference from his own.

were occupied with the many researches indicated here. But as the contents of the 'Principia' became familiar and intelligible to men of science, a large army of workers, collected from all sides, had within the first century after its publication accumulated a great mass of research. It is the glory of the old French Academy of Sciences, in spite of the opposition to Newton that ruled there for some time, to have in all earnest taken up his great bequest, and to have made such a summary possible as was given by Laplace in the two works above referred to. To Laplace belongs also almost exclusively the merit of having recognised the importance which attaches in all human science to the existence of error, and of having founded the theory of probability. The element of error cannot be eliminated from our observations and our reasonings: the only true scientific method is to measure and study it.

15. Laplace and Newton.

The gravitation formula of Newton not only brought precision and definiteness into scientific work in the three directions mentioned above—it not only produced strict definitions of the fundamental notions of dynamics, promoted accurate measurements of physical quantities, and inaugurated a new literature in pure mathematics; but it had, as all other great generalisations have had since, a very far-reaching influence on scientific thought in other ways. There always have been, and always will be, several distinct interests which induce men to study nature. Some are driven to it by curiosity, or a pure love of nature. To those who belong to this class the end of the study of nature is to describe and to portray the objects which surround us, to see and know them

16. Several interests which promote science.

better. It would seem as if to such minds the scientific formula, the so-called law of nature, must be distasteful, and probably useless. Nevertheless the scientific view, of which the mathematical formula is an extreme expression, has reacted, though not always beneficially, upon the labours of those who confine themselves to observation and description; it has given to their efforts general interest and encouragement, indicated new directions, and frequently opened new fields. Thus the new formula of Copernicus and Galileo gave a great impetus to stargazing, which was greatly increased by the almost contemporary invention of the telescope. The new theory required the rotation of the planets, and led to minute observations of their phases, and to the discovery of the satellites of Jupiter and the ring of Saturn. Variable stars were incidentally discovered by Tycho, and the long-neglected comets received greater attention. Bernoulli attempted, and Halley actually carried out, the calculation of the return of a comet. Still later—in fact, not before the end of the eighteenth or the beginning of the present century—came the turn for reliable observation of meteors and auroras; for as late as 1790 the 'Décade philosophique,' as well as the Paris Academy and many learned persons, ridiculed the authentic reports of the fall of meteors, and Chladni's classical dissertation on the stone of Pallas.[1] It seems as if the purest love of

[1] When in the year 1790 the municipality of Juillac in Gascony submitted a report, signed by more than 300 eyewitnesses, to the Paris Academy, on a fall of stones which had there taken place, one of the editors of the 'Décade philosophique' remarked that it would be better to deny such incredible things than to enter into any explanations. Bertholon could not help pitying a community which had such a foolish *maire*, and remarked in the 'Journal des Sciences utiles': "How sad it is to find a whole municipality attesting formally by protocol popu-

17. Insufficiency of mere observation.

nature, the greatest devotion of the observer and the collector, lead only a little way in finding out the hidden paths of natural things or the behaviour of natural objects; and however grateful we must be to those pioneers of knowledge who with unrewarded patience amass the material for later theorists, it is to the classification of a Linnæus, to the arrangements of a Cuvier, to the theories of a Darwin, to the measurements of a Bradley and a Herschel, most of all to the formulæ of a Newton or a Gauss, followed by the calculations of their pupils, that we are indebted for a real grasp, for a comprehensive knowledge, of great masses of natural phenomena.

18. Practical interest.

Next to the pure love of nature, the desire to apply natural knowledge, and to make it useful for practical purposes, has rendered in return great services to science. The Royal Society and the Royal Institution had both from their infancy a large admixture of the practical spirit. These were founded, more even than the academies abroad, to a great extent upon the desire to make knowledge useful.

The Governments of England and of France promoted

lar fables which are only to be pitied! What can I add to such a protocol? The philosophical reader will himself suggest what to say when he reads this authentic proof of an evidently wrong fact, of a phenomenon which is physically impossible" (Wolf, 'Geschichte der Astronomie,' 1877, p. 697 *sq.*) Chladni published his essay on the large mass of iron found by the traveller Pallas in Siberia in the year 1794, and, in spite of adverse criticisms, followed it up by a catalogue and an atlas of meteoric stones, suggesting that they were of cosmic origin. Fortunately, a remarkable fall of stones, accompanied by meteoric phenomena, took place in 1803 not far from Paris, at l'Aigle in the department de l'Orne, and Biot was commissioned by the Academy to proceed to the district and examine the case. In the 'Relation,' &c., which he read before the Institute, he established the fact that a meteor exploded in the district, and that at the same time a fall of many thousand stones, weighing about 20 tons, took place (Biot, 'Mélanges scientifiques et littéraires,' vol. i. p. 15 *sqq.*)

the study of the "mechanics of the heavens" by offering large prizes for scientific and practical means of determining the longitude at sea. The lunar theory, which has occupied the attention of the greatest mathematicians since Newton—of Euler, Clairaut, and Tobias Mayer in the last century; of Burckhardt, Plana, and Hansen, of Delaunay and Adams, in the present century—was an outcome of this. It still engages the attention of scientific minds, involving as it does all the most delicate astronomical calculations, whilst for practical nautical purposes the moon has ceased to be the great timekeeper, and has since 1763 been replaced by the wonderful chronometers of Harrison and his successors. A similar stimulus both to abstract scientific research and to the perfection of the practical instruments of measurement was given in this century by the development of submarine telegraphy: in this case both sides of the problem, the scientific and the practical, were attacked, and carried to a high degree of perfection by one and the same mind[1]—

[1] William Thomson's (Lord Kelvin's) investigations and inventions, which made submarine telegraphy at long distances commercially practicable, refer mainly to the overcoming of the "embarrassment" occasioned by the property (discovered by Werner Siemens, 1849, and investigated by Faraday, 1854) which submerged cables possess of "retaining a quantity of electricity in charge along the whole surface." In 1854 Thomson made a full theoretical examination of this phenomenon, showed how it depended on the length, the electric resistance, and the electrostatic capacity of the line, and gave a mathematical formula, with practical examples of the retardation of the signals and the gradual increase of the strength of the electric current at the receiving end of long submarine cables ("On the Theory of the Electric Telegraph" and other papers, reprinted in the 2nd vol. of 'Math. and Phys. Papers,' 1884). The importance of constructing delicate instruments for registering feeble signals, and of a method for reducing the time of single signals, became evident through these theoretical investigations. The mirror galvanometer was first used in 1858 on the first Atlantic cable, and afterwards on the successful cables of 1865 and 1866. It was followed by the spark-recorder, which led to the syphon-recorder (1867-70), which

an almost unique instance of the combination of abstract reasoning and practical inventiveness. An almost equally important problem, having both scientific and practical interest, arising out of the Newtonian gravitation formula, is the problem of the tides. Here also the first suggestions towards a theory were given in the 'Principia,' whereas the first attempt at a solution is contained in Laplace's great work. A closer approximation was reached by Sir W. Thomson in his extensive theoretical and practical use of Fourier's mathematics.

I shall have frequent opportunity to refer to the beneficial and fructifying influence which practical problems have exerted on scientific thought;[1] in fact, in spite of

has since been in use in submarine telegraphy. The best account of these discoveries and inventions is to be found in Lord Kelvin's own papers, a good summary being given in his short article in Nichol's 'Cyclopedia,' reprinted as No. 82, vol. ii. p. 138.

[1] How much science owes to the practical interests of navigation can be seen by a glance at the subjects contained in the third volume of Lord Kelvin's 'Popular Lectures and Addresses.' The Tides, Deep-Sea Sounding, Cable-Laying, and Terrestrial Magnetism all furnish important practical as well as highly abstract theoretical problems, the solution of which demands new instruments and new methods of calculation. The phenomena of the tides and those of terrestrial magnetism are intimately connected with two of the most refined mathematical theories which this century has developed. The former was first attacked by the so-called equilibrium theory—the problem being to find the figure of equilibrium of a rotating ellipsoid covered with water under the influence of various attracting forces. Laplace, followed by Airy and Thomson, showed how it is much more a question of dynamics than of statics, and that it resolves itself into the analysis and subsequent synthesis of a number of periodic movements, dependent upon the several periodic changes of the rotation of the earth and the revolutions of the moon round the earth and the sun. A general method of dealing mathematically with the superposition of several periodic changes had been invented by Fourier in the early part of this century, and it was this which, especially in the hands of Lord Kelvin and his brother—the late Prof. James Thomson—led to the harmonic analysis of tide motion and the subsequent invention of tide-predicting apparatus (see the above volume, p. 177 *sqq.*) The observation of the magnetism of the earth is connected with great improvements in the theory and construction of the mariner's compass, suggested and carried out by

the great reciprocal influence which science has gained in the course of this century over practical life, I am still doubtful whether scientific thought has, at the end of our century, as yet balanced the debt which it owes to practical inventors. It is instructive, for instance, to consider how much, in the hands of Rumford, of Sadi Carnot, of Hirn, and of Rankine, science has learnt from the steam-engine, and to reflect whether from all the theoretical insight gained any really radical improvement of the steam-engine—still one of the most imperfect machines—has resulted.[1]

Lord Kelvin; and it has in another direction led to remarkable scientific results in the hands of Gauss, who between the years 1830 and 1840 brought the theory almost to perfection. Here again the physical phenomenon required for its treatment a special mathematical analysis, which Gauss greatly furthered in his 'Allgemeine Lehrsätze in Beziehung auf die im verkehrten Verhältnisse des Quadrats der Entfernung wirkenden Anziehungs- und Abstossungs-Kräfte' (1840). This is a mathematical investigation of the Newtonian gravitation-formula. Gauss followed out the theories of Laplace and Lagrange simultaneously with Green, whose now celebrated memoir on the subject remained long unknown (see *supra*, pp. 231, 247). The mathematical theory showed that in a sphere containing a certain amount of attracting (magnetic) matter an ideal distribution on the surface of the sphere can be found which takes the place of the real but unknown distribution in the interior, and that if through observation the necessary data are supplied, the magnetic condition of any point on the surface can be foretold with great approximation. As an example, Gauss foretold from the imperfect data at his command the position of the south magnetic pole. In 1840 Capt. Sir James Ross approached it sufficiently to show the correctness of the calculation. The theoretical investigations in connection with magnetic attraction and with tidal movements have remodelled the methods of observation of the phenomena themselves, the older methods having proved to be in many ways insufficient. A full account of Gauss's labours here referred to will be found in E. Schering, 'C. F. Gauss und die Erforschung des Erdmagnetismus,' Göttingen, 1887.

[1] I refer in this matter to two addresses delivered recently—one by Prof. Unwin ('Electrician,' vol. 35, pp. 50 and 79) on "The Development of the Experimental Study of Heat-Engines"; the other by Prof. Lodge on "The Second Law of Thermodynamics" ('Electrician,' vol. 35, p. 80 *sqq.*) From a perusal of these papers one gains the impression that science has been more successful in teaching us why the steam-engine is so wasteful a machine than in showing how it can be greatly improved. It is interesting to hear that "al-

19. Focalising effect of mathematical formulæ.

The mathematical formula is the point through which all the light gained by science passes in order to be of use to practice; it is also the point in which all knowledge gained by practice, experiment, and observation must be concentrated before it can be scientifically grasped. The more distinct and marked the point, the more concentrated will be the light coming from it, the more unmistakable the insight conveyed. All scientific thought, from the simple gravitation formula of Newton, through the more complicated formulæ of physics and of chemistry, the vaguer so-called laws of organic and animated nature, down to the uncertain statements of psychology and the data of our social and historical knowledge, alike partakes of this characteristic, that it is an attempt to gather up the scattered rays of light, the diffused knowledge, in a focus, from whence it can be again spread out and analysed, according to the abstract processes of the thinking mind. But only where this can be done with mathematical precision and accuracy is the image sharp and well defined, and the deductions clear and unmistakable. As we descend from the mechanical, through the physical, chemical, and biological, to the mental, moral, and social sciences, the process of focalisation becomes less and less perfect,—the sharp point, the

most all the present difference between the best steam-engine and the worst is some 5 or 6 per cent" (Lodge). Prof. Unwin sums up by saying: "Since 1845 purely scientific men, scientific experimenters, and practical engineers have all been engaged in the study of the steam-engine. I do not believe that any one of the three can claim all the credit for the improvement of the steam-engine to the exclusion of either of the others. . . . Representing perhaps rather the scientific than the practical interest, I do not think that the mathematical and physical researches of which I have tried to give an account have had no influence on the practical business of the engineer."

focus, is replaced by a larger or a smaller circle, the contours of the image become less and less distinct, and with the possible light which we gain there is mingled much darkness, the source of many mistakes and errors. But the tendency of all scientific thought is towards clearer and clearer definition; it lies in the direction of a more and more extensive use of mathematical measurements, of mathematical formulæ.

There is probably no science which has come so perfectly under the control of this kind of mathematical expression as has astronomy since the time of Newton or of Laplace, and, we may add, there exists probably no mathematical formula which has stood the test of application to existing phenomena so long and so thoroughly as the gravitation formula of Newton. It possesses two unique properties which no other formula possesses—so far as we can now see—it is universal[1] and it is accurate.[2] These

[1] The law of gravitation can be called the first and most general physical law or statement of universal application. The laws of motion may be called mechanical or dynamical statements. Both the law of gravitation and the laws of motion describe facts, and have been found by experience; but the laws of motion contain no physical constant—*i.e.*, no quantity which requires to be fixed and measured by observation, and the absolute value of which has for us at present no ulterior meaning. The law of gravitation has one physical constant, the universal gravitation constant (see p. 320). As it measures what we call matter, it need not be determined, and its actual determination, which has been accurately made only in recent times, has not in any direction advanced our general physical knowledge. For all practical purposes of physics the unit of mass is a weight, just as for all commercial purposes gold is the standard of value. The astronomical view permits us to go a step further and express the mass of a pound of matter in units of time and space, and the political economist may seek for a real standard of value—for instance, an article of food like wheat. Other fundamental physical laws or general statements involve other physical constants, as we shall see later on.

[2] The accuracy of the so-called laws of nature, or, more correctly, of the expressions which science gives to the laws of nature, is a very important question. Little is said on this point in the ordinary text-books. It is only in very

two properties of the gravitation formula have been brought out by a long line of investigations, carried on with the view of substantiating or of refuting the formula. They mark the development of whole sciences, the foundation of quite novel branches of research. I propose briefly to follow up these developments.

20. Matter and force mathematically defined.

Common-sense has never had any difficulty in knowing what matter and force are, or in defining them for the purposes of practical life. But it took thousands of years to find a definition of these quantities which could serve as the basis of exact measurement, and permit calculations of results into which both factors entered in varying

recent publications that attention is sufficiently drawn to the fact that very few mathematical formulæ in physics or chemistry are more than approximations. The law of gravitation is one of the few mathematical expressions which, besides being universal, have stood the most rigorous tests as to accuracy. A most interesting attempt to prove the inaccuracy of Newton's law was made, but speedily abandoned, by Clairaut, one of the earliest Newtonians in the old Academy of Sciences. Clairaut began about 1743 to study the lunar theory in the light of Newton's system, which Madrin before him had already despaired of reconciling with the facts of observation. When he himself, on calculating the annual motion of the moon's apogee (or farthest point in its orbit round the earth), found only half the value which observation furnished, he was tempted in his communication to the Academy of November 1747 to suggest that the Newtonian formula might require a correction for great distances. This suggestion was followed, as Lalande tells us, by a veritable scandal in the learned world. Buffon, for purely metaphysical reasons, objected to this infringement of the simplicity of the laws of the universe. The opponents of Newton's system had a short triumph, which however was speedily reversed when Clairaut, putting a greater precision into his calculations by taking inequalities into account which he had previously neglected, explained to the Academy in May 1749 that he had succeeded in reconciling the movement of the moon's apogee with the law of attraction according to the inverse square of the distance. From that time the Newtonian theory, to which only shortly before mathematicians like Euler had been won over, reigned supreme. See Lalande in the 4th volume of Montucla's 'Histoire des Mathématiques,' p. 67, &c. Euler's merits in solving many problems in physical astronomy were so great that the Academy procured permission from Louis XV. to receive him as a *surnuméraire*, the eight places granted to external members being all occupied.

quantities and in varying combinations. That a smaller quantity of matter in motion could produce the same action as a larger which was moving slowly, or even apparently at rest, and acted only by what is termed its dead-weight, was a well-known phenomenon; but it was only within the half-century which preceded the publication of the 'Principia' that, through the labours of Galileo and of Huygens, mathematical definitions and simple formulæ were laid down, and generally accepted, which gave the means of accurately measuring and calculating the phenomena of moving bodies and the combination of forces. These labours resulted in a definition of matter which, translated into the language of our day, says that matter is that which moves and is capable of resisting any change of motion. Motion is a measurable quantity. For its measurement we require the measurement of space and time, and the well-known relation of both—*viz.*, velocity.

The above formula therefore says that matter is measured by the resistance it offers to change of motion or of velocity. And correspondingly force is that which is capable of producing change of motion, or velocity in matter, and it is measured by the amount of change it produces. Given a definite, though unknown, force, portions of matter—*i.e.*, masses—can be compared by the resistance they offer to the change of their motion; the smaller the change the larger the mass or quantity of matter. Given a definite, though unknown, quantity of matter, forces can be measured by the different changes they produce in the motion—*i.e.*, the velocity—of this quantity; they are greater or smaller in the proportion

of the change of velocity which they produce. One of the great difficulties which stood in the way of the fixing of these very simple mathematical relations and definitions was the fact that all matter with which we can experiment is under the influence of a constant but unknown force, that which makes it fall if not supported. It was only by freeing themselves from the effect of this constant force, or by balancing it, that philosophers gradually arrived at the conception and definition of mass, or quantity of matter, as something independent of its weight. It was reserved for Newton to show and define the exact relation which weight bears to the other properties of matter defined and measured by his predecessors. By doing so he added a new definition, a new means of measuring the quantity of matter or its mass, showing at the same time to what extent the popular measure of matter—*i.e.*, its weight—could be accurately used for scientific purposes. Again, to express it in the language of our day, Newton showed that matter is not only that which offers resistance to change of motion, but also that which causes change of motion in other portions of matter: it is not only the object on which force spends itself, it is the seat of this force, and the degree in which it can change motion in other portions of matter is proportional to the degree in which it resists the change of its own motion—in other words, the gravity or weight of matter is proportional to its mass or inertia, and is not dependent on any other difference, whether of size or of quality. This second universal property of matter, which brought out more clearly the reciprocity of all mechanical, and subsequently of all

21. Weight and mass.

physical actions, is, however, dependent on the mutual distances of the particles of matter, and can therefore be altered, but can as little as the existence of matter itself be removed. This view of Newton's explained or described clearly[1] the phenomena of moving and falling

[1] The distinction between an explanation and a description of the facts of nature has been slowly developed in the course of modern thought. Probably Leibniz was the first to insist on it, and to maintain in the abstract that all description of nature would be mechanical, but that the explanation or interpretation of nature must be spiritual. But the first practical instance of this important distinction is really to be found in Newton's philosophy. In many passages of the 'Principia,' and especially in the 'Optics,' the double view of the problems of philosophy is clearly indicated. The principles of science since the time of Newton are general facts, established by experience and put into mathematical language, admitting of constant verification by observation and by the deductions of the calculus. These principles are not the ultimate causes, but only a concise description of some of the phenomena of nature. These principles Newton calls mathematical—referring to measurable quantities—and distinguishes them from the philosophical principles ('Princ.,' 1st ed., p. 401). Especially as regards gravitation, Newton explains many times that he uses this term not as an explanation, but only as a mathematical description of the force with which bodies approach each other, whatever the cause of this phenomenon may be, which he leaves others (called with some irony metaphysicians) to determine ('Optics,' query 31). That Newton, besides giving the precise mathematical principles of all future dynamical science, indulged also in further speculations, which he put into the form of queries and advanced with hesitation and merely tentatively, gave his opponents ample opportunity to attack the doubtful and uncertain statements in his philosophy. Instead of studying and understanding the mathematical truths of the 'Principia,' they attacked the doctrines which were fragmentarily put forward in the queries to the 'Optics' or added in the general scholium at the end of the second edition of the 'Principia.' Roger Cotes in his preface to the second edition of the 'Principia,' and Clarke in his correspondence with Leibniz, pointed out the difference between Newton's descriptive and calculating and the older or metaphysical philosophy. They were, however, more interested in disproving the atheistical consequences of which Newton's philosophy had been accused than in clearly insisting on the fundamental difference between mathematical and metaphysical principles—*i.e.*, between the exact and the philosophical views of nature. And in Bentley's Boyle lectures, delivered in 1692 and 1693, the principles of Newton's philosophy were specially brought forward to refute atheism, an undertaking which Newton himself supported in his contemporary correspondence with Bentley, published half a century later, in 1756.

masses, not only at a point on the surface of our earth, where the force of gravity can be considered to be constant, but all through the universe, where it varies with the distances of the moving masses.

22. Gravitation not an ultimate property of matter.

The Newtonian formula of gravitation was not at once accepted by philosophers as a correct statement of the facts of nature.[1] It appeared to limit the existence of

[1] The philosophy of Descartes, which then reigned on the Continent, seemed in many ways to hinder the acceptance of Newton's doctrines. Descartes had taken a great step in advance in philosophical teaching; he had placed mathematics at the head of his doctrine; he had opposed the older metaphysical methods, and he had, through his application of algebra to geometry, made great progress towards a mechanical description of phenomena. But he had not separated the description from the interpretation of nature. Philosophy and science remained united, the mathematical formulæ were only a new kind of metaphysics, incapable without observation of making any real advance in the knowledge of nature. The facts of geometry which are required for an application of analysis are the well-known axioms of Euclid. An application of analysis to dynamics requires a knowledge of the laws or fundamental properties of motion. These were not correctly and completely known to Descartes; Newton placed them at the head of his mathematical philosophy of nature. A further application to physical phenomena required a knowledge of some general physical fact: such was supplied by Newton in the gravitation formula. The laws of motion and gravitation once admitted as facts, there was plenty to do for mathematics. Not so with Descartes. In his philosophy the basis of facts was too narrow and indefinite, and had to be supplemented by metaphysical suppositions and deductions. The field for mathematical reasoning not being sufficiently prepared and wide enough, Descartes had speedily got back again into metaphysical reasoning. In fact the doctrines of Newton, in which mathematical and philosophical deductions had for the first time been successfully separated, encountered on the Continent the doctrines of Descartes, in which mathematical and philosophical deductions were hopelessly mixed up. On one point especially the two views seemed to clash. Descartes had by metaphysical considerations tried to define what matter is. Newton had postponed the answer to this question, but had defined mathematically two properties of matter—*viz.*, inertia and gravitation. Descartes' metaphysical considerations had led to the conception that matter and extension were identical, that space therefore could not be empty. Newton, occupying himself not with matter in the abstract, but only with moving observable matter, had established the general law of gravitation, leaving it undecided whether the apparent vacuum existing between visible bodies was really empty or full. For the deductions from the law of gravitation it might in the first instance be considered empty. Thus on this question about space

matter to certain changing places in an empty space, and to attach the forces of nature likewise to this distribution of matter. This was hardly the intention of the author himself, who saw in the so-called law of gravitation not a final explanation, but only a description of the phenomena of nature—notably of the larger phenomena. That behind the mathematical formula there may be conditions which are capable of further analysis,—that the larger or molar phenomena of moving bodies are made up of their smaller or molecular movements, was well known to Newton. For before he approached the great laws of the universe he had been occupied with investigations which led him into the minutest phenomena, those of light and colour. To him, indeed, are owing some of the observations and methods by which subsequently the greatest and the smallest measurements of natural objects have been carried out. But in exact science the deeper philosophical meanings disappear where the strict mathematical deductions point to definite conceptions, mark certain fixed paths of research, and promise definite results. The eighteenth century gradually settled down to a wholesale adoption of the gravitation theory—looked upon space as empty, upon matter as subject to a definite though changing distribution in space, and upon the forces of nature as attached to certain moving centres, between which only a mathematical, but no intelligible physical, connection

—whether it was empty or full—the two doctrines came into conflict. That Newton's position was not a final, but only a provisional one, was overlooked; he was accused of introducing again the occult qualities of the scholastic philosophy, and a great fight was started against his views in the Academy of Sciences, where Descartes' philosophy reigned supreme.

could be traced.[1] What to some contemporaries of Newton, and even to Newton himself, seemed an absurdity—that action could take place at a distance [2]—became through

[1] Voltaire, who did not dive very deep into the teachings of Newton, gives a graphic description of the different opinions then current in English and French learned circles. In his 'Lettres sur les Anglais,' written about the time of the death of Newton, after having discoursed on Quakerism, the Church and Government, on vaccination, Bacon and Locke, he devotes four chapters to the philosophy of Newton, which he contrasts with that of Descartes. "Un Français qui arrive à Londres trouve les choses bien changées en philosophie, comme dans tout le reste. Il a laissé le monde plein, il le trouve vide. Paris on voit l'univers composé de tourbillons de matière subtile, à Londres on ne voit rien de cela. Chez nous c'est la pression de la lune qui cause le flux de la mer; chez les Anglais c'est la mer qui gravite vers la lune. . . . Chez vos Cartésiens tout se fait par une impulsion qu'on ne comprend guère; chez M. Newton c'est par une attraction dont on ne connait pas mieux la cause. . . . Descartes assure encore que l'étendue seule fait la matière, Newton y ajoute la solidité" (lettre xiv.)

[2] "You sometimes speak of gravity as essential and inherent to matter. Pray, do not ascribe that notion to me; for the cause of gravity is what I do not pretend to know" (Newton's 2nd letter to Bentley, 17th January 1692-93). "It is inconceivable that inanimate brute matter should, without the mediation of something else, which is not material, operate upon and affect other matter without mutual contact, as it must be, if gravitation, in the sense of Epicurus, be essential and inherent in it. And this is one reason why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers" (3rd letter to Bentley, 5th February 1692-93). And in the fifth answer to Leibniz (published after Leibniz's death) Clarke says: "That the sun attracts the earth . . . —that is, that the earth and sun *gravitate* towards each other, or tend towards each other, with a force which is in a direct proportion of their masses, . . . and in an inverse duplicate proportion of their distances, and that the space betwixt them is void—that is, has nothing in it which sensibly resists the motion of bodies passing transversely through: all this is nothing but a phenomenon or actual matter of fact, found by experience. That this phenomenon is not produced *sans moyen*—that is, without some cause capable of producing such an effect—is undoubtedly true. Philosophers therefore may search after and discover that cause, if they can; be it mechanical or not mechanical. . . . The phenomenon itself, the attraction, gravitation, or tendency of bodies towards each other, and the laws or proportions

a century of confirming thought, observation, and calculation an adopted axiom, and the accepted formula of all physical explanations. For a time, indeed, the exact formula of gravitation seemed liable to some correction, but gradually the apparent anomalies disappeared, and even in our century none of the many attempts to modify the gravitation formula, to look upon it as merely an approximation, or to go behind it and find some more general relation from which it could be deduced, have been generally useful or acceptable.[1] It still stands there as the only universally accepted mathematical expression which corresponds to a general physical property of natural objects.

Two different lines of thought combined to give the formula of Newton a still wider importance than its author primarily intended, or than it has been found possible to maintain in the course of further inquiry. The first was the ancient philosophical idea of attraction, which, without being mathematically defined and practically useful, had nevertheless, from the dawn of Greek speculation

of that tendency, are now sufficiently known by observations and experiments. If this or any other learned author can by the laws of mechanism explain these phenomena, he will not only not be contradicted, but will, moreover, have the abundant thanks of the learned world. But in the meantime, to compare gravitation, which is a phenomenon or actual matter of fact, with Epicurus' declination of atoms seems to be a very extraordinary method of reasoning" (§§ 118-124, Leibniz's 'Philosophische Schriften,' by Gerhardt, Berlin, 1890, vol. vii. p. 439 *sq.*)

[1] A very complete account of these different attempts will be found in the writings of C. Isenkrahe, 'Das Räthsel von der Schwerkraft,' Braunschweig, 1879; "Euler's Theorie von der Ursache der Gravitation," in 'Zeitschrift für Mathematik und Physik,' vol. xxvi.; 'Ueber die Fernkraft,' Leipzig, 1889; "Ueber die Zurückführung der Schwere auf Absorption," in 'Abhandlungen zur Geschichte der Mathematik,' vol. vi., Leipzig, Teubner, 1892. See also as bearing on this subject, Paul du Bois-Reymond, 'Ueber die Grundlagen der Erkenntniss in den exacten Wissenschaften,' Tübingen, 1890.

and all through ancient and mediæval philosophy, figured as one of the occult causes or forces which regulate the behaviour of living and dead matter. That the force of attraction alone would result in an accumulation of all matter in one body was of course recognised, and a second arbitrary and occult force—that of repulsion—was introduced as a counteracting or balancing agent.

23. Attraction and repulsion.

In Newton's system of the universe the balancing force was found to be that of an inherent initial motion which matter, in consequence of its mass or inertia, maintained in addition to the motion due to gravitation. If motion and inertia were able to account for the apparent repulsion of bodies at a distance, it might be that they could also account for their apparent attraction. This idea, though expressed about the time when the Newtonian gravitation formula was established, did not meet with serious attention till far on in our century other lines of thought led to similar views.[1] The phenomena of attrac-

[1] Newton himself seems to have looked for a mechanical explanation of gravitation. Long before the publication of the 'Principia' he laid before the Royal Society a paper containing "a hypothesis explaining the properties of light" by the assumption of an "ætherial medium, much of the same constitution with air, but far rarer, subtiler, and more strongly elastic" (Letter to Oldenburg, January 25, 1675-76, given in Brewster's 'Memoirs of Sir I. Newton,' vol. i. p. 390 *sqq.*), which might explain magnetic and electric phenomena, as well as those of gravitation, and especially light. And in a letter to Robert Boyle, of 28th February 1678-79 (Brewster, vol. i. p. 409), he reverts to this subject. Having, however, in the course of the next decade found it more useful to work out the mathematical conclusions to be drawn from the phenomenon of gravitation, which was a fact and not a hypothesis, he abandoned the metaphysical part of the subject, the question how gravitation was to be explained, "finding" (as Maclaurin says in his account of Newton's discoveries) "that he was not able, from experiment and observation, to give a satisfactory account of this medium and the manner of its operation in producing the chief phenomena of nature." And in his letter to Boyle, as well as in a later one to Halley (20th June 1886, Brewster, vol. i. p. 439), he carefully distinguishes between the results of the 'Principia' and

tion and repulsion at a distance rather received additional weight and importance when, following Newton's cosmical measurements, Cavendish and Coulomb, towards the end

the mere framing of hypotheses and conjectures, for which he professes to have little fancy, though "the heads of some great virtuosos run much upon hypotheses"; and he describes his earlier speculations as "guesses which I did not rely on." In fact, the elaboration of the theorems contained in the 'Principia' marks the transition from the metaphysical to the exact or scientific treatment of natural phenomena. Before Newton showed the far-reaching consequences, the unexpected grasp of a simple mathematical formula in combining facts apparently disconnected, no one could have suspected that such would be possible, and it is not to be wondered at that when once philosophers realised the power of such formulæ, an opposite movement set in through which mathematical processes were extolled at the expense of experiment and observation on the one side, and of philosophical reasoning on the other. Newton himself never fell into this error. He knew well the importance of observation, and he retained to the end of his life a great interest in the philosophical or metaphysical problems which lay beyond or behind the mathematical statement; he carefully distinguished between the *vis gravitatis* and the *causa gravitatis*. Two other great thinkers, second only to Newton himself, took up a similar position to the law of gravitation. Whilst firmly believing in it, they considered it to be not an ultimate law of nature, a *causa occulta*, but believed that it must be possible to derive it from some mechanical properties of matter. The one was older than Newton. It was Huygens (1629-95) who through his analysis of centrifugal forces (1673) had done so much to pave the way for Newton's own work. In 1690, after having paid a visit to England in order to become more intimately acquainted with Newton's work, he published at Leyden his 'Discours sur la Cause de la Pesanteur,' a treatise which was little noticed at the time, and in which he is supposed to have revived the vortices of Descartes. Those who have carefully examined it (Fritsch, 'Theorie der Newton'schen Gravitation,' &c., Königsberg, 1874; and Isenkrahe, 'Das Räthsel von der Schwerkraft,' p. 87, &c.), find that Huygens reverted to his conception of a material fluid, an ether, such as he had suggested for the explanation of optical phenomena, "which surrounds the earth up to very great distances, which consists of the minutest particles, which fly about in the most different ways in all directions with tearing velocity"—an anticipation surely of Lesage's "ultramundane corpuscles." The other great thinker who, whilst firmly believing in Newton's law, sought for a mechanical explanation of it, was Leonhard Euler (1707-83). In his ether theory, to which he reverts frequently, he made an attempt to explain the various physical agencies, among them gravitation (1743, in his 'Dissertatio de Magnete,' which received in 1744 the prize offered by the Paris Academy), by the pressure of the ether. He admits the difficulty of the problem, but insists upon the necessity of finding a mechanical cause for gravitation. See Isenkrahe in 'Zeitschrift für Mathematik und Physik,' vol. xxvi.; but

24. Electrical and magnetic action.

of the last century, subjected the less universal terrestrial phenomena of magnetic and electric action to exact measurements, finding that a formula corresponding to the gravitation formula described them with surprising accuracy, with this remarkable difference, that here not only attractive but also repulsive forces, following the same mathematical relations as to mass and distance, came into play. To these confirmatory discoveries must be added the measurement of the intensity of radiations which proceed from centres, such as those of light and heat, made by various philosophers during the latter half of the last century. Newton, and his great successor Laplace more than a century after him, both favoured the emission or emanation hypothesis of light, and it was thus natural to fasten upon the analogy which existed between the intensity in which radiation, gravitation, and electric and magnetic action change with the distance from their respective centres. All these agencies came thus under the general conception of forces emanating from fixed centres, and spreading through space, in the proportion of the superficial area of the spheres described around their centres with increasing radii—*i.e.*, decreasing or becoming diluted in the ratio of the squares of these radii or distances. These analogies were indeed recognised to be very imperfect, inasmuch as light and radiant heat occupy a measurable time to spread from their centres, whereas the time occupied by the force of gravitation is

25. Law of emanations.

especially Miething, 'L. Euler's Lehre vom Aether,' Berlin, 1894. In the course of this century the mechanical theory of gravitation, including the attempts of Lesage, Euler, Huygens, and Newton himself, has again received attention through Faraday's, Maxwell's, and Hertz's electric theories, and Wm. Thomson (Lord Kelvin) has especially studied the ideas of Lesage. Of this more later on.

either exceedingly small or this force is propagated instantaneously through the greatest cosmical distances which come under our notice. Then, again, light and radiated heat spend themselves as they meet with reflecting or absorbing bodies, whereas gravitation does not seem to be affected by intervening or screening bodies.[1]

[1] It is now known that this screening effect exists likewise in magnetic and electric action. In the formula which expresses the action at a distance of magnetic, electrical, and ponderable masses, *viz.*, $f=\mu \frac{m.m'}{r^2}$, the older view—previous to Faraday's researches—considered m and m' the masses (ponderable or imponderable), and the distance r to be variable, μ a constant, corresponding to the gravitation constant. As stated above, the gravitation constant is, so far as we know, a real constant—*i.e.*, it is not affected by the nature of the medium which fills the space intervening between m and m', the attractive masses. Faraday doubted this; but leaving gravitation—"as a relation by some higher quality"—aside, he directed his efforts to the testing of the validity of this view as regards electric and magnetic action. He found that μ is not a real constant, but dependent on the nature of the medium and the objects which intervene between the magnetic and electric masses. These researches, which are probably the first step in the direction of gaining by observation some notion of the mechanical manner in which action at a distance is brought about, begin with the year 1837 (see 11th series of 'Experimental Researches in Electricity,' No. 1252). The result was that the "specific electric induction for different bodies" was established, contrary to the ideas of Poisson and others ('Exper. Res.,' No. 1167), and the word "dielectric" invented to denote the "action of the contiguous particles of the insulating medium" (No. 1168). From this point he was led a step farther, to "expect that all polar forces act in the same general manner"—*viz.*, by contiguous particles. Faraday, however, is careful to remark that by contiguous particles he means those "which are next to each other, not that there is *no* space between them" (No. 1665).

In 1838 Faraday was still doubtful whether magnetic action was similar in this respect to statical electric action; but he thought it probable that it was "communicated by the action of the intervening particles" (No. 1729), and in pursuing this line of thought, in spite of many unsuccessful trials, he at last saw his ideas realised, discovered the magnetisation of light, and invented the term "diamagnetic" to describe "a body through which lines of magnetic force are passing, and which does not by their action assume the usual magnetic state" (1845, 'Exper. Res.,' No. 2149). At the end of the 19th series of researches he says: "In former papers (1838) I proposed a theory of electrical induction founded on the action of contiguous particles, . . . and I then ventured to suggest that probably . . . magnetic action was also conveyed onward in a similar manner. At that time I could discover

Nevertheless, the fact that gravity, radiation, and electric and magnetic action appear as central emanations, decreasing with the square of the distance,—two properties which lend themselves to mathematical and geometrical representation,—seemed to pave the way for further generalisations. All forces in nature were put down as central forces, either attractive or repulsive, and if not following the Newtonian formula, still dependent on the distance according to some mathematical expression. For nearly a century theoretical physics were occupied in working out the mathematical formulæ expressive of these ideas, and Laplace himself promoted these attempts by the weight of his great authority. We do not possess the final views on this point with which the great mathematician intended to complete the last edition of his 'Exposition du Système du Monde'; but some of the later chapters of

26. Molecular action.

this work, treating of gravitation and molecular attraction, show us clearly in which direction he looked for progress in theoretical physics.[1]

no peculiar condition of the intervening or diamagnetic matter; but now that we are able to distinguish such an action; . . . now that diamagnetics are shown not to be indifferent bodies, I feel still more confidence in . . . asking whether it may not be by the action of the contiguous or next succeeding particles that the magnetic force is carried onward," &c. (No. 2443). Faraday also made repeated experiments with the view of determining how the force of gravitation is communicated, believing as little as Newton did in an *actio in distans*, and he was wont to quote Newton's words on this matter, referring also to Euler's ether theory (No. 3305).

[1] In the fifth edition of the 'Exposition du Système du Monde' Laplace had suppressed these chapters, and had announced his intention "to unite the principal results of the application of analysis to phenomena depending on a molecular action differing from universal attraction" into a special treatise which should form a sequel to the 'Exposition,' &c. This project was never carried out (see "avertissement au sixième édition de 'l'Exposition'"). The success which attended Laplace's attempts to explain double refraction and aberration of light (following Newton's suggestions in the 'Principia' and 'Optics') as well as capillary phenomena (following Haukesbee) left no

The great prominence given by Laplace to the gravitational explanation of all natural phenomena, the fact that all the observable movements of the universe, the shape and size of the moving masses, and the orbits they describe, as well as many phenomena observable on the surface of our globe, such as the aberration and refraction of light, the phenomena of the tides, of atmospheric pressure, and some of the more important molecular properties of matter, could be perfectly or approximately described, calculated, and predicted by gravitation or analogous attractions, gave to what we may call—following a hint of Clerk Maxwell's—the astronomical method[1] of con-

doubt in his mind that such phenomena "are owing to attractive and repulsive forces between molecule and molecule" ('Expos.,' 6^{me} éd., p. 328). He saw in molecular attraction the cause of the solidity of bodies, of chemical affinities, and of the properties of chemical saturation, which Berthollet had developed about that time ('Expos.,' p. 360); he thinks it likely that the law of molecular attraction is the same for all bodies, and he finally dwells on the question whether the attraction of gravity and molecular attraction could be united under one common law or expression (p. 363), and throws out the idea that thus the phenomena of physics and astronomy might be brought under one general law, adding, however, significantly, "Mais l'impossibilité de connaître les figures des molécules et leurs distances mutuelles, rend ces explications vagues et inutiles à l'avancement des sciences."

[1] "Cavendish, Coulomb, and Poisson, the founders of the exact sciences of electricity and magnetism, paid no regard to those old notions of 'magnetic effluvia' and 'electric atmospheres' which had been put forth in the previous century, but turned their undivided attention to the determination of the law of force, according to which electrified and magnetised bodies attract or repel each other. In this way the true laws of these actions were discovered, and this was done by men who never doubted that the action took place at a distance, without the intervention of any medium, and who would have regarded the discovery of such a medium as complicating rather than as explaining the undoubted phenomena of attraction. . . . Ampère, by a combination of mathematical skill with experimental ingenuity, first proved that two electric currents act on one another, and then analysed this action into the resultant of a system of push-and-pull forces between the elementary parts of these currents. . . . Whereas the general course of scientific method then consisted in the application of the ideas of mathematics and astronomy to each new investigation in turn, Faraday seems to have had no opportunity of acquiring a technical knowledge of

27. The astronomical view. Cosmical, molar, and molecular phenomena.

sidering nature a great impetus. As we have seen, it was entirely an outcome of Newton's great discovery.

It is sometimes useful to distinguish between cosmical, molar, and molecular phenomena; it is, however, well to note that this distinction is a popular or practical, not a scientific one. The question, in how far pure magnitude affects the appearance and relations of the parts or elements of which the universe is composed, is indeed of great scientific interest, but it has not yet received a definite answer. In the meantime we can use the term cosmical for such magnitudes of space, mass, or time as far transcend our own powers of direct measurement by the foot-rule, the balance, and the timepiece, and still more, our powers of direct action: those dimensions compared with which our own homes and actions absolutely disappear. We will call molar those masses which we can handle directly, those dimensions in which we build our own homes and pass our own lives. And we will call molecular those sizes and masses which on the other side are so small that the utmost powers of the microscope and the dividing machine fail to make them directly visible, still less tangible or manageable for our active powers. The lines which limit these three regions are indeed neither fixed nor fixable; the middle region, which

mathematics, and his knowledge of astronomy was mainly derived from books. . . . Thus Faraday was debarred from following the course of thought which had led to the achievements of the French philosophers, and was obliged to explain the phenomena to himself by means of a symbolism which he could understand, instead of adopting what had hitherto been the only tongue of the learned" (Clerk Maxwell, "Action at a Distance," 'Proceedings of the Royal Institution,' vol. vii. Reprinted in 'Scientific Papers,' Cambridge, 1890, vol. ii. p. 317 *sq.* Cf. also vol. i. p. 156). Du Bois-Reymond uses the term "astronomical knowledge" in a somewhat wider sense in his discourse "Ueber die Grenzen des Naturerkennens" ('Reden,' vol. i. p. 120).

we may call our own home, seems to be extending through improved means of seeing and handling; still every one has a vague notion, and science has supported this notion, that there are certain limits, marking the immeasurably large and the immeasurably small, which we cannot transcend. Now it is a question of great scientific interest to what extent mere enlargement, such as the microscope makes familiar to us, would essentially alter the behaviour and appearance of things natural. Would the planetary or stellar systems, reduced in size many million times, present an aspect similar to the view we here enjoy of the inanimate matter on the surface of our earth, and would the molecular structure of microscopic objects, many times enlarged, differ essentially from that aspect? Our present knowledge would lead us to say they would essentially differ. Certain phenomena or modes of motion seem, so far as we know, essentially characteristic of the molecular, others of the molar, others again of the cosmical world.[1]

[1] Laplace has made a significant remark on this point. See 'Exposition du Système du Monde,' 6 éd., p. 319 *sq.*: "La loi de la pesanteur réciproque au carré des distances . . . est celle de toutes les émanations qui partent d'un centre, telle que la lumière; il paraît même que toutes les forces dont l'action se fait apercevoir à des distances sensibles, suivent cette loi: on a reconnu depuis peu, que les attractions et les répulsions électriques et magnétiques décroissent en raison du carré des distances, en sorte que toutes ces forces ne s'affaiblissent en se propageant, que parcequ'elles s'étendent comme la lumière; leurs quantités étant les mêmes sur les diverses surfaces sphériques que l'on peut imaginer autour de leurs foyers. Une propriété remarquable de cette loi de la nature est que si les dimensions de tous les corps de cet univers, leurs distances mutuelles et leurs vitesses, venaient à augmenter ou à diminuer proportionellement; ils décriraient des courbes entièrement semblables à celles, qu'ils décrivent, et leurs apparences seraient exactement les mêmes; car les forces, qui les animent, étant le résultat d'attractions proportionelles aux masses divisées par le carré des distances, elles augmenteraient ou diminueraient proportionellement aux dimensions du nouvel univers. On voit en même temps, que cette propriété ne peut appartenir qu'à la loi de la nature. Ainsi, les apparences des mouvements de

28. Special interest attached to molar dimensions.

And we cannot but be struck by the fact that only those dimensions which we call molar appear to be the abode of living and conscious beings. The cosmical world has, so far as we know, no inhabitant which can behold it in the same way as man beholds this planet, and the same obtains so far as we are acquainted with the molecular world. So far as our knowledge goes and is likely ever to reach, a special importance or dignity will therefore always belong to molar dimensions and masses. The process by which we try to picture to ourselves in tracings and models, constructed in molar dimensions, the behaviour and appearance of cosmical as well as molecular masses will always recommend itself, not only as the most practical, but likewise as the most interesting and plausible, for only by this procedure do these unreachable worlds become amenable to direct observation and to the processes of experiment in the physical laboratory. It seems *prima facie* that the wealth of phenomena and the variety of different kinds of motion decrease as we ascend into the cosmical, or as we descend into the molecular world, giving way in the former to essentially uniform, though to many times multiplied modes of motion, and disappearing in

l'univers sont indépendantes de ses dimensions absolues, comme elles le sont, du mouvement absolu, qu'il peut avoir dans l'espace ; et nous ne pouvons observer et connaître que des rapports." This is easily seen. For if in the formula $f=\frac{m.m'}{r^n}$, the dimensions be all multiplied by K, we get the new formula $F=K^{6-n}\times\frac{m.m'}{r^n}$, and the acceleration of a body moving round a centre like the sun would be $\frac{F}{K^3m'}=K^{3-n}\times\frac{m}{r^n}$, which is only K times the acceleration $\frac{m}{r^n}$, if $n=2$. In another passage Laplace repeats the above statement in slightly different words : "L'univers réduit successivement jusqu'au plus petit espace imaginable, offrirait toujours les mêmes apparences à ses observateurs" (p. 440). That this would not apply to molecular attractions or repulsions is evident.

the latter in stable and self-repeating averages. Possessed therefore, as we seem to be, of the greatest wealth and variety of observations and notions, we may—perhaps erroneously—conclude that we can grasp the simpler cosmical and molecular movements and phenomena by starting from molar, physical, or mechanical models.[1]

[1] English naturalists have always excelled in this line of investigation, whereas foreign scientific literature has been rich in purely mathematical deductions from formulæ which contained no *construirbare Vorstellung*. And it is interesting to note that both lines of thought go back to Newton. Whereas Newton himself believed in the possibility of a mechanical explanation or representation of the gravitation formula, the second edition of the 'Principia' by Cotes can be looked upon as sanctioning the view that gravitation is an ultimate quality which must be accepted as such ; and as it was the second edition through which Newton's ideas became largely known on the Continent, it is not surprising that he was there accused of reintroducing the *qualitates occultæ* of the older metaphysics, which Descartes and others had successfully banished. Clerk Maxwell says ("Action at a Distance," 'Scient. Pap.,' vol. ii. p. 316): "The doctrine of direct action at a distance cannot claim for its author the discoverer of universal gravitation. It was first asserted by Roger Cotes in his preface to the 'Principia,' which he edited during Newton's life. According to Cotes it is by experience that we learn that all bodies gravitate. We do not learn in any other way that they are extended, movable, or solid. Gravitation, therefore, has as much right to be considered an essential property of matter as extension, mobility, or impenetrability. And when the Newtonian philosophy gained ground in Europe, it was the opinion of Cotes rather than that of Newton that became most prevalent." In fact, philosophers could be divided into two classes—those who took the fact of gravity or the wider idea of a universal attraction as a beginning, and drew from this beginning all the possible mathematical and experimental consequences which they could think of ; and those who, whilst admitting this process as a legitimate one, thought it necessary to go behind the assumed beginning and find a still more hidden mechanical reason for this admitted property. To the latter class belonged Newton himself, Huygens, Euler, and in modern times notably Faraday and his followers ; to the former class belonged Daniel Bernoulli, who wrote to Euler, 4th February 1744, referring to the ether theory of the latter : "Moreover, I believe both that the ether is *gravis versus solem* and the air *versus terram*, and I cannot conceal from you that on these points I am a perfect Newtonian, and I am surprised that you adhere so long to the *principiis Cartesianis ;* there is possibly some feeling in the matter. If God has been able to create an *animam* whose nature is unknown to us, He has also been able to impress an *attractionem universalem materiæ*, though such is *attractio supra captum*, whereas the *principia Cartesiana* involve always something *contra captum*" (see

29. Geometrical axioms.

I may, in passing, mention here that in the course of our century certain views have been put forward in pure mathematics, or rather in geometry, which make it conceivable, if not probable, that our ideas of space might not apply to immeasurably small or to immeasurably large dimensions.[1] Should the future progress of thought

Miething, 'L. Euler's Lehre vom Aether,' p. 30). In quite recent times a similar position has again been taken up by Paul du Bois-Reymond in his essay "Ueber die Unbegreiflichkeit der Fernkraft," in the 'Naturwissenschaftliche Rundschau' (vol. iii. No. 14), and in his posthumous work, 'Ueber die Grundlagen der Erkenntniss in den exacten Wissenschaften' (Tübingen, 1890), in which he adds action at a distance as a third "ignorabimus" or unknowable problem to the two given in his brother Emil's address, "Ueber die Grenzen des Naturerkennens" (1872, reprinted in 'Reden,' vol. i. p. 105). On the Continent, about thirty years ago, the fruitlessness of pursuing this problem seemed generally admitted. Helmholtz in 1847 speaks of the initial assumption "that all actions in nature are to be reduced to attracting and repelling forces, whose intensity depends merely on the distance of points mutually acting on each other" (*actio in distans*), and Du Bois-Reymond repeats this in 1871 in his address. But it is significant that Helmholtz, who (through his memoir on vortex motion in 1858) gave such an impetus to the mechanical explanations of molecular forces, modified his views on this point (see his address on Magnus, 1871, 'Vorträge und Reden,' vol. ii.); accordingly in the reprint of his memoir of 1847 he has accompanied it with some significant remarks on the necessity of that initial assumption (1881, 'Wissenschaftliche Abhandlungen,' vol. i. p. 68).

[1] Reimann was probably the first to give expression to this line of thought. His memoir on this subject, "On the Hypotheses which lie at the Foundation of Geometry," bears the date 1854. It was read before the Philosophical Faculty of Göttingen in the presence and at the request of Gauss, on whom it made a profound impression (see the biographical notice on Reimann by Dedekind, attached to Riemann's 'Gesammelte Werke,' Leipzig, 1876). The memoir was not published till after Riemann's death in 1867. In England the late Prof. Clifford introduced the subject to the Cambridge Philosophical Society in 1870: "The axioms of plane geometry are true within the limits of experiment on the surface of a sheet of paper, and yet we know that the sheet is really covered with a number of small ridges and furrows, upon which these axioms are not true. Similarly although the axioms of solid geometry are true within the limits of experiment for finite portions of our space, yet we have no reason to conclude that they are true for very small portions; and if any help can be got thereby for the explanation of physical phenomena, we may have reason to conclude that they are not true for very small portions of space" (see Clifford's 'Mathematical Papers,' p. 21. Compare also his lectures on "The Philosophy of the Pure Sciences" in 'Lectures and Essays,' vol. i. p. 295 *sqq.*)

or observation bring forward any indications that the idea is not only a theoretical possibility, but an actual reality, then the mode of thought now so successfully used—*viz.*, that of transferring phenomena belonging to molar dimensions, and exemplified in the physical laboratory, into cosmic or molecular space by a process of enlarging or of reducing—would become inapplicable. Mathematics indeed would not fail, but our ordinary geometry and the physical model and mechanism would fail: we should probably still be able to calculate, though not to represent, those phenomena of immeasurable dimensions.

As it is, the first great example of calculating and predicting the phenomena of an unreachable world was Newton's successful attempt to explain the movements of the moon, and other cosmical bodies, by using the phenomena of falling bodies on the surface of the earth described by Galileo and Huygens; and he was rewarded by the discovery of a universal law of attraction, which would probably never have been discovered by experiments carried on within molar dimensions, the mass of the earth being so immeasurably greater than that of any molar masses under our control. It quite escapes our observation that in the action and reaction of the falling stone the immensity of the earth's mass is compensated by the vanishing distance through which the earth moves when attracted by the stone. Thus the astronomical view came to the rescue of physical or molar experiments, helped to explain them, and indicated the manner in which cosmical forces could be measured even on the surface of the earth. The pendulum experi-

30. Difficulty of measuring gravitation directly.

ments of Richer, Halley, and many others, the measurements of the arc of the meridian, and Cavendish's and Maskelyne's experiments, were some of the direct results of the discovery.

It was natural that, having explained the cosmical, and subsequently many terrestrial phenomena, successfully by the formula of attraction, Newton himself, and still more Laplace and his school, should have attempted the explanation of molecular phenomena by similar methods.

31. Astronomical view of molecular phenomena.

The astronomical view spread into molar and molecular physics. Newton himself made use of the notion of molecular attraction[1]—*i.e.*, of attraction existing only at

very small distances—to explain the refraction and inflection of light passing from empty space, or from the

[1] In the fourteenth section of the first book of the 'Principia' Newton is, however, careful to speak always of "attractio vel impulsus," leaving it open to the reader to form his own opinion whether it is an action at a distance or a "vis a tergo," a push. He says also that the particles of light approaching solid bodies with a definite velocity are bent, "quasi attracti in eadem (*i.e.*, corpora)." And in the twenty-third query to the first Latin edition of the 'Opticks' (1706) he says: "May not the small particles of bodies have certain virtues, powers, or forces by which they act at some distance, not only on the rays of light, reflecting, refracting, or inflecting them, but also on each other, producing various natural phenomena? For it is sufficiently known that bodies mutually act on each other through the attraction of gravity and through magnetic and electric virtue. And these examples show what is the order and reason of nature, so that it becomes very probable that there may be other attractive forces. For nature is very similar and agreeing to herself. Through what efficient cause these attractions are brought about I do not inquire here. What I here call attraction may well be produced by an impulse or in some other way unknown to us. I take this word attraction here in this way, that it be understood merely to mean some universal force with which bodies try to approach each other, whatever cause this force may have to be attributed to. For from the phenomena of nature it behoves us first to be taught which bodies attract each other, and what are the laws and properties of this attraction, before we inquire by what efficient cause this attraction is brought about. The attraction of gravity and of the magnetic and electric virtue extend to sufficiently large distances, so that they fall under the notice of the vulgar senses; but it may be that there are others which are contained in such narrow limits that they have so far escaped all observation." And he goes on to speak of the deliquescence of some salts and of chemical combinations of finely powdered substances. And further on in the same query, after referring to attractive forces acting only at small distances, he proceeds: "And as in algebra, when the positive quantities disappear and cease, negative quantities begin; so in mechanics, where attraction stops, there a repelling force must come in. But that such a force exists, seems to follow from the reflection and inflection of the rays of light. For the rays are repelled by bodies in both these cases, without the immediate contact of the reflecting or inflecting body. And if all this is so, then the whole of nature will be very simple and similar to herself; performing all the great motions of the heavenly bodies by the attraction of gravity, which exists between all those bodies, and almost all the smaller motions of their particles through some other attracting and repelling force, which exists mutually between those particles" ('Optice,' MDCCVI., p. 341). The suggestions of Newton regarding forces of molecular dimensions were taken up by other contemporary writers and experimentalists, and the 'Philosophical Transactions' during the early years of the last century contain several memoirs touching on this subject, notably by John Keill (1708), who refers to Newton's 'Opticks,' and enlarges, as does also John Freind ('Prelectiones Chymicæ'), on the usefulness of the idea of molecular attraction in explaining chemical and physiological phenomena. In the later editions of the 'Opticks,' evidently in consequence of the elaborate experiments of Hauksbee, Newton enters more fully into the question of molecular, especially capillary, action; and his last query, No. 31, is quoted by Laplace in his 'Théorie de l'Action capillaire,' which forms the supplement to the tenth book of the 'Mécanique céleste.' I may here mention that as some confusion exists in the different editions of the 'Optics' regarding the numbering of the "Queries," it is best to refer to Horsley's Collected Edition of the Works of Newton, where the latest English edition is reprinted, and all the variations and additions noted from the first (English) edition through the subsequent ones. The first edition breaks off with query 16; the first Latin one with query 23, and this was in later editions numbered 31, a number of new queries being inserted, Nos. 18 to 24, referring to the "probability of a medium more subtle than air" and the "mechanical efficient of gravity." This was added "to show" (Newton's words in preface dated 16th July 1717) "that I do not take gravity for an essential property of bodies, . . . choosing to propose it by way of a question, because I am not yet satisfied about it by way of experiments." We may note that this was written a few years after the second edition of the 'Principia' was published by Cotes, whose preface did a good deal to occasion the misunderstanding regarding Newton's views on gravitation as a primary quality of matter. From his correspondence with Cotes, edited by Eddleston (1850), we know that Newton is composing the "Scholium generale," which is added to the second and later editions of the 'Principia,' had intended to say "much more about the attraction of the small particles of bodies," but that on second thoughts he abandoned this intention (p. 147).

atmosphere, into or in the neighbourhood of solid bodies. He conceived light to be a material substance, consisting of minute particles, propelled in straight lines from the luminous centres. These small particles, when arriving at or near the surface of transparent bodies, came under the influence of an attraction from the substance of such bodies, and Newton succeeded in showing that for rays of light which fall on transparent surfaces at an angle, the path of the ray in the body would be deflected according to the rule experimentally determined by Snell, and published by Descartes. This application of the idea of attraction, or action at a distance, to very small or molecular dimensions, required a modification of the gravitation formula. The first who took an important step farther in this direction was Francis Hauksbee. Between the year 1709 and 1713 he made a series of experiments on

32. Capillary action.

what is called capillary action. His experiments were discussed by Newton in the later editions of the 'Opticks,' and followed by those of Dr Jurin in 1718. Hauksbee, Newton, Jurin, and subsequent writers, like Clairaut, all attributed these and similar phenomena to molecular attractions, and Laplace showed that for the mathematical treatment of the subject a knowledge of the exact law (corresponding to the Newtonian law of molar attraction) was unnecessary, but that it was necessary and sufficient to assume the existence of an attraction of the molecules of bodies, which decreases very rapidly as their distances increase, "so as to become insensible at the smallest distances perceptible by our senses."[1] The phenomena of atmospheric refraction as well as those of cohesion and adhesion of bodies—*i.e.*, the attraction of particles of the same or of different matter under what is commonly called contact or at distances which we call in science molecular—were thus submitted to calculation, and the results brought largely into harmony with experience.[1] The problem presented itself and occupied natural philosophers all through the last century, whether a more general law of action at a distance could be found which comprised the phenomena of molecular as well as of molar attraction.

33. Boscovich's extension of the Newtonian formula.

The most celebrated attempt in this direction is that of the Jesuit Roger Boscovich, who in 1758 published an elaborate treatise on this subject.[2]

[1] See 'Mécanique céleste,' vol. iv. (1805), Supplement, p. 67. See also p. 2: "J'ai cherché, il y a long-temps, à déterminer les lois d'attraction qui représentent ces phénomènes: de nouvelles recherches m'ont enfin conduit à faire voir qu'ils sont tous représentés par les mêmes lois qui satisfont aux phénomènes de la réfraction, c'est-à-dire par les lois dans lesquelles l'attraction n'est sensible qu'à des distances insensibles; et il en résulte une théorie complète de l'action capillaire."

[1] The terms insensible and imperceptible, which are commonly used in these discussions, must be taken with caution. It is now known that, though not directly perceptible or sensible, the distance through which molecular action takes place is measurable. Plateau in Belgium (1843 and following years) and Quincke in Germany (1868) made experiments on independent lines, and came to very similar results. The distance of molecular action appears to be about the twenty thousandth part of a millimetre. See Clerk Maxwell's article on Capillary Action in the 9th edition of the 'Ency. Brit.,' reprinted in 'Scientific Papers,' vol. ii.; also Violle's 'Cours de Physique,' German edition, vol. i. p. 591, &c., and p. 639.

[2] Roger Joseph Boscovich, of the Society of Jesus (1711-87), took up the ideas thrown out by Newton in the last query to the 'Opticks,' and published in 1758 at Vienna an elaborate treatise with the title 'Theoria Philosophiæ Naturalis redacta ad unicam legem virium in Natura existentium.' A second edition was published at Venice in 1763. His speculations begin with the year 1745, when he hit upon his general view that all forces in nature can be reduced to the action of indivisible and inextended atoms, endowed with inertia and with a mutual force which at vanishing distances is repulsive, which at insensible distances alternates according to some mathematical formula between repulsion and attraction, and, finally, at sensible distances becomes identical with Newton's force of gravitation. The general form of the curve which exhibits this action at a distance is given, and the algebraical formula discussed, in the Supplement. But it was, of course, impossible to define the law any further. The

Though many of the views contained in this treatise were really the same as those embraced by a large school of Continental mathematicians till far into this century,

whole treatise is really more of a philosophical than a mathematical or experimental investigation. A large portion is taken up in defending his view against possible objections, and in showing how it agrees with or differs from the philosophies of Leibniz and Newton. Whilst this treatise represents in general a view largely held by Continental philosophers of nature, it does not contain any new mathematical methods such as the 'Principia' contained before and Laplace's 'Mécanique céleste' later, nor does it contribute any experiments such as those works likewise contained and suggested to others. In fact, it is more a metaphysical than an exact treatise, and as such has exerted no lasting beneficial influence on the progress of science. "The eighteenth century made a school of science for itself, in which for the not unnatural dogma of the earlier schoolmen, 'matter cannot act where it is not,' was substituted the most fantastic of paradoxes, *contact does not exist.* Boscovich's theory was the consummation of the eighteenth - century school of physical science. This strange idea took deep root, and from it grew up a barren tree, exhausting the soil and overshadowing the whole field of molecular investigation, on which so much unavailing labour was spent by the great mathematicians of the early part of our nineteenth century. If Boscovich's theory no longer cumbers the ground, it is because one true philosopher required more light for tracing lines of electric force" (Sir William Thomson's Lecture before the Royal Institution, May 1860. Reprinted in 'Papers on Electrostatics and Magnetism,' 2nd ed., 1884, p. 224). Nevertheless it is extraordinary to note that Boscovich's theory was more popular among British than among Continental physicists. In France the book seems to have been little appreciated, although Boscovich was well known through his optical and astronomical researches (see Montucla's 'Histoire des Mathématiques,' vol. iii. p. 490, vol. iv. p. 188); and his differences with d'Alembert were notorious. But French science was then occupied less with metaphysical theories than with mathematical analysis and experimental research. In Germany the book remained unknown, probably because Euler's authority favoured an opposite theory. In this country, however, the theory is often referred to from the time of Priestley ('History of Optics') to Faraday ("On the Nature of Matter," 'Phil. Mag.,' 1844, vol. 24), and more recently Thomson (Lord Kelvin). The last has probably more than any other living writer of similar eminence referred to Boscovich, whose theory he considers suggestive, and we are indebted to him for the first serious attempt to establish by actual calculation the real capabilities of the Boscovich atoms in explaining the properties of chemical molecules, their stability and degree of saturation (see the Report of the British Association at Liverpool, 1896). In Scotland Boscovich's theory was fully discussed in a posthumous article on "Corpuscular Forces" by John Robison, Professor of Natural Philosophy at Edinburgh, and published by Brewster in the 1st volume of Robison's 'System of Mechanical Philosophy' (Edinburgh,

the book was almost completely forgotten on the Continent.[1] No real progress has indeed been made in the explanation of physical phenomena by the application of

1822). His 'Elements of Mechanical Philosophy' (Edinb., 1804) betray, according to Dugald Stewart, "a strong and avowed leaning to the theory of Boscovich" (Works by Hamilton, vol. v. p. 107). The theory probably found favour, among other reasons, because it seemed to give support to the prevalent corpuscular theory of light, which Euler opposed, as he did simple action at a distance. In the Scotch school of philosophy, of which Dugald Stewart was the most popular exponent, Boscovich was well known. Stewart refers to him frequently (Works by Hamilton, vol. ii. pp. 50, 107, 110, 343; vol. iii. p. 233; vol. v. p. 93 *sqq.*; vol. vii. p. 173 *sqq.*) He quotes Priestley, Robison, and James Hutton as followers of Boscovich, whilst his own adherence is certainly very qualified, and he makes a very pertinent remark in his Introduction to the 'Elements of the Philosophy of the Human Mind' (1792): "I cannot help taking this opportunity of remarking that if physical inquirers should think of again employing themselves in speculations about the nature of matter, instead of attempting to ascertain its sensible properties and laws (and of late there seems to be such a tendency among some of the followers of Boscovich), they will soon involve themselves in an inextricable labyrinth, and the first principles of physics will be rendered as mysterious and chimerical as the pneumatology of the schoolmen" (vol. ii. p. 50). Boscovich seems to have been fond of tracing mathematical curves to represent all kinds of processes, such as the intellectual advancement of the age, and he shows graphically that this was declining (Dugald Stewart's quotation in his 'Dissertation,' Works, vol. i. p. 499).

[1] When Fechner published the first edition of his 'Atomenlehre' (1st ed., Leipzig, 1855; 2nd ed., 1864), he does not seem to have known of Boscovich's treatise (see p. 229 of the 2nd edition), and it was similarly unknown to the Dutch meteorologist Buys Ballot, whose curves of the attracting and repelling forces of matter agree almost exactly with those of Boscovich (see 'Fortschritte der Physik,' 1849, p. 1 *sqq.*; also Rosenberger's 'Geschichte der Physik,' vol. iii. p. 536 *sqq.*) In French scientific literature the treatise of Boscovich is mostly ignored—the 'Grande Encyclopédie' does not even give its title. In fact, French science does not consider itself beholden to the celebrated Jesuit for what I call the astronomical view of matter. See St Venant in 'Comptes Rendus,' vol. 82, p. 1223: "Plusieurs auteurs, soit anglais, soit allemands, dans ses œuvres qui sont du reste d'une haute portée, . . . se sont pris à condamner vivement, sous le nom de *théorie de Boscovich*, non pas son idée capitale de réduction des atomes à des centres d'action de forces, mais la loi même, la loi physique générale des actions fonctions des distances mutuelles des particules qui les exercent réciproquement les unes sur les autres. Et ils attribuent ainsi au célèbre religieux *l'erreur grave* où sont tombés, suivant eux, Navier, Poisson et nos autres savants, créateurs, il y a un demi-siècle, de la mécanique moléculaire ou interne. Or cette loi blâmée, cette loi qui a été, mise en œuvre aussi par Laplace, &c., et

Boscovich's or similar formulæ, though the idea of action at a distance between the minute particles of matter underlies the theories by which Poisson, Navier, Cauchy, Lamé, and others calculated the effect of elastic forces in solid bodies, or the phenomena of light passing through transparent and crystalline substances. A different school of physicists, starting from ideas of a different kind, with which we shall become acquainted hereafter, have shown that specific notions as to the molecular structure of bodies are not required in order to deal with the phenomena referred to. Nevertheless, the idea of action at a distance governing the movements of immeasurably small, as it seemingly does those of immeasurably large masses in nature, received a great support by the development of two other branches of science, which belong essentially to the history of the present century.

34. Coulomb's measurements.

The sciences of electricity and magnetism can be said to have originated with Coulomb's accurate measurements with the torsion-balance. With this instrument he measured the attracting and repelling forces of bodies, electrified or magnetised, by comparing them with the mechanical forces required to twist a metallic wire. In this way he fixed what have ever since his time been termed the units of electricity or magnetism, reducing these quantities to the same system of measurement with which we measure the masses or inertia of moving bodies. His methods were adopted and modified and greatly perfected by Gauss and Weber—the

35. Extended by Gauss and Weber.

prise par Coriolis et Poncelet pour base de la mécanique physique, n'est autre que celle de Newton lui-même, comme on le voit non seulement dans son grand et principal ouvrage, mais dans le scholie général de sa non moins immortelle 'Optique.'"

former applying them to the measurement of the magnetic forces of the earth, the latter to that of the forces exerted by currents of electricity—*i.e.*, by electricity which is not at rest but in motion. As I have already stated, the measurements of Coulomb confirmed the prevalent notion that action at a distance, varying inversely as the square of the distance, and directly in the proportion of the quantities of the acting substance, was a universal formula or law of nature.[1] The idea

[1] Coulomb's exact measurements of the attraction and repulsion at a distance of electrified bodies and of magnets were published during the years 1784 to 1789 in seven memoirs presented to the Paris Academy of Sciences. They are conveniently collected, together with some other memoirs of Coulomb, Poisson, and others on kindred subjects, in the first volume of the 'Collection de Mémoires relatifs à la Physique,' published in 1884 by the Société française de Physique. Coulomb made use of the torsion-balance and the proof-plane, the actions of which he carefully examined. He confirmed the law, which had been vaguely or approximately expressed by various writers before him, that electrified bodies act on each other with a force which is proportional to the inverse square of their distances. This he did by direct measurements of the repulsion of small electrified bodies in the torsion-balance (1785, 1st Mémoire). He then extended his measurements by an indirect method to the action of electrified bodies of larger size and to magnets (2nd Mémoire). He also defined what is meant by quantity and density of electricity and magnetism, and showed how these could be measured and how the action of electrified bodies and magnets depended on the more or less of these quantities. Coulomb's researches contain experiments of great delicacy. Although the laws which bear his name appear so simple when written down, the phenomena they represent are most complicated, as in the case of electricity the effect of electrical influence, called by Faraday induction, and in the case of magnetism the presence of the earth's magnetism, and the fact that we have never to do with one kind of magnetism but always with two states, destroys all chance of exhibiting experimentally the simple case represented by the mathematical formula. It was therefore necessary to consider this formula as being merely a convenient description of the elementary action of supposed isolated quantities of electricity and magnetism, and by a process of summation to deduce mathematically the actual effects for such cases of interaction as are actually observable in the laboratory. It was especially the phenomena of the distribution of electricity on the surface of electrified bodies of simple shape and the distribution of magnetic forces in the neighbourhood of magnets which had to be calculated and measured. In physical astronomy a similar course of reasoning and observation combined had verified

of mass, which in the Newtonian formula meant merely the quantity of matter, had indeed to be enlarged, and to the attracting forces had to be added those of repulsion; still, though physically the phenomena were entirely different, the mathematical expression which ruled the two electric and the two magnetic quantities, usually termed fluids, looked very much like the Newtonian gravitation formula: it betrayed philosophers into thinking they possessed an explanation where really they had only a measurement and a description.[1]

Newton's elementary law of gravitation, Laplace as it were summing up the evidence in his great work. What Laplace did for Newton was done by Poisson for Coulomb's elementary law of electric and magnetic action, and on a still larger scale by Gauss, who worked out the mathematical theory and applied it to the case of the magnetic distribution on the earth's surface. In England, already before Coulomb's researches were published, Cavendish had, likewise by a combination of experiment and calculation, established the elementary formulæ and properties of electrical phenomena. See note to the following page.

[1] The exact measurements of Coulomb and the mathematical analysis of Poisson and Gauss superseded the vaguer discussions on the nature of electricity and magnetism which were very frequent before that period, just as the mathematical principles of Newton and Laplace drove into the background the discussion on the nature and cause of gravity. Coulomb himself does not profess to settle the controversy carried on between the two schools of which Dufay and Franklin can be considered as the principal representatives—*viz.*, whether there existed two electric fluids or only one. Coulomb judged the rival views simply as to their usefulness in describing and measuring phenomena: "Comme ces deux explications n'ont qu'un degré de probabilité plus ou moins grand je préviens, pour mettre la théorie . . . à l'abri de toute dispute systématique, que dans la supposition des deux fluides électriques je n'ai d'autre intention que de présenter avec le moins d'éléments possibles, les résultats du calcul et de l'expérience, et non d'indiquer les véritables causes de l'électricité" ('Collection de Mémoires,' vol. i. p. 252). He had previously, in 1777, rejected the theory of vortices to explain magnetic phenomena: "Il semble qu'il résulte de l'expérience que ce ne sont point des tourbillons qui produisent les différents phénomènes aimantains, et que, pour les expliquer, il faut nécessairement recourir à des forces attractives et répulsives de la nature de celles, dont on est obligé de se servir pour expliquer la pesanteur des corps et la physique céleste" (vol. i. p. 8). And in 1789 he is still more cautious: "Pour éviter toute discussion, j'avertis . . . que toute hypothèse d'attraction et de répulsion suivant une loi quelconque ne doit être regardée que comme une formule qui exprime un résultat d'expérience" (vol. i. p. 297).

The extension and confirmation which the Newtonian attraction formula had thus gained in the minds of many seemed to be entirely upset by a series of discoveries in which electrical, and subsequently magnetic, phenomena played an important part. These were, the discovery of galvanic electricity by Galvani in 1791 and by Volta in 1800; of the physiological and chemical effects of this form of electricity, especially by Davy (1806); of the magnetic effect of moving electricity by Oersted in 1820; of the connection of heat and electricity by Seebeck in 1822; of induction by Faraday in 1831—*i.e.*, of the action of electric currents and magnets in generating other electric currents or magnetic effects in bodies which are moving in their neighbourhood; and, finally, of diamagnetism by Faraday in 1845.

Many of the celebrated men with whose names the modern discoveries in electricity are identified, and amongst them notably Davy and Faraday, were not brought up in the mathematical school of the Continent,[1] in which 36. Davy and Faraday.

[1] To these must be added the name of Cavendish (1731-1810), whose electrical researches, in which he anticipated many of Coulomb's results, proceeded on entirely different lines from those of the Continental school. He proved—in or before 1773—from the fact that a small globe situated in the hollow of a large electrified globe and communicating with it showed no signs of electricity, that electric attraction and repulsion must be inversely as the square of the distance. In his published and posthumous papers (edited by Maxwell in 1879 under the title of 'The Electrical Researches of the Hon. Henry Cavendish') he anticipated, as Maxwell has shown, many later investigations of British and Continental writers. He had a clear notion of electrical capacity, of potential and of electrical resistance, he anticipated Ohm's law—*i.e.*, the proportionality between the electro-motive force and the current in the same conductor. He studied the properties of dielectrics, and "not only anticipated Faraday's discovery of the specific inductive capacity of different substances, but measured its numerical value in several substances"

the astronomical view of phenomena had been established and strengthened mainly by a development of the Newtonian philosophy. They belonged to another school, which approached that great field of research from the purely experimental side,—mainly, so far as Davy was concerned, from the side of chemistry, which, dealing with the qualitative, not merely the quantitative, properties of matter, was at that period almost entirely thrown

(Maxwell's Introduction to the 'Researches,' p. xlix *sqq.*) Cavendish's electrical work seems to have remained unnoticed abroad. Cuvier, who fully appreciates him as a pioneer in modern chemistry, does not refer to his electrical researches, and in Continental works his name is hardly mentioned in connection with electrical science. He, however, clearly belongs to the same lineage as Davy and Faraday, whose breadth of experimental observation somewhat prevented them from fully assimilating the results of Coulomb and his school, which moved in narrower but more precise lines. If Cavendish was unknown abroad as an electrician, Coulomb was little known in England. Whewell, who did more than any other to make known the researches of the mathematical school (see his article in the 'Encyclopædia Metropolitana,' 1826, and his British Association Report, 1835), could state in the first edition of his 'History of the Inductive Sciences' (1837) that "the reception of the Coulombian theory has hitherto not been so general as might have been reasonably expected from its very beautiful accordance with the facts which it contemplates" (3rd ed., vol. iii. p. 28). He then refers to the experiments of Snow Harris. These experiments, as well as those of Faraday, carried on about the same time, dealt largely with the properties of dielectrics and of what we now call the electric field, a subject almost entirely neglected by the mathematical school of that period. It was not till 1845 that William Thomson (Lord Kelvin) cleared up the whole subject in a memoir, "On the Mathematical theory of Electricity in Equilibrium" (see 'Reprint of Papers,' &c., p. 15). He there refers to the fact that "many have believed Coulomb's theory to be overturned by the investigations" of Snow Harris and Faraday, and he therefore proposes to show that "all the experiments which they have made having direct reference to the distribution of electricity in equilibrium are in full accordance with the laws of Coulomb, and must therefore be considered as confirming the theory" (p. 18). He thus brought together the two independent lines of research and thought, the mathematical and the experimental, represented by the school of Gauss and Weber abroad, and by Faraday in England, and suggested those further researches of which Maxwell's 'Treatise on Electricity and Magnetism' is the great exponent. See the preface to this work, p. xi, &c., 1873; also Maxwell's 'Scientific Papers,' vol. ii. pp. 258, 302, 304.

upon experimental research.[1] Chemistry had only just entered the list of the exact sciences, by the use of the balance, largely owing to Lavoisier and his followers.

[1] Although Faraday's 'Experimental Researches in Electricity' (1831-52) contain mostly what chemists would call "qualitative" investigations and only few exact "quantitative" measurements—forming in this respect a very remarkable contrast to Weber's 'Electrodynamische Maasbestimmungen' (1846-78)—it is important to remark that one of the methods for exact measurement of the electric current—*viz.*, by the chemical decomposition of compounds—was established by Faraday in 1833 and 1834. He showed that whenever decomposition took place the quantities decomposed were in proportion to the amount of electricity flowing through the circuit and in proportion to the chemical equivalents. Owing to the want of a clear definition of quantity and intensity of current, Berzelius opposed this view of Faraday's as illogical, confounding the quantity of substance decomposed with the force required to set it free. Clearer definitions and accumulated experience have confirmed Faraday's law, which is now looked upon as one of the best established general facts of chemical and electrical science. Somewhat earlier than Faraday, Georg Simon Ohm established (1827, 'Die galvanische Kette, mathematisch bearbeitet') the proportionality of the quantity of electricity passing through a circuit with the electromotive force in the same conductor, introduced the notion of electrical resistance, and showed how this varies as the length and inversely as the thickness of the same conductor, and is different in different conductors. The accuracy of Ohm's law, though elaborately tested by Fechner and confirmed by Pouillet, was frequently doubted; in France it met with tardy recognition, and in England some of the most important researches—such as those of Faraday—were carried on without reference to it. In the first edition of Whewell's History it is not mentioned. When the second edition was published (1847), Ohm had received the Copley Medal of the Royal Society (1841), and Wheatstone had besides in the year 1843 drawn attention to the clear definitions which Ohm had introduced. The opinion has been expressed that Ohm found his law by theoretical considerations based on analogy with the flow of heat in conductors, and that he subsequently proved it experimentally. The publication of Ohm's collected papers by Lommel ('Gesammelte Abhandlungen,' Leipzig, 1892), however, disproves this opinion; as his experimental measurements had during 1825 and 1826—not without some initial mistakes—led him to the well-known expression of the relations of the different quantities (see Lommel's Introduction, p. vii). Whereas in Germany it was a purely scientific interest—that, namely, of subjecting physical phenomena to mathematical calculation—which induced Ohm, Gauss, and Weber to devise instruments and methods for exact measurement, it was in England mainly the practical requirements of telegraphy which created the desire for clear definitions and exact methods. With these requirements in view Wheatstone invented his instruments and drew attention to the definitions of Ohm. See his Bakerian Lecture for

Yet the great variety, more than the exact measurement of phenomena, attracted the attention of natural philosophers in this new field. And when through Davy, Berzelius, and Faraday in different ways the importance of electric action in chemical processes became established, it was natural that from this school an entirely different view of electrical and magnetic phenomena should emanate: we may term it—in opposition to the astronomical—the physical view of phenomena. This view, which, as the astronomical view had done, found later on its expression in a mathematical formula, will occupy our attention in a subsequent chapter. It has in the course of the second half of the century very largely expelled the other and rival view from the domain of molar and molecular physics. But the astronomical view, with its largely developed mathematical apparatus, was not easily defeated: it was quite able to grapple with even such complicated processes as the discoveries of Oersted and Faraday had revealed.

37. Ampère and Weber develop the astronomical view.

In the opinion of many Continental thinkers it won its greatest laurels when, under the treatment of Ampère in France and of Neumann and Weber in Germany, the perplexing interactions of magnets, diamagnets, and

1843 ('Philos. Transactions,' 1843, p. 303, &c.): "An energetic source of light, of heat, of chemical action, and of mechanical power, we only require to know the conditions under which its various effects may be most economically and energetically manifested to enable us to determine whether the high expectations formed in many quarters of some of these applications are founded on reasonable hope or on fallacious conjecture." Forty years later Lord Kelvin, in his address "On the Electrical Units of Measurement" (1883; see 'Popular Lectures and Addresses,' vol. i. p. 76), could still speak of the comparatively recent date at which "anything that could be called electric measurement had come to be regularly practised in most of the scientific laboratories of the world," whereas such measurements had then been for many years "familiar to the electricians of the submarine cable factories and testing stations."

electric circuits—the phenomena of electro-magnetism, diamagnetism, and induction—were all resolved into elementary processes of attraction and repulsion, and summed up in a formula which looked like an extension of the Newtonian gravitation formula, revealing the mysterious influence of molecular forces.

"Oersted had found that an electric current acts on a magnetic pole, but that it neither attracts it nor repels it, but causes it to move round the current. He expressed this by saying that the electric conflict acts in a revolving manner. The most obvious deduction from this new fact was, that the action of the current on the magnet is not a push-and-pull force, but a rotary force, and accordingly many minds began to speculate on vortices and streams of ether, whirling round the current. But Ampère, by a combination of mathematical skill and experimental ingenuity, first proved that two electric currents act on one another, and then analysed this action into the resultant of a system of push-and-pull forces between the elementary parts of these currents."[1]

Weber in Germany took up the work where Ampère had left it.[2] One of his objects was to combine the

[1] Clerk Maxwell "On Action at a Distance" ('Scientific Papers,' vol. ii. p. 317).

[2] Weber's interest was twofold. The primary object was to put accurate quantitative data in the place of merely qualitative descriptions or mere estimates of phenomena. He had then already published, together with his brothers (see *supra*, p. 196, note 3), two works in which in a similar way exact research has taken the place of inexact description. The first was his experimental investigation of wave-motion ('Die Wellenlehre auf Experimente gegründet,' 1825), the other the still more delicate attempt to treat a physiological phenomenon, the mechanism of the organs of locomotion, on exact mechanical principles (1836). This rare gift of exactness, invaluable at all times, but almost unique at that time in Germany, where philosophical vagueness was only too common, attracted the notice of Gauss, who brought Weber to Göttingen in 1830 after

different electric phenomena—those of electricity in the state of rest, called statical effects; those of electric currents on each other, the dynamical results; and those of electric conductors in a state of motion, the phenomena of induction—in one general and fundamental formula or law. He had before him Coulomb's electro-static formula, Ampère's electro-dynamic formula, and a more general one established by Franz Neumann, which described and embraced not only the phenomena discovered by Oersted, but also those of moving conductors discovered by Faraday. It is not necessary here to enter into the details of the investigations, experimental and mathematical, by the aid of which Weber succeeded in establishing his very remarkable and seemingly all-embracing formula. Two remarks, however, present themselves, bearing upon the history of thought and the value of precise mathematical expressions. The first is, that as the gravitation formula necessitated a series of the most careful definitions and measurements of physical quantities, and the invention of accurate instruments and

38. Weber's fundamental measurements.

methods of measurement, so the first and probably the most valuable performances of Weber were his ingenious apparatus, and the careful measurements by which he

the death of Tobias Mayer. Gauss introduced Weber to his own exact measurements of terrestrial magnetism, and from hence Weber's own line of thought led through the phenomena of magneto-induction (discovered by Faraday in 1831) and terrestrial magneto-induction (1832) to electro-dynamics, the science which Ampère had created in the years 1820 to 1823. In 1846 Weber speaks in the introduction to the 'Electro-dynamische Maassbestimmungen' of the endeavour to determine natural phenomena according to number and measure, expressing surprise that this has not yet been done in electro-dynamics, and then proceeds to describe his "electro-dynamometer," an instrument used by him for many years. With this instrument he then, further, proceeds to confirm Ampère's formula for the action at a distance of the elements of electric currents.

fixed the elementary conceptions and quantities with which he operated. All his researches were comprised under the very significant title "electro-dynamical measurements." As such they remain a great monument of ingenuity and unparalleled accuracy.[1] The second

[1] Gauss had, some years before Weber commenced his electrical researches, introduced the idea of an absolute measure of other than mechanical forces—*i.e.*, following up the definition of force in the Newtonian laws of motion, that it is the cause which brings about a change of motion, he suggested that every physical force can be measured by the velocity it imparts to a movable body of measurable mass, the quantity of mass being in the same locality measured by its weight; and he applied this to the measurement of magnetic forces. In applying the same idea to the measurement of electric currents, Weber came at once upon the circumstance that the forces exerted by an electric current can be measured in two ways—*viz.*, by the action they have upon magnets or by that which they have on other electric currents. Now by a familiar conception, electricians look upon a current of electricity as measurable by the quantity of electricity which flows through a section of the circuit in a given unit of time, this quantity of electricity being measurable in the same way as Coulomb measured the action at a distance of charged bodies. Should it then be possible to carry out this latter measurement of an electric current, a comparison between the electro-magnetic and the known electro-static units of electricity would become possible. Faraday had already, in 1833 and 1834, made estimates of the numerical relation of the quantity of electricity in a current, measured by its chemical or electro-magnetic effects, and of the same quantity if produced by an electrical machine. These estimates were more than twenty years later, in 1856, reduced to accurate measurements by Weber and Kohlrausch. Through these measurements, which confirmed the enormous numbers which are revealed when we compare electricity at rest and electricity in motion, Weber finished the series of accurate measurements, reduced to an absolute or mechanical standard, which had been begun by Gauss in 1833. It was soon recognised of what practical importance these data must be to electricians. Accordingly the British Association at their meeting at Manchester in 1861 appointed a committee, on the suggestion and under the presidency of Sir William Thomson, called the "British Association Committee of Electrical Standards." "This committee worked for nearly ten years through the whole field of electro-magnetic and electro-static measurement, until in its final report, presented to the Exeter meeting in August 1869, it fairly launched the absolute system for general use" (Thomson, 'Popular Lectures and Addresses,' vol. i. p. 84). In recognition of Weber's great merit in first introducing this system into electrical science and practice, the name "Weber" had been selected by Latimer Clark for the unit of current. In the final fixing of the units in Paris in 1881 other units than those previously in use were adopted, and to avoid confusion the names were somewhat differently

point I wish to urge is, how in those days the Newtonian formula was taken as the great model of a law of nature, and how the researches of Coulomb, Poisson, Ampère, and Weber stand in logical connection with the theory of gravitation. Let us see what Weber himself says on this subject:[1] "After the general laws of motion had fur-

chosen. This explains the fact, deplored by Weber's friends and admirers, that his name has dropt out of the list of terms now adopted throughout the civilised world. (See Wiedemann, 'Die Electricität,' Braunschweig, 1885, vol. iv. p. 906, &c.) Recently Prof. Lodge has suggested the introduction of the names of Weber and Gauss to denote some of the derived units in the electrical measurements. See Brit. Assoc. Report, 1895, p. 197 n.

[1] Weber's theoretical conception of the nature of electric action at a distance is mixed up with his exact measurements of electrical quantities, though these can be stated without making use of his theoretical conceptions. It is the nature of the absolute system of measurement that it establishes numerical relations based upon a small number of original units (space, time, and mass, or space and time alone, see note to p. 323 above) which are universally intelligible. Whatever, therefore, the theoretical views may be which led the investigation, in the end these are eliminated in the system of original (primary) and derived (secondary) units. But Weber's theory commands attention for its own sake as the furthest stage to which the gravitational view of phenomena, provisionally introduced by Newton, has been pushed. It has been extolled and condemned, according to the favour with which the purely mathematical treatment of phenomena has been received. In the school of Laplace this purely mathematical treatment quite obscured all other views which did not minister to it. Thus Laplace remained to the end an adherent of the emission or corpuscular theory of light, and opposed the ideas of Young and Fresnel, who developed the dynamical view. In order to make the cosmical view of nature useful for the explanation of molecular phenomena, two distinct and definite conceptions, contained in the gravitation formula, had to be modified and enlarged. The conception of matter, which in physical astronomy is limited to gravitational matter, had to be extended so as to bring into calculation what was then called imponderable matter, such as light, heat, and electricity. And the law of gravitation, which defines the purely attractive property of ponderable matter, had to be modified so as to embrace also the repulsive action observable in a certain class of phenomena. Coulomb had shown that ponderable matter charged with electricity followed the same formula for attraction and repulsion as gravitating bodies did: he simply adopted the two-fluid theory of electric matter. Poisson developed the mathematics of fluids, actuated by repelling forces depending on the inverse square of the distance. Oersted showed the action of electric currents on magnets; and Ampère showed that magnets can in their action be supplanted by electric currents. Laplace very early satisfied himself that

nished a foundation, there remained in physics mainly the investigation of the laws of interaction of bodies; for without interaction bodies would for ever remain in that state of rest or motion in which they happened to be.

these actions of ponderable matter, in which electricity was flowing, could be reduced to an action at a distance proportional to the inverse square of the elements of the electric circuits. When Faraday showed that a current of electricity under certain conditions induced in conductors in its neighbourhood other currents, this was explained by saying that the electric fluid exerted not only pondero-motoric but also electro-motoric action at a distance. Not only did electrified matter act on other electrified matter, but electricity as a fluid acted on electricity itself. Weber adopted, for the purpose of putting these apparent actions into mathematical language, and for finding an elementary law of the ultimate particles of electric matter out of which by summation the observable data might be calculated, the hypothesis of Fechner, according to which in an electric current the two electric fluids were moving with equal velocity in opposite directions. It then became evident —looking at the phenomena discovered by Oersted, Ampère, and Faraday—that the electro-static formula of Coulomb required to be supplemented by an additional term, if the mutual action was to be determined not only for the case of equilibrium and rest, but also for that of relative motion. The additional term, depending on this relative motion, had to be found. (See 'Electrodynamische Maasbestimmungen,' vol. i. p. 102). From this starting-point, and with this definite problem in view, Weber undertook a series of most valuable measurements. No doubt can exist as to the lasting importance of these measurements. Any theoretical conception which produces in its application such results must hold a prominent place in the history of scientific thought. And the very fact that, unlike Boscovich and other purely metaphysical theorists, Weber undertook to fix by experiment the actual constants or numerical quantities which his abstract formula contained, led to much enlargement of actual knowledge. I will mention only one of the most interesting points in his elaborate researches. I stated above that it took a whole century after the discovery of the law of gravitation before the gravitation constant was approximately fixed, but that for the progress of physical astronomy this was of little importance, gravity being a universal property of matter. Still such a constant exists, because we possess another definition of matter —*viz.*, inertia or mass. The constant in Coulomb's law cannot be determined in a similar manner, as the property of attraction or repulsion defines for us ultimately the numerical quantity of electricity. We have—so far—no other ultimate absolute measure of electricity. But in Weber's law it was the quantities of electrical matter which acted on each other not only according to their distances, but also according to their relative motion or their velocities. A second constant thus entered into his formula, and this constant established a relation between electricity at rest and electricity in motion. This constant was a velocity, and, if determinable, it revealed a constant of nature in

All changes of these states, and all phenomena dependent thereon, are therefore consequences of these interactions. But bodies exert such mutual actions when in contact as well as from a distance, and it was evident that a beginning had to be made with the latter in order to gain a clue for the investigation of the former; this being especially needful whenever the spatial relations of bodies escape observation, as is the case with bodies which are in contact. And so it has really happened, inasmuch as a beginning was made by examining the mutual action of cosmic bodies—*i.e.*, with the phenomena of gravitation. To this first field of research—*viz.*, the phenomena of gravitation—there was then added the investigation of electric and magnetic interactions, as next to gravitation these are the only actions which take place from one body to another at measurable distances,—these actions being themselves measurable. Now for a long time Newton's doctrine of gravitation furnished the leading idea for nearly all theories of electricity and magnetism, till a new clue was gained through Oersted's and Ampère's discoveries

the form of a velocity. It had for Weber a theoretical as well as a practical meaning, for it enabled him to effect a connection between the electro-magnetic and the electro-static or absolute system of measurements. When he succeeded in measuring this quantity, it was found that the figure for the constant, which meant a velocity, was practically the same as that for the velocity of the propagation of light. Weber himself does not seem to have attached any physical meaning to this coincidence: later he and Kirchhoff remarked that under certain conditions an electrical wave-motion might take place in an electrical conductor, and that the velocity of the propagation of this would coincide with that of light (see Kirchhoff in 'Annalen der Physik und Chemie,' 1857; and Weber, 'Electrodyn. Maasbest.,' 1864). It was reserved for Clerk Maxwell to point to the real physical interpretation of Weber's constant. Of this I shall speak in a later chapter (see Maxwell's memoir 'On Physical Lines of Force,' 1862, reprinted in 'Scientific Papers,' vol. i.)

regarding the equivalence of closed electrical currents with magnets. This led, first, to the reduction of all magnetic effects to the action of electrical currents; and, secondly, to the enunciation of a fundamental law of the interaction of two elements of electricity in motion. A third leading idea was that of reducing the interaction of all bodies to that of the mutual action of pairs of bodies. This idea could in general be considered as well established and confirmed by experience on a large scale." [1]

39. Necessity of developing the infinitesimal methods.

This leads me to another and a final remark on the view of natural phenomena, first introduced by Newton's gravitation formula, which has been so successful in the calculation of all the movements of cosmic bodies, and which in the eyes of such a great authority as Laplace contained the clue to an explanation also of molar and molecular phenomena.[2] This view calculates

[1] 'Electrodynamische Maasbestimmungen,' 1878, p. 645.

[2] Although Weber followed the lines so deeply impressed upon the whole of Continental thought by the labours of Laplace and his school, it does not seem that he held the same exalted opinion of the value of any mathematical formula as did Laplace. Though he looked upon his electro-dynamic law as well established by experiment and valuable in guiding further research, he was fully impressed with the fact that all such formulæ are merely provisional. Thus he says in the first part of his researches, written in the year 1846: "It seems to follow that the immediate interaction of two electrical particles does not depend upon these alone, but also upon the presence of third bodies. . . . It is conceivable that the forces comprised in the discovered fundamental law may be partly the forces which two electrical particles exert indirectly on each other, and which therefore depend on the intervening medium. . . . The general law for the determination of the acting forces might perhaps be yet more simply expressed by taking the intervening medium into account, than has been possible without it in the fundamental law now established. The exploration of the intervening medium, which might afford an insight into many other matters, can alone give an answer to this question. . . . A hope now exists that it will be possible, in several new ways, to gain some information as to the neutral electric fluid which pervades everything. Perhaps in

the actions of large masses and complicated systems of bodies by a process of summation from the interaction of units placed in the simplest relation—that of two and two, pushing or pulling each other in a straight line. Now, in consequence of the great distances at which we are placed from the heavenly bodies, these appear to us as mere points, and the observation of their movements, their orbits, and their periods enabled astronomers like Kepler, and mathematicians like Newton, to gain by mere observation and subsequent calculation an idea of the elementary rule which masses, considered to be concentrated in points, follow in their motion in a connected system. The next step was to see how these elementary actions would add up in cases where the dimensions of the moving bodies were not vanishingly small in comparison with their distances. The infinitesimal methods, invented in the age of Newton, and developed by him and others into a special calculus, came to the aid of mathematicians, and enabled them to calculate from elementary data the motions and phenomena of extended bodies and systems of bodies. These could afterwards be actually measured, thereby confirming the elementary formulæ and assumptions which had formed the basis of those calculations. As already remarked, this process

other bodies, which are not conductors, there exist, not currents, but only vibrations, which may in future be observed by the methods indicated above. Further, I need only point to Faraday's recent discovery of the influence of electric currents on the vibrations of light, which makes it probable that the all-prevading neutral electric medium itself constitutes the all-prevading ether which contains and propagates luminous vibrations, or at least that the two are so intimately connected that the observation of luminous vibrations may afford some information regarding the properties of the neutral electric medium." He then refers to Ampère's own suggestion in this direction. ('Electrodynamische Maasbestimmungen,' Part I., p. 169.)

of confirmation occupied a long period, during which it became more and more satisfactory and complete. In fact, so great has the coincidence of calculation with observation turned out to be, in all problems of physical astronomy, that no astronomer at the end of this century doubts that the gravitation formula alone will suffice to explain all anomalies which still exist in great number in the movements of cosmic bodies—such, for instance, as the moon.

40. The Newtonian formula the basis of physical astronomy.

Moreover, in the whole wide range of physical and chemical, not to speak of other natural phenomena, there is probably no instance of a simple mathematical relation having been applied to so large a field of facts, found so trustworthy a guide, and been so unfailingly verified.

And yet the very extent of this field must not blind us to the fact that for the explanation of molecular[1]

[1] This is indeed not to be wondered at when we consider that in all molecular and molar phenomena such a variety of elements and forces come into play that it is impossible to isolate any special quantities as we do when from the cosmic point of view we lose sight of everything except mass, time, and distance—*i.e.*, the elementary factors of our system of measurement. In the phenomena of electricity, for instance, it is merely by a process of mental abstraction, which has no counterpart in the observable phenomena, that we speak of electrical masses, be they one or two; of fluids; of elements of currents, which in nature cannot exist alone; of velocities of a something which as yet cannot be clearly defined. Any mathematical formula can under such conditions be merely tentative, and the preciseness of it must not hide from us the fact that it is based upon hypothetical relations and artificial definitions. This was, for the gain of scientific thought, very clearly brought out in the theoretical discussions which followed upon Helmholtz's critical examination of Weber's and kindred formulæ, and is well expressed by Carl Neumann: "Electrical matters"—if such there be—"never exists alone, but only in combination with ponderable matter." Any law like that of Weber can therefore be merely a "particular," not a "fundamental" or "universal" law, for it refers merely to a small portion of the properties, forces, and relations of electric and ponderable matter, leaving others—as, for instance, those between electricity and heat, electricity and light,

phenomena, or even for such processes as happen continually under our eyes and our hands, this universal law of gravitation has practically done nothing. The action of gravitation alone between masses which we can manipulate directly is so weak that it takes the very finest instruments to detect it at all, and at molecular distances it is so immeasurably small that it is hardly conceivable how it can explain the existence of those enormous forces with which we here have to deal.[1] If

&c.—more or less in the dark (see 'Mathematische Annalen,' vol. xi. p. 323). From a philosophical point of view these discussions, in which many other eminent leaders of scientific thought took part, are of great interest and importance, as they bear upon the value of mathematical formulæ in physical research, upon the definition of laws of nature, the extent of their applicability, the correct lines of future research, the use of analogies in the formation of physical theories, &c. I therefore refer here to the literature of the subject: Tait, 'Sketch of Thermodynamics' (1868, pp. 57, 76); Thomson and Tait, 'Natural Philosophy' (1st ed., p. 311); Carl Neumann, 'Die Principien der Electrodynamik' (Tübingen, 1868); Helmholtz in various memoirs from 1872 onwards, all collected in 'Wissenschaftliche Abhandlungen' (vol. i. pp. 545, 636, 774, &c.) and in 'Vorträge und Reden' (vol. ii. Faraday Lecture); Carl Neumann, 'Mathematische Annalen' (vol. xi. p. 318). See also Riecke on 'Wilhelm Weber' (Göttingen, 1892), and Clerk Maxwell, 'Electricity and Magnetism,' (vol. ii. last chapter); 'Elementary Treatise on Electricity' (p. 51).

[1] An interesting speculation as to whether the Newtonian formula of gravitation is capable of explaining cohesion and capillary attraction will be found in Thomson's (Lord Kelvin's) paper to the Royal Society of Edinburgh (1862), and in his lecture before the Royal Institution (1866), on Capillary Attraction, both reprinted in the first volume of 'Popular Lectures and Addresses.' He there shows that if we combine Newton's law with the assumption of an ultimate heterogeneousness of matter,—as is demanded in the so-called atomic theory used in chemistry,—the mass of ultimate portions of matter at vanishing distances, or what is called in contact, may give rise to molecular forces of attraction of any magnitude; since the Newtonian attraction depends on two data—the distance and the density (or mass) of attracting particles. He concludes by saying that "it is satisfactory to find that, so far as cohesion is concerned, no other force than that of gravitation need be assumed" (p. 63). It does not seem that this view, which was also held by Sir John Herschel, is generally adopted by physicists (see Todhunter and Pearson, 'History of the Theory of Elasticity,' vol. i. p. 418, &c.; vol. ii. art. 1650). Another interesting speculation arose out of the discussion over Weber's law. One of the objections started by Helmholtz against Weber's law was that, under certain conditions,

for the purpose of discovering the forces which exist in the universe between cosmic bodies we had been confined to experiments in the laboratory, as we are in all other departments of physics and chemistry, it is very doubtful whether this universal law of gravitation would ever have been discovered. And yet it stands there as almost the only formula universally applicable to all matter throughout the visible and tangible universe.

41. The Newtonian formula unique as to universality and accuracy.

In the foregoing pages I have sometimes spoken of this great discovery of Newton, on which is based the astronomical view of nature, as a formula, sometimes as a law. A formula is merely the expression in definite terms of certain relations of measurable quantities. By a law we are apt to understand something more—*viz.*, the statement of some fundamental, all-pervading property of the things of nature, which, so far as we are concerned, is final.[1] Whether the human mind is at all

this expression would give an infinite value for the force between electrical particles in motion. Weber replied that the same argument could be used against the gravitation formula, and hinted at the possibility that a correction might have to be added to the Newtonian formula to make it applicable to molecular distances ('Electrodyn. Maasb.,' 1871, p. 60). This idea was taken up by several Continental mathematicians (see Isenkrahe, 'Das Räthsel von der Schwerkraft,' p. 33, &c.; Paul du Bois-Reymond, 'Ueber die Grundlagen der Erkenntniss,' p. 50; Tisserand, 'Comptes Rendus,' September 1872).

[1] Helmholtz says, referring to Weber's so-called law: "If we are to consider Weber's law as an elementary law, as an expression of the ultimate cause of the phenomena to which it refers, and not merely as an approximately correct expression of facts within narrow limits, then we must demand that, if applied to objects of the largest imaginable dimensions, it should give results which are physically possible" (1873, 'Wissenschaftliche Abhandlungen,' vol. i. p. 658). This sentence raises a philosophical question as to the demands which we can legitimately expect to be satisfied by any so-called law of nature expressible in the symbols of human thought, be these words or algebraic signs. I venture to think that nowadays, and largely in consequence of discussions similar to those carried on over Weber's law, physicists do not any longer expect to find laws of that general and fundamental character which the words given above describe.

capable of finding out the ultimate properties of things, is a question which has been answered in opposite ways. But whatever the answer may be to this philosophical question, the further and more modest question can be raised, Does the gravitation formula express one of those universal facts which we have to accept as final, beyond or behind which we cannot penetrate? Opposite answers have been given to this question. But it stands very much in the same position in which Laplace left it when he said:[1] "The extreme difficulty of the problem referring to the system of the universe obliges us to have recourse to approximations, which leave room for the fear that the neglected quantities may have a sensible influence on the results. As soon as mathematicians by observation became aware of this influence they returned to their analysis: by rectifying the same they have always found the cause of the observed anomalies; they have determined the laws of these, and frequently they have outrun observation by discovering irregularities which had not yet been observed. The lunar theory, the theory of Saturn, of Jupiter and his satellites, offer many examples of this kind.[2] Thus we may say that nature herself has helped in perfecting the astronomical theories founded upon the

42. Is the Newtonian formula an ultimate law?

43. Laplace's opinion.

[1] Exposition du Système du Monde,' 6th ed., p. 318.

[2] Tisserand, in discussing the difficulties which still beset the lunar theory, and after referring to the "prix Damoiseau" offered by the Academy of Sciences for an essay on this subject, says ('Bulletin astronomique,' 1891, vol. viii. p. 501): "La théorie de la lune se trouve arrêtée par la difficulté que nous venons de développer; déjà à l'époque de Clairaut la gravitation universelle paraissait impuissante à expliquer le mouvement du périgée; elle triomphera encore du nouvel obstacle qui se présente aujourd'hui, mais il reste à faire une belle découverte."

principle of universal gravitation. This is, in my opinion, one of the greatest proofs of the truth of this admirable principle. As to this principle, is it a primordial law of nature? Is it only a general effect of an unknown cause? Here the ignorance in which we are as to the ultimate properties of matter stops us, and removes all hope that we shall ever be able to answer these questions in a satisfactory manner."

In the meantime, as I have tried to show, the clue afforded by this principle has led physicists by strict analysis, by observation, by cleverly arranged experiments as well as by guesses drawn from analogy, to the discovery of many unknown phenomena, to the fixing in mathematical language of interesting relations, and in general to a large extension of the field of natural knowledge. No wonder that a principle which has done, and is still doing, such valuable service in physical astronomy should have done much to establish the astronomical view of nature.[1] As one of the latest representatives of physical science abroad has said, "The present generation

[1] This view was concisely put by Poisson at a time when the corpuscular theory of the imponderables —light, heat, and electricity—still reigned supreme in the Continental school: "Toutes les parties de la matière sont soumises à deux sortes d'actions mutuelles. L'une est attractive, indépendante de la nature des corps, proportionnelledu produit des masses, et en raison inverse du carré des distances: elle s'étend indéfiniment dans l'espace, et produit la pesanteur universelle et tous les phénomènes d'équilibre et du mouvement qui sont du ressort de la mécanique céleste. L'autre est attractive et répulsive; elle dépend de la nature des particules et de leur quantité de chaleur; son intensité décroit très rapidement quand la distance augmente, et devient insensible, dès que la distance a acquis une grandeur sensible" ('Journal de l'École polytechnique,' cahier xx, p. 4, 1831). See also Clerk Maxwell, 'On the Equilibrium of ElasticSolids' (1850, reprinted in 'Scientific Papers,' vol. i. p. 30), where a similar assumption is stated as the basis of the mathematical theories of Navier, Poisson, Lamé, and Clapeyron.

is still more or less accustomed to think in the manner of Newton's view of nature, in which the supposition of forces acting at a distance appears as the most simple view: we feel it difficult to step out of this circle of ideas."[1] Nevertheless, the country itself which produced

[1] Kundt, 'Die neuere Entwicklung der Electricitätslehre' (Berlin, 1891, p. 35). This habit is probably more marked on the Continent than in England. In this country the later developments of Laplace's astronomical view of nature have remained unknown except to a few scientific specialists. Through Faraday's influence, and in consequence of the backwardness which the English school of science exhibited early in the century in assimilating Continental ideas (see p. 232, note), theoretical views on electricity as well as on other forms of energy were formed and taught more in conformity with experimental observation. I am not aware that Weber's theory was expounded in any English text-book or handbook before Maxwell referred to it as the view to which Faraday and he himself were opposed. In fact, the astronomical view of molecular physics is almost entirely of foreign growth. In England "action at a distance" is now stigmatised as a pernicious heresy (Tait, 'Properties of Matter,' 2nd ed., 1890, Introduction) or as unthinkable (O. Lodge, 'Modern Views of Electricity,' 1892, p. 386, &c.) Abroad weighty authorities have pronounced against the astronomical view of nature as final or even helpful in the present stage of physical and chemical science. Helmholtz, who was trained in it, gradually emancipated himself, probably under the influence of physiological studies; so did Kirchhoff, who in his lectures on Electricity (edited by Planck, 1891) hardly mentions Weber's law, though he had previously, in 1857, based an elaborate and valuable investigation upon it ('Ueber die Bewegung der Electricität in Drähten,' 'Gesammelte Abhandlungen,' p. 131, &c.) Still more marked is the aversion to the attitude or habit of thought which belongs to the astronomical view of nature on the part of those who approached physical problems from the side of chemistry. Hittorf (quoted by Lehmann, 'Molecularphysik,' vol. ii. p. 456) explains the opposition of Berzelius to Faraday's electrolytic law and to his other results from the fact that they stood in direct opposition to that view "which at the end of the last century had been introduced into chemistry through the success of Newton's law in astronomy, and under the influence of Laplace on Lavoisier and Berthollet," and sees the importance of his own laborious researches in the demonstration "that the mysterious potential energy cannot in the case of uncombined chemical substances be explained by the work of attractive forces," and "that a confession of ignorance in such matters is more conducive to progress than the assertion that every process in nature is essentially a phenomenon of attraction in the Newtonian sense." Of Ostwald's endeavours to liberate theoretical views in chemistry from the tyranny of the older hypotheses I shall have frequent occasion to speak. His discourse 'Die Energie und ihre Wandlungen' (Leipzig, 1888) contains an expression of opinion similar to those quoted here.

44. Opposition to the astronomical view of nature.

the author of this the astronomical view of nature has also been the birthplace of a different manner of regarding physical phenomena. It will be the object of a future chapter to trace the origin and growth of what I propose to call the physical view of nature. We shall then learn how the germs of this different view can be traced even in the writings of Newton. But before I take up this subject I must deal with another and independent way of regarding nature which very largely supplemented the astronomical view. If the Newtonian gravitation formula is the basis and principle of physical astronomy—of our knowledge of cosmic phenomena—the view I am now going to explain has been equally useful in building up another most important science of modern times—the science of chemistry.

CHAPTER V.

THE ATOMIC VIEW OF NATURE.

1. Recapitulation.

In the last chapter I have shown how, under the influence of the Newtonian philosophy, the ancient but indefinite ideas of Attraction and Repulsion acquired a definite meaning, and how—at least so far as cosmical phenomena are concerned—the Newtonian Gravitation formula was made the foundation of very successful explanations[1]

[1] I use the word explanation in conformity with the popularly accepted meaning of the term. It is, however, well to remark here that, in the course of our century and greatly owing to the influence of the exact scientific spirit, a change is being gradually introduced into language, which will assist in conveying more correct views as to the objects of science. In England the metaphysical interest has been so long banished from scientific literature, the part also which experiment and observation have played has been so great, that misunderstandings as to the real objects of science have been less frequent than abroad, especially in Germany, where the metaphysical or philosophical interest still largely pervades scientific literature, though metaphysics themselves may be on the decline. There the definition of the science of mechanics (now more usually termed dynamics in this country), given by Kirchhoff in his 'Vorlesungen über mathematische Physik' (vol. i. p. 1), has marked quite an epoch in the philosophy of the exact sciences. This definition is as follows: "Mechanics is the science of motion; we can assign as its object: to describe completely and in the simplest manner the motions which occur in nature." Inasmuch as a large school of natural philosophers consider that it is the object of all exact sciences to give a mechanical explanation of natural phenomena, it would follow that the object of all science is to reduce the phenomena of nature to forms of motion, and to describe these completely and in the simplest manner. We may feel some reluctance in assenting at once to this definition. Still an analysis of

of nature. Towards the end of the last century, and all through the present one, this view of things natural, which I have called the Astronomical view, has exerted a great fascination over scientific minds: especially in the mathematical schools of France and the Continent it has been a leading idea in scientific thought. It has been extended into molar and molecular physics, and has in these led to some very extraordinary and ingenious theories. In England, this astronomical view of Nature has, in the course of the present century, been received

what has been done since Newton in real science will probably convince us that the definition is safe and sufficient. It means the analysis of phenomena as to their appearance in space and their sequence in time. Both can, in consequence of the small number of elementary relations on which arithmetic, geometry, and dynamics are built up, be reduced to—or described in—a small number of elementary terms or conceptions, the alphabet of all science. To show how in every instance the terms of this alphabet are to be put together, in order to correspond to any phenomenon, is all the explanation we can give. Objections have been raised to Kirchhoff's definition by Du Bois-Reymond ("Göthe und kein Ende," in 'Reden,' vol. i. p. 434), inasmuch as it does not define the difference between the descriptive (historical) and the exact (mathematical) sciences of nature; but the difference is really maintained if we demand a complete description. Natural history only affords an incomplete description. The only complete description is that afforded by a mathematical formula in which the constants are supplied by observation. This permits us to calculate those features or phases of phenomena which are hidden from our observation in space or in time. An objection to the view which identifies physics with mechanics, seems implied in Mach's remarks contained in the last chapter of his very thoughtful book 'Die Mechanik in ihrer Entwickelung' (Leipzig, 1889). According to his view, the aim of exact science is not necessarily to give mechanical explanations or descriptions of phenomena, inasmuch as temperature, electric potential, &c., are just as simple elements of natural phenomena as mass and motion. It seems, nevertheless, that exact measurements are only possible in the data of time and space. Assuming that a complete and simple description—admitting of calculation—is the aim of all exact science, it is evident how much and how little we may expect from science. We shall not expect to find the ultimate and final causes, and science will not teach us to understand nature and life. The search after ultimate causes may perhaps be given up as hopeless; that after the meaning and significance of the things of life will never be abandoned: it is the philosophical or religious problem.

with less favour, although it was entirely owing to Newton's gravitation formula that it ever obtained its great influence, the labour of Continental men of science being very largely spent in two directions: first, in drawing the purely mathematical consequences of Newton's formula—in this they have met with increasing success, unparalleled by that in any other domain of science; and secondly, in extending the principle of Newton, by experiment and analogy, into other departments. In some of these, very remarkable results have been achieved; but nevertheless at the end of the century no extension or analogue of the Newtonian gravitation formula has been generally accepted, and it still stands there as almost the only firmly established mathematical relation, expressive of a property of all matter, to which the progress of more than two centuries has added nothing, from which it has taken nothing away. The value, however, of all those partial attempts in another direction has been enormous; for with the aim of applying, extending, or modifying a rigorous mathematical formula, those philosophers have carried out a series of the most exact observations and measurements of physical quantities, very greatly extended our knowledge of natural phenomena and their mutual relations, and founded that general system of physical measurement which is now universally adopted. The names of Gauss and Weber stand out prominently as leaders in this work. I shall have to come back to this point later on, after I have shown that other views of nature besides the astronomical have also led up to it, and placed it in similar prominence.

Chemistry

About a century after the publication of the 'Principia,' which, by propounding the gravitation formula, raised the ancient and indefinite notion of Attraction to the rank of a useful and rigorously defined expression, another favourite theory of the ancient philosophers[1] was similarly elevated to the rank of a leading and useful scientific idea.

Although no mathematical relation equal in value and definiteness to the gravitation formula marks the introduction of the Atomic theory in Chemistry, it nevertheless owes its success to similar qualities—*viz.*, to the fact that it led natural philosophers to make definite measurements, and put exact research in the place of vague reasoning. 2. Atomic theory.

The atomic theory, usually associated with the name of Dalton, is, however, not nearly as much the historic property of that great man as gravitation is that of Newton, for whereas the latter gave the fullest generalisation that can so far be safely made, the atomic

[1] Ancient philosophers have furnished us with three distinct abstractions which have survived, and which, put into definite mathematical language, have led exact research in physics and chemistry in modern times — the theory of Attraction and Repulsion, the Atomic Theory, and the Kinetic Theory, or the notion that everything is motion. Of these three theories the second was most developed in antiquity; Lucretius's great poem on the nature of things being really a treatise on the subject, in which the atomic view is placed in the centre, the two other ideas being likewise largely utilised. The historians of ancient philosophy trace these abstract or leading ideas back to the earlier Greek thinkers. Thus Heraclitus of Ephesus is credited with having first taught that everything is in motion. Empedocles of Agrigentum made use of the notions of Attraction and Repulsion, poetically represented as Love and Hatred, to explain the action of his elements; and Democritus of Abdera is universally considered to be the true founder of the atomistic theory, which was adopted and developed in the School of Epicurus, and very fully explained by the Roman poet. A very good analysis will be found in Lange's 'History of Materialism' (English translation by Thomas, 3 vols.), in which also the historical connection with modern thought, especially through Bacon, Gassendi, and Hobbes, is clearly brought out.

theory has been gradually defined and variously modified in the course of this century, and is still in a somewhat unstable condition. We are also bound to attach the greatest importance to the preliminary step taken by Lavoisier, who is even more justly called the father of modern chemistry than Kepler is called the father of modern astronomy.

3. Lavoisier.

The exact claims of Lavoisier to this important place in the history of chemistry have been variously stated:[1]

[1] Continental writers are pretty unanimous in dating modern chemistry from the time of Lavoisier (1743-1794). In this country there has been less unanimity, the names of Black, of Cavendish, of Priestley, even of Robert Boyle, having occasionally been put forward. The fact that Lavoisier did not sufficiently acknowledge his indebtedness to some of his English contemporaries has given occasion in some quarters to depreciation of his merits. It cannot be upheld that he was the first formally to express the doctrine of the indestructibility or conservation of matter, as this idea underlay many experimental researches before his time; nor that he was the first to refer to the balance as the ultimate test of chemical facts. The assertion that he first introduced the idea of two different kinds of matter, ponderable and imponderable, is also questionable, and still more so his claim to having discovered oxygen, the composition of water and of atmospheric air, the combustibility of the diamond, and other special facts. His fame rests upon a much broader basis, and has been most clearly investigated and settled by Hermann Kopp in his 'Entwickelung der Chemie in der neueren Zeit' (München, 1873). In this excellent work the author somewhat modifies the view he took in his earlier 'Geschichte der Chemie' (Braunschweig, 1843, especially vol. i. p. 274, &c.), and sums up Lavoisier's merit in the following words (p. 145): "His contemporaries could dispose of the same inherited and much new material, but not one of them understood how to build up out of this material and his own independent researches a chemical system, the reception of which should form the starting-point for all future improvement of this science. Lavoisier has the whole merit of having achieved this. He added to his own recognition of the correct views the work of procuring recognition for them from others. He imparted his own matured views to those who represented chemistry at the end of the last century. . . . We must measure his greatness not merely by his own insight but also by the resistance which he had to overcome in other chemists who clung to the older theory. These achievements are great enough not to require the exaggeration with which they have occasionally been announced, and not to be touched by attempts on the other side to minimise them."

there is however no difference of opinion on this point, that since his time, and greatly through his labours, the quantitative method has been established as the ultimate test of chemical facts; the principle of this method being the rule that in all changes of combination and reaction, the total weight of the various ingredients—be they elementary bodies or compounds—remains unchanged. The science of chemistry was thus established upon an exact, a mathematical basis. By means of this method Lavoisier, utilising and analysing the results gained by himself and others before him, notably those of Priestley, Cavendish, and Black, succeeded in destroying the older theory of combustion, the so-called phlogistic theory.[1] From a

[1] This result was announced in 1777 to the Paris Academy, and the demonstration completed in a memoir of 1783. "He closes this latter memoir with the expression, that his object had been to bring forward new proofs of his theory of combustion of 1777, and to prove that Stahl's phlogiston was something purely imaginary,—that without it facts could be more easily and more simply explained than with it; he did not expect that his views would be at once accepted, . . . time would have to confirm or to reject the opinions he had developed, but already he recognised with satisfaction that unprejudiced students of the science, unbiassed mathematicians and physicists, believed no longer in phlogiston as Stahl viewed it, and that they considered the whole doctrine more as a hindrance than as a helpful scaffolding in erecting the edifice of science" (Kopp, 'Entwickelung,' p. 202). This and the further remark of Kopp that it was the mathematicians who took up Lavoisier's views (see *supra*, p. 115, note 2) are significant signs of the introduction of the mathematical, the measuring, spirit into chemistry. Few ideas which once exerted so great and lasting an influence on science as that of phlogiston, have so entirely disappeared from our text-books, and it is interesting to note that those whose researches were guided by it were not so far from grasping a valuable truth as has been supposed. This theory, elaborated by Stahl, a contemporary of Newton and Leibniz (1660-1734), was the first attempt to co-ordinate a great mass of observations, to bring the phenomena of chemical change under one common principle. Phlogiston was the thing the migration of which gave rise to chemical change, and as the most obvious changes were exhibited in the processes of combustion, "Phlogiston" or "Brennstoff" was the name which suggested itself as most suitable for this principle. Chemical changes were not to be measured so much by the resulting change of weight as by the readiness with which

scientific point of view, the principal defect in this theory was, that its explanations could not be subjected to any strict and exact numerical verification. Whenever an element enters into our operations which has either no weight or a negative weight, and thus evades exact determination and control, explanations and observations become vague and uncertain.

In the time of Lavoisier, and pre-eminently through his exertions, this vague and unmeasurable principle phlogiston was eliminated from the laboratory and the text-books: quantities took the place of indefinable qualities, and numerical determinations increased in frequency and accuracy. The vague phlogistic theory, which contained a germ of truth, but one which at that time could not be put into definite terms, had helped to gather up many valuable facts and observations: these were collected and restated in a new and precise language. It has been said that every science must pass through three periods of development. The first is that of presentiment, or of faith; the second is that of sophistry; and the third is that of sober research. Liebig states the case somewhat

4. Phlogistic theory.

substances enter into chemical reaction; and the mobility or inertness of chemical substances was to be measured by the presence or absence of a definite something. A hundred and fifty years after Stahl, science had so far advanced, that besides the change of weight or mass, the change of the power of entering into chemical combination could also be measured, and the term "potential energy" was introduced to describe many of those properties and processes which Stahl had fastened upon, when he, as the pioneer, undertook to co-ordinate chemical phenomena. If Stahl considered phlogiston to be a substance, though he did not inquire into its mass or ponderable property, the question might be put again, whether "energy" is not to be considered after all as a substance. Cf. Tait, 'Properties of Matter' (2nd ed., introduction, especially p. 5 *sqq.*); 'Recent Advances of Science,' introduction; also Clerk Maxwell, 'Electricity and Magnetism' (last chapter); Ostwald, 'Chemische Energie' (Leipzig, 1893, p. 41).

more correctly when he says: "To investigate the essence of a natural phenomenon, three conditions are necessary: We must first study and know the phenomenon itself, from all sides; we must then determine in what relation it stands to other natural phenomena; and lastly, when we have ascertained all these relations, we have to solve the problem of measuring these relations and the laws of mutual dependence—that is, of expressing them in numbers. In the first period of chemistry, all the powers of men's minds were devoted to acquiring a knowledge of the properties of bodies; it was necessary to discover, observe, and ascertain their peculiarities. This is the alchemistical period. The second period embraces the determination of the mutual relations or connections of these properties; this is the period of phlogistic chemistry. In the third period, in which we now are, we ascertain by weight and measure and express in numbers the degree in which the properties of bodies are mutually dependent. The inductive sciences begin with the substance itself, then come just ideas, and lastly, mathematics are called in, and, with the aid of numbers, complete the work." [1]

As Galileo, Huygens, and Newton, by a series of brilliant investigations and theories, such as those of the pendulum, the fall of bodies, finally of universal gravitation, established the usefulness of the mathematical treatment of physical phenomena, so Lavoisier and his school proved the correctness and usefulness of their views by the new theory of combustion, as consisting in the combination of a special body or element called oxygen with other bodies 5. Theory of combustion

[1] 'Familiar Letters on Chemistry,' translated by Blyth, 4th ed., London, 1859, p. 60.

or elements. A very large field of research—all on the lines pointed out by the new school—was opened out. But the age for a further application of mathematical reasoning came much more slowly in chemistry than in physical science.

The latter had at least one great department, in which a small number of factors, all admitting of mathematical accuracy—those of distance, mass, and motion—sufficed to explain the phenomena, at least if viewed from a great distance. This science is the physics of the heavens, the science of cosmic phenomena. On this earth—in physical and still more in chemical phenomena—the matter stood very differently. Here we have not to deal with a few measurable quantities only. A large number of elements or factors, of which only very few can be accurately measured, combine to make up what we called in the last chapter molar and molecular phenomena. In the study of inanimate nature, astronomy—the mechanics of the heavens—deals with the simplest relations; chemistry—the science of the changes which bodies undergo when being combined or separated—deals with the most complicated side of reality. Physics occupy an intermediate position, and thus we can also trace in the history of physical research the twofold influence of the astronomical method of inquiry on one side, and the chemical on the other.

But the general rule, that in chemical changes the weight of all the constituents put together never changes, was not the only numerical relation which came to the aid of students of nature, when they, at the end of the last century, betook themselves to exact measurements and

determinations. That rule is indeed the foundation of all work in the laboratory, the principle which decides the degree of accuracy attained in every analysis, and which not infrequently is the only method of determining the presence of some undiscovered constituent.[1] Not long

[1] The revolution in chemistry at the end of the last century manifests itself in nothing more than in the various distinct problems, corresponding to different courses of scientific thought and different interests, which have guided chemical research since that time. The first definite object was the search after the real elements, the attempt to decompose the existing substances of nature into their ultimate constituents. This interesting occupation somewhat pushed into the background the theoretical investigations regarding the forms of the combinations of the various elements into compounds, still more the study of chemical affinity. A second definite object was the development of the theory of combustion which Lavoisier propounded, and the confirmation or refutation of the idea according to which oxygen occupied almost as important a position in chemical reactions as phlogiston had done before. A third definite object was the development of analytical chemistry, the systematic and methodical use of the balance. So far as the first branch of this pursuit was concerned, Lavoisier's catalogue of the elements was still very incomplete; it contained thirty-three members, including light and heat, and twenty-three of the substances which now figure in the list of the seventy elements enumerated in the text-books; the alkalies and earths were still considered to be simple bodies. A great addition to our knowledge in this department came through Davy's decomposition of soda and potash. And after his proof of the elementary nature of chlorine the oxygen theory of Lavoisier had also to be greatly modified. "Through a series of most important investigations, he rose in the beginning of this century to such eminence, that he was then considered to be the first representative of chemical science. With great experimental ability he combined a singular freedom from all the theoretical doctrines which were recognised in his age" (Kopp, 'Entwickelung der Chemie,' p. 451). In this he resembled Dalton and Faraday and other natural philosophers in this country, on whom theoretical notions formed in the Continental schools had little or no influence. Qualitative analysis was less indebted to Lavoisier than other branches of the science were. In fact, it was more at home in Sweden and Germany, where the interests of mineralogy and metallurgy promoted it. Bergmann and Scheele in Sweden, Klaproth in Berlin, were the forerunners of Berzelius and of the Berlin school of analysts. In this country Black and especially Cavendish had carried out some important quantitative determinations, the accuracy of which seems very far behind modern standards (see Kopp, 'Geschichte der Chemie,' vol. ii. p. 70, &c., 1844). It was the introduction of the notion of chemical equivalence, a term used already by Cavendish, which furnished the ultimate test for accuracy and revolutionised quantitative analysis.

6. Rule of fixed proportions.

before the age of Lavoisier, another general conception had been introduced into chemical research; this was the rule of definite proportions—*i.e.*, the fact that substances, whether simple or compound, combine only in definite proportions of their weight, and that the numbers marking these proportions are characteristic of every definite chemical substance. It took some time, nearly a century, before this idea, which arose through the examination of neutral salts and the determination of the quantities of acids and alkalies which were wanted to effect mutual saturation, became clear; before the rule of definite proportions was generally established, becoming a guide for chemical analysis. It is interesting to note how the vaguer terms of chemical affinity and elective attraction, of chemical action, of adhesion and elasticity—mostly borrowed from other departments of science where they had definite meanings—gradually disappeared, when by the aid of the chemical balance each simple substance and each definite compound began to be characterised, and labelled with a fixed number. Nevertheless, even at the beginning of this century, eminent chemists were still so much engaged in discussing the rival claims of the old phlogistic, and the modern theory of combustion, of Berthollet's chemical equilibrium, of the so-called dynamical and the electro-chemical views of phenomena, that the first methodical attempt actually to fix these numbers—*i.e.*, to give a table of chemical equivalents—remained unnoticed.[1]

[1] The history of chemistry early in this century furnishes a good example of the sway which theoretical views exercised over the minds of investigators. Berthollet, who began by critically examining Bergmann's doctrine of chemical affinities, was evidently much influenced by the mathematical theory of attraction, and by the mechanical laws of equilibrium, which formed so prominent a subject of investigation in the writings of Laplace and his school. Chemical affinity was to be coordinated with what he called astronomical attraction; both were to be ultimately the same physical property; they acted differently, because in the case of gravitation the dimensions were so large, that the form, distances, and peculiar properties of the molecules had no influence. It was an attempt to introduce the astronomical view of matter into molecular physics, and to base chemistry upon this view. Berthollet adhered to the corpuscular theory of heat against Rumford, who had just propounded his opinion that heat is not a constituent part of bodies; and he maintained that chemical affinity was a function of the mass of bodies as was astronomical attraction. The germ of truth in Berthollet's views, which were approved by Laplace, but cast into oblivion under the influence of Proust and Richter's theory of fixed proportions, has in recent times been shown by Lothar Meyer ('Modern Theories of Chemistry,' Introduction), and by Ostwald ('Allgemeine Chemie,' vol. ii. p. 557, 1st ed., also 'Die Energie und ihre Wandlungen,' Leipzig, 1888, p. 20). If the astronomical view of molecular phenomena prevented Berthollet from accepting Proust's doctrine of fixed proportions and definite combinations, Richter injured his own reputation by adhering to the nomenclature of the phlogiston theory after it had been discarded by French chemists, and in Germany after Klaproth's determinations in 1792. The oxygen theory of combustion of Lavoisier got such a firm hold on the minds of Continental chemists that the labours of those who, like Cavendish in England and Richter in Germany, put forward important discoveries in the language and on the principles of the older theory, were temporarily forgotten. See Kopp, 'Entwickelung der Chemie,' p. 271, &c.

The merit of having made this attempt belongs to one who approached chemistry entirely from the mathematical side, who wrote the first chemical book with a title pointing directly to measurements, but who perhaps spoilt his work by giving way to the fascination which regular numerical and geometrical arrangements have again and again exercised over philosophical inquirers. Jeremias Benjamin Richter—a name possessed of no popular celebrity—published in 1792 to 1794, in three parts, his "Stœchiometry, or the art of measuring chemical elements."[1] From his data, Fischer calculated in 1802 the

7. J. Benjamin Richter.

[1] Stœchiometry comes from the Greek τὰ στοιχεῖα, the constituent parts, and μετρεῖν, to measure. All Richter's works are connected with the application of mathematics to chemistry; his inaugural dissertation, which appeared in 1789, bearing the title 'de usu matheseos in chymia' (Kopp, 'Geschichte der Chemie,' vol. ii. p. 350). "Richter était préoccupé de l'idée d'appliquer les mathématiques à la chimie, et en

first table of chemical equivalents, taking sulphuric acid as the standard with the figure 1000.

The conviction that chemical substances combine according to fixed and simple proportions gained ground on the Continent, chiefly during the discussion in which Proust finally disproved and defeated Berthollet's theory
8. Dalton. of chemical affinity; but it is to Dalton that the doctrine of fixed and multiple proportions is indebted for a consistent exposition. Dalton based it upon a mental representation which ever since has been the soul of all chemical reasoning.

When Newton, from the measurable data of the movements of cosmic bodies, deduced the celebrated gravitation formula, he had to descend to molar—nay, even to molecular—dimensions, and to express it as a relation referring to the very elements of matter, before he could apply it in a useful manner: he had to express it as a formula which had reference to the smallest portions of matter. In the same way, the measurements made by

particulier de découvrir des relations numériques entre les quantités des corps qui se combinent. Ses efforts, dans cette direction, n'ont pas été également heureux ; car, s'il a reconnu et énoncé le premier la loi de proportionnalité entre les quantités de bases qui s'unissent au même poids d'acide et entre les quantités d'acides qui s'unissent au même poids de base, fait important et exact, il a cherché à démontrer, d'un autre côté, que ces quantités fermaient des séries numériques dont les termes augmentent suivant des relations simples, ce qui est erroné. . . . Ces erreurs n'ont pas échappé, sans doute, à l'attention des contemporains de Richter et ont contribué à discréditer ses travaux. . . . Mais nous n'avons pas à insister sur ce dernier point. Relevons, dans l'œuvre de Richter, les idées justes et les découvertes fondamentales qui recommandent d'autant plus son nom à l'attention reconnaissante de la postérité qu'il est demeuré méconnu et presque ignoré de son temps" (Wurtz, 'La Théorie atomique," 7me ed., 1893, p. 9, &c.) "L'opposition même, qu'il professait pour les doctrines du réformateur [Lavoisier] semble avoir contribué à discréditer les travaux de Richter : son heure n'était pas venue ; l'intérêt était ailleurs, et en Allemagne, comme en France et en Angleterre, les esprits étaient entraînés par le courant des idées nouvelles" (ibid., p. 13).

many chemists previous to Dalton had to be interpreted as referring not only to such quantities as the balance could determine, but to the very smallest immeasurable particles of which chemical substances consist. For this purpose Dalton adopted what was known as the atomic view of matter. The conception of matter as made up of independent particles, which for our means and methods prove not only indestructible but likewise indivisible, was revived as the ancient theory of attraction had been. Combined with the Newtonian view that weight is a universal property of all matter, it made the two fundamental rules of chemical action intelligible: the two facts—*first*, that the total weight of substances remains always the same, be they combined in ever so many different ways; and *secondly*, that all substances, be they in large or in small quantities, combine with each other, or separate from each other, in definite and fixed proportions. This view could not be consistently maintained, except it was referred to the smallest particles into which matter is practically divisible: the figures expressing the combining numbers were viewed by Dalton as representing the relative weights of the actual atoms or elements of matter. That the ultimate particles of matter have definite weights is the reason why substances combine in fixed proportions, and why the combining weight of the compound is the sum of the combining weights of the constituents.

As the gravitation formula had given rise to a surprising activity in physical astronomy, to a long series of exact measurements, and to theoretical deductions of a purely mathematical kind, so the atomic theory of Dalton

in the early years of the century fixed the task of chemists for a long time ahead.

To begin with, an enormous amount of work had to be done in determining the actual proportions in which elementary substances combine. A very large share of

9. Berzelius.

this work belongs to Berzelius, who by a great number of very accurate determinations confirmed inductively the correctness of Dalton's theory. And even more important than the conformation of the theory was the great harvest of actual knowledge of the things and processes of nature which was collaterally gathered, whilst chemists were trying to prove or to refute existing opinions.

Indeed, whilst the atomic theory of Dalton was the first step towards a systematic and comprehensive study of chemical phenomena—*i.e.*, of the qualitative varieties under which matter presents itself to us on the surface of this globe—the extension which was gained in the domain of actual facts was much greater than the simplification which the theory had attempted to give. The number of elements or simple bodies, which in Lavoisier's time hardly exceeded thirty, increased before the year 1830 to more than double: the number of new compounds, unknown before, has probably never been counted. Compared with this growth of actual knowledge of facts, the development of the theory was slow and uncertain. The view of nature from the atomic point of view marks indeed a great contrast to that from the astronomical

10. Atomic theory and gravitation compared.

point of view. We now live about as long after the reform of chemistry through Lavoisier and Dalton as Laplace lived after the reform of physical astronomy

through Newton. But who could compare the state of chemistry at the present day with that of astronomy in the age of Laplace? There, every step had tended to show that the one Newtonian formula sufficed to comprehend all cosmic phenomena; here, the simplification introduced by Dalton has had to give way to a series of modifications which have rendered the atomic theory one of the most complicated machineries ever introduced into science. Let us review in brief the fate of Dalton's hypothesis during the century which followed. Quite in the early years of the atomic theory, Wollaston propheti-

11. Wollaston's prophecy.

cally foretold that if once an accurate knowledge were gained of the relative weights of elementary atoms, philosophers would not rest satisfied with the determination of mere numbers, but would have to gain a geometrical conception of how the elementary particles were placed in space. Van't Hoff's 'La Chimie dans l'Espace'—published at Rotterdam in 1875—was the first practical realisation of this prophecy. Many stages had to be gone through before this latest phase of the atomic view was attained. Had it been the case that every elementary substance combines with any other substance only in one fixed numerical proportion, no necessity would have existed to look upon the atomic numbers as anything else than equivalents. But it was found that though the combining numbers were fixed they were not always the same; it was found that if a substance combined in two or more proportions with any other, the larger proportions were always exact multiples of the smallest proportion. And this—the rule or law of *multiple proportions*—was

12. **Rule of multiple proportions.**

exactly what gave to Dalton's view its great plausibility,[1] for if the elementary atom of each substance had a definite weight, it might be that not one atom only combined with one other, but that one combined with two, or two with three, and so on. Indeed it was soon found that this was

[1] The different factors of thought which combined to give the atomic theory that definiteness and usefulness which it attained through and since Dalton lay ready-made before him; but no one had seen so clearly as he did how to combine them. Proust had taught how to distinguish between chemical compounds and mixtures. When he prepared carbonate of copper artificially, he found that it had the same composition as the mineral which he found in nature. Richter had shown that definite proportions describe the quantities in which acids and bases exist in neutral salts. Fischer had attached to his translation of Berthollet's work the first table of equivalent quantities of bases and acids which combine to neutralise each other. Richter, and after him Gay-Lussac, had also found that the quantities of different metals which dissolve in the same quantity of acid to form saturated solutions combine also with the same weights of oxygen to form oxides. Richter, and after him Proust, had found that certain metals, like iron and mercury, form more than one fixed compound with oxygen, but without perceiving that the different quantities of oxygen in these fixed compounds stand in simple proportions to each other. So far as the theoretical side is concerned, the idea that bodies are formed of distinct particles—the notion of the ultimate heterogeneousness or discontinuity of matter—was not only familiar to the ancients, but was adopted by many physicists before Dalton; though the chemical specialists who prepared the way for Dalton do not seem to have made use of this idea. Boerhaave, and before him Boyle, had spoken of atoms and of the *massulæ* or particles. Theories were not wanting that these ultimate particles differed in size and form, nor the opposite view, that the particles which combined had the same weight. The latter was the view of Higgins, in the exposition of which (1790) he entangled himself in contradictions, losing his chance of being one of the founders of the atomic theory. As Wurtz and Kopp and others who have carefully investigated the rival claims have said: This honour of founding the atomic theory belongs undividedly to Dalton. It seems important to notice that his experiments with mixtures of gases, which must have begun about 1790, impressed upon him the idea that different gases could exist independently of each other in the same space, suggesting the conception that neither of them filled the whole space, but that they consisted of discontinuous particles. He himself refers to these first investigations as containing the germ of his later opinions. It must, however, be borne in mind that Dalton was only imperfectly acquainted with the writings of contemporary — especially Continental—writers, and that he had a wholesome distrust for statements of facts which he had not verified or observed himself. All this is very clearly stated in Kopp's 'Entwickelung der Chemie,' p. 285, &c.

actually the case. The lowest number according to which any substance entered into combination with any other was called the atomic weight or equivalent.

There was, so far, no necessity to look upon atomic weights as anything else than numbers fixing a proportion. The unit could be selected arbitrarily. It was not long before that element, hydrogen, which entered into compounds in the relatively smallest weight was taken as an arbitrary unit, and all other elements and compounds were tabulated according to the relative amount of their weights required to form compounds with hydrogen or with any other element—*e.g.*, oxygen—the equivalent of which with hydrogen was known.[1]

13. **Equivalents.**

[1] For many years after the enunciation of the atomic theory great uncertainty and much difference of opinion existed on this and other points. The man who did most to elaborate the edifice of which Dalton had laid the foundations, who filled in the outlines and invented the language of chemistry, was Berzelius. He proceeded inductively and gathered materials from all sides; to him are also owing the greatest number of accurate analyses, especially of inorganic substances. When he began his labours he was favourably disposed towards Dalton's hypothesis; he clearly saw its capabilities, but also that it was based only upon a happy suggestion, that it was introduced more by deductive than by inductive reasoning, and that it needed to be exhaustively tested and verified. After ten years, during which he published in Gilbert's 'Annalen' and in Thomson's 'Annals of Philosophy' many series of investigations, he was able in 1818 to publish, in his 'Essay on Chemical Proportions and on the Chemical Effects of Electricity' (French translation, 1819; German translation, 1820), the first systematic and complete exposition of the atomic theory. The beginning of a really exact treatment of chemistry has been dated by H. Rose, the greatest analytical chemist of the century, from this year 1818—the year in which Dalton's hypothesis was proved and generally accepted. Others have dated the beginning from 1808, when Dalton published his theory; others again from 1776, when Lavoisier destroyed the older phlogiston theory and appealed to the balance; others again from Black's discovery of latent heat in 1760. In an international history of thought it is not of much interest to decide whose claims to be the founder of modern chemistry as a science are best established. Every one of these dates marks an epoch in the advance of an important and independent branch of research. Black took an important step in the foundation of physical chemistry through his introduction of the conception of the quantity

A great door was now opened, not only for actual observation and research, but also for speculation—*i.e.*, for abstract thought. Some substances, if they entered into combination with hydrogen, required more than one unit of hydrogen, and it might therefore be that the proportion of the combining weight of hydrogen with any substance did not correctly give the atomic weight of the latter, but merely a multiple or sub-multiple of it. Thus, assuming oxygen combined with hydrogen in the proportion of 8 parts of the former to 1 part of the latter, a possibility was that the proportion might more correctly be written 16 to 2 than 8 to 1. Then, again, were the equivalent or atomic weights necessarily whole numbers? Were combinations all binary, such as acids and alkalies forming salts? and were more complex compounds resolvable into binary compounds of simpler binary compounds? Further, assuming the proportions fixing the combining weights to be known, how did the volumes of bodies combine?—was there a rule of volumes as there was of weights? and lastly, what was the reason or cause which made substances change their combinations, forming new ones, what did chemical affinity consist in, what did it depend on, how could it be defined and measured?

Considering that we have to do with a large number of independent, apparently unchangeable, elements, entering into many thousands of differing compounds, the task of

of heat. Lavoisier led the way in the development of the purely arithmetical department of chemistry, in the exclusive study of which physical chemistry was greatly neglected. Dalton suggested a formula which lent itself admirably to the representation of these purely arithmetical relations, and Berzelius elaborated this and invented a practical nomenclature. Black and Dalton threw out novel ideas; Lavoisier and Berzelius elaborated great systems and created great schools which numbered many converts and industrious workers.

the chemist was enormous, offering a large, almost limitless, field of research and speculation. Let us see under what leading ideas this knowledge has been arranged.

In the gradual development and clearer definition of these conceptions a general rule of thought seems to have unconsciously guided philosophers probably more than in any other department of knowledge. It is the rule of simplicity.[1] How the human mind should have 14. "Simplex sigillum veri."
arrived at the old formula of "simplex sigillum veri" is difficult to understand on any other ground than that of convenience and expediency. The prevailing impression, indeed, which the world of phenomena makes on the mind of an unbiassed observer must be the very reverse of simplicity or unity of law and purpose. That, nevertheless, the knowledge of some simple relations in time, number, and space would enable the human intellect to acquire a considerable insight into the course of events and the order of Nature's processes must have come to philosophers

[1] The progress of chemical theory is the history of the attempt to find simple relations of number and form, representing the countless combinations of elementary substances; and of the growing conviction that nearly every simplification must, in course of time, be abandoned. No formula remains unchallenged except the doctrine of fixed and fixed multiple proportions, and that only if we confine ourselves to solid compounds; but the proportions themselves are not accurately known, though no phenomenon exists which disproves the assumption that they are invariable. The original conception of the atom as a round hard body had to be abandoned for the more complicated notion of a molecule, an assemblage of atoms; the conception of elementary bodies had to be amplified by that of compound elements or radicles; the idea that the atomic weights were multiples of a lowest number had to be abandoned; the binary theory of the combination of bodies was replaced by the theory of radicles, of nuclei, of types; the simple nature of the elementary particles had to give way to a complicated atomicity, from which there had to be again distinguished the valency or capacity of saturation of the elementary constituents. It is a progress from simpler to more and more complex methods of representation.

as a kind of revelation, and it is not surprising that it came late in the course of civilisation.[1]

Nothing can have tended more in this direction than the success of the Newtonian gravitation formula, and of the simple laws of motion, which, at the time of the birth of modern chemistry, stood firmly established as the key to all problems of physical astronomy. No wonder that men were on the look-out for correspondingly simple—perhaps analogous—relations in the world of molecular phenomena. One of the earliest suggestions, which came forward soon after Dalton's atomic view had helped to establish the prevailing rule of fixed and of multiple proportions in the chemical combinations and reactions of matter, was the idea that, as to each element belonged a definite combining number, all these numbers must be the multiple of the lowest among them, the equivalent or atomic weight of hydrogen. This is Prout's celebrated hypothesis, which had some ardent admirers, and which has been repeatedly abandoned and revived in the course of this century.[2] It is hardly possible to maintain it any longer, since the accurate and elaborate measurements of

15. Prout's hypothesis.

[1] Except indeed the Pythagorean notions are regarded as an anticipation of it.

[2] The hypothesis of Prout, published anonymously in 1815, and warmly defended by Thomson, has been again and again revived. From the beginning it was put forward together with the suggestion that the different elementary substances might after all turn out to be all derived from one and the same primary form of matter, and that the atoms of this might in the atoms of our present elements merely be aggregated in different numbers and figures, held together by forces, which by the means and processes at our command could not be broken up. This primary substance might then be either hydrogen, the lightest in weight of known substances, or some other substance of which hydrogen itself was an atomic multiple. Abroad, Prout's hypothesis was disproved by Berzelius's accurate determinations, in England by Turner's, and about 1830 it fell into oblivion. It was again revived in 1840 by Dumas, who, as well as his followers, Laurent and Gerhardt, favoured the idea that the explanation of the different properties of chemical compounds, notably organic compounds, was to be found in the arrangement of the elementary atoms, in the structure rather than in the material difference of the elements themselves. The development of this view in the modern chemistry of "types" and "structures" will always go hand in hand with an avowed or tacit belief in the existence of an ultimate uniformity of substance, out of which by a diversity of configuration of atoms the infinite variety of compounds is produced. The accurate measurements of Stas had again about the year 1860 disproved the hypothesis of Prout. It has, however, again turned up in recent scientific literature. The theories of evolution, physical and philosophical, the discoveries of the spectroscope regarding the small number of elements contained in the photosphere of the sun, the periodic laws of Lothar Meyer and Mendeléeff and the stereometric theory of the carbon-compounds, of which I shall speak later on, all point to the conclusion that our so-called elements are composite bodies, and favour a view, similar to that of Prout, that possibly a single kind of matter may form the only substance of which atoms, molecules, elements, and compounds are made up. Professor Crookes in his address to the chemical section of the British Association in 1886 revived interest in the subject. After quoting a variety of authorities, he sums up: "From these passages, which might easily be multiplied, it plainly appears that the notion—not necessarily of the decomposibility, but at any rate of the complexibility of our supposed elements—is, so to speak, in the air of science, waiting to take a further and more definite development. It is important to keep before men's minds the idea of the genesis of the elements; this gives some form to our conceptions, and accustoms the mind to look for some physical production of atoms." Further on he coins the word "protyle" (from πρώτη and ὕλη) to denote the original kind of matter, and thus reminds us that, though speculations of this nature are not infrequent in English philosophy since Roger Bacon, the English language has no word to denote what the Germans call "Urstoff," the Romans "prima materia," the Greeks τὸ στοιχεῖον or simply ὕλη. The line of thought which again and again leads philosophers to speculate on this "prima materia" and upon a hypothesis similar to that of Prout is interesting and noteworthy, though it must be acknowledged that, so far, no real scientific benefit has been derived from it, and that it rather tends to upset the only firm foundation of modern chemistry, the fixity of the equivalent proportions as we now use and know them. Mendeléeff himself, in his excellent Faraday lecture on the periodic law ('Journal of the Chemical Society,' 1889, p. 634, &c.) distinctly refuses to recognise any connection between the periodic law and the idea of an unique matter.

Stas, who began with a belief in the hypothesis, led to the result "that the simplicity supposed by Prout's hypothesis to exist in the ratios of weights which come into play in chemical processes has experimentally not been found; it does not exist in reality."[1]

[1] Stas, quoted by Ostwald, 'Lehrbuch der Allgemeinen Chemie,' vol. i. 2nd ed., Leipzig, 1891, p. 129. The revival of the hypothesis of Prout about the middle of the century was owing to the discovery by Dumas and Stas of the fact that Berzelius's figure, 12·20, for the

Prout's simple but incorrect assumption belongs to the age which witnessed the decomposition of many compounds into their two constituents by Davy's successful use of the galvanic battery, at the poles of which the two elements of substances made their separate appearance. Substances which had always been considered as elemental and permanent, such as many oxides and earths, came to be ranged among the list of binary compounds. This lent plausibility to the idea that even the supposed elements themselves might ultimately prove to be aggregates—differing in number and figure—of the elementary particles of one and the same primary substance. Though with Prout's hypothesis this view has been repeatedly held and refuted, another theory—recommended likewise by its simplicity—had its origin in the discoveries of Davy, and the further development of them by Berzelius. This is the so-called electro-chemical or binary theory of chemical compounds. The dual combination of one elementary substance with another, and again of two dual compounds with each other, and so on, even to the most complicated compounds, was to be the simple type of chemical combination. This view, so

atomic weight of carbon, taking oxygen as 16, was incorrect. An account of the long series of determinations of this important constant will be found in the same work, p. 82, &c. I believe that in the first edition of this work will also be found the first consistent attempt to introduce into chemical data an estimate of the degree of accuracy or the amount of error which attaches to our knowledge of the constants of nature and the so-called laws of phenomena. This consideration, so familiar to astronomers, was, I believe, quite overlooked in many of the best handbooks during the earlier half of our century, and it is even yet hardly touched upon in the ordinary textbooks. The result is an entirely erroneous impression produced on the popular mind as to the degree of certainty which belongs to scientific statements.

simple and plausible, governed research for a long period, but has finally been abandoned as insufficient.[1]

16. Discovery of isomerism.

Another blow was dealt at the simple theory by which

[1] The electro-chemical theory of Davy and Berzelius was, after about fifteen years of development, during which period the use of the significant terms electro-positive and electro-negative was not consistent, finally enunciated by Berzelius in 1818 in his 'Essay on the theory of Chemical Proportions and on the Chemical Action of Electricity.' From that time it reigned almost supreme for twenty years, when both physical and chemical discoveries began to show its insufficiency. A very concise account of it is given in Kopp's 'Entwickelung der Chemie,' and in E. von Meyer's 'History of Chemistry,' translated by M'Gowan (Macmillan & Co., 1891). Berzelius clung to it to the last, and at the present moment there exists a widespread opinion that the future will see a revival and modified acceptance of the Davy-Berzelius theory. In relation to this Helmholtz's celebrated Faraday lecture of the year 1881 should be read (see the reprint in Helmholtz's 'Vorträge und Reden,' vol. ii.) The peculiarity of the electro-chemical theory was that it was an atomic theory as well as a theory of chemical affinity. When it was abandoned, the two distinct interests, that of developing the atomic view, so as to give a correct description of the constitution of chemical compounds and reactions, and that of giving an explanation of chemical affinity, fell for a time asunder. The former interest preponderated, owing mainly to two reasons, the one theoretical, the other practical. The theoretical reason was the need of a different method of systematically arranging the chaos of new organic compounds with which chemistry became crowded about the year 1840. Berzelius had created the nomenclature and notation of chemistry; but this proved insufficient to describe and grasp the processes and products of the many carbon compounds. The practical reason which cast into the background the study of chemical affinity and its nature was the growing demands of manufacturing chemistry. This was during a long period occupied mainly with the analysis and synthesis of new products, or with new and simpler methods for producing well-known compounds. The study of reactions and of the products of bodies was practically of more interest than that of the forces which governed them. The question of the cost of producing chemical products was for a long time a secondary one. Towards the end of our century both theoretical and practical considerations forced upon chemists the necessity of making themselves acquainted with the different forms of energy which are at our command in chemical as well as in mechanical operations, and this has led to a renewal of the study of chemical and mechanical energy, and of the nature and laws of chemical affinity. Economy in practical chemistry can be divided into two branches: the economy of materials and the economy of energy. The great developments in the course of this century have consisted largely in utilising by-products and in avoiding waste of substance. We are now only approaching the second problem: how to put the energy which is at our command to the best use.

Berzelius united Dalton's and Davy's researches into a comprehensive system of chemistry. The identity or difference of chemical substances seemed in the early part of the century to be fixed by the constituent elements and their quantitative proportions determined by a qualitative and quantitative analysis. This simple view had to be abandoned when Wöhler in 1823, Liebig in 1824, and Faraday in 1825 found that entirely different qualities, indicating a different constitution, could belong to bodies having the same elements in the same numerical proportions.[1] The composition of a compound had to be distinguished from its constitution, the elementary from the constituent analysis and formula. It took forty years before the great variety of views which were brought forward with the purpose of explaining how composition and constitution of the same aggregate of elements might

[1] This phenomenon is termed "Isomerism," from the Greek word *ἰσομερής*, which signifies "having equal parts." The term was introduced by Berzelius in 1830, after he had satisfied himself that compounds existed, differing widely in their properties, which contain the same constituent elements in the same proportions, and which combine with other bodies in the same proportions to form neutral salts. This he found to be the case with "racemic" and "tartaric" acid. Up to that time he had hesitated in accepting the growing evidence that equal constituents in equal proportions did not constitute identity of compounds. Wöhler in 1823 and Liebig in 1824 had found the same numerical composition for "cyanate" and "fulminate" of silver. In 1825 Faraday found two hydrocarbons which contained the same proportions of carbon and hydrogen, but showed totally different properties, such as unequal density in the gaseous state. Two oxides of tin, having the same composition, were also known, and two modifications of "phosphoric acid." The explanation of these anomalies caused Berzelius much difficulty. He resorts to the notion of a difference of grouping of the constituent atoms. "The isomerism of compounds," he says, "in itself presupposes that the positions of the atoms in them must be different" (see E. von Meyer, 'History of Chemistry,' p. 238). A. Rau in his 'Theorien der modernen Chemie' (3 parts, Braunschweig, 1877-84) gives in the appendix to the third part a detailed history of isomerism. He denies that Berzelius refers to the different position of atoms in order to explain isomerism; he attributes this suggestion to Dumas in 1833.

differ, could be approximately brought into line and order. This period was filled by the development of the chemistry of organic compounds. The chemical substances which make up the framework and numerous tissues of all living beings, the juices and products of vegetable, the food and the excreta of animal organisms, consist mostly of a few elementary bodies, combined according to numbers which are highly complex and unintelligible. Most of these compounds, if removed from the organism which contained them, proved to be subject to rapid decomposition. An increasing number of stable compounds, however, were in course of time prepared from these residues, and these formed especially the subject of organic analysis. Already Lavoisier had indicated how some system might be brought into the apparent complexity of these organic bodies; and this view was adopted by Berzelius and incorporated in his dual or binary system.[1]

17. Organic chemistry.

[1] Kopp's account of the development of Berzelius's views on organic compounds is most interesting and instructive. As late as 1814 he could not reconcile the composition of organic acids, such as oxalic acid, with the atomic theory; but renewed efforts and improved methods of analysis taught him in the following years how to apply the atomic formulæ to the description of such compounds. "He was the first to show the only right road to inform ourselves regarding the constitution of these bodies, the method, namely, of analysing their combinations with inorganic substances of known atomic weight. . . . He had also a great share in establishing the view that the ratios of combinations in organic compounds are analogous to those of inorganic substances, and that theories of the former must begin by comparing them with the latter" ('Geschichte der Chemie,' vol. i. p. 398; cf. also 'Die Entwickelung der Chemie,' p. 532, &c.) To Berzelius is thus due more than to any other man the breaking down of the barrier which had before his time divided the chemistry of organic from that of inorganic substances. For a considerable time Berzelius did not look upon organic compounds as binary—in fact, in 1814 he assumed that the difference between organic and inorganic compounds lay in this, that the latter were all binary, whereas the former were ternary or quaternary. The French chemists, under the influence of Lavoisier's oxygen theory, favoured the binary view, and this was much strengthened by Gay-Lussac's researches on cyanogen (in

It was supposed that the simple and well-known elements of these bodies might have the property of forming primarily combinations which were more firmly knit together than others, that these primary combinations might then as it were take the place of elements and act like them, forming with others of similar constitution, or with the simple elements themselves, more complex compounds. In these higher compounds they might behave like elementary bodies, entering into and being expelled from them in their own proper combinations without being broken up into the ultimate elementary constituents. One of the functions of the living organism was by the action of the vital forces to produce these primary compounds or complex atoms. It was thus thought that as inorganic bodies were made up of constituents which were elements, so organic bodies were made up of constituents which were themselves partly compounds. A new term had to be coined for those constituents which might comprise both elementary bodies and these primary compounds which behaved like elements in organic substances. This was the term "Radicle." A radicle might be an element or a compound.[1] For a long time it was thought that these

1815), a compound of carbon and nitrogen, which was shown to behave like an element. Ampère in the following year showed how the salts of ammonia could be brought into line with the salts of other alkalies by considering them to contain a compound element (consisting of nitrogen and hydrogen) in place of a simple element. In his celebrated essay of 1818 Berzelius defines organic acids as binary compounds of oxygen with compound elements or radicles (Kopp, 'Geschichte der Chemie,' vol. iv. p. 269).

[1] The term "radicle," to designate the principal constituent of a compound, was used as far back as 1787 in the discussions through which the French chemists reformed the nomenclature of chemistry (Kopp, 'Geschichte,' &c., vol. iv. p. 266). It acquired a more definite meaning about the year 1835, when Liebig, in common with Berzelius and with Dumas, established what is now called the older radicle-theory of organic compounds. As Kopp has shown ('Entwickelung der Chemie, p. 576, &c.), it remained undecided at that time whether these organic radicles had actual existence, or whether they were merely a convenient symbolism,—whether they could be isolated, like cyanogen, or whether they existed only in combinations, — whether they were fixed and unchangeable, or whether they could themselves be converted one into another,—whether the same compound could be referred—for convenience sake —to more than one constituent radicle. "By most chemists the definition of organic chemistry given by Liebig ('Organic Chemistry,' 1843) was adopted, that it was the chemistry of compound radicles; . . . that these radicles really existed in the compounds as definite constituents; and if it was then said that these radicles were mostly hypothetical, this was understood as meaning that some of them were known in the free state, others not" (p. 581).

complex radicles, as distinguished from the elements, were produced mainly—if not exclusively—in the organism of the plant or of the animal. Liebig himself, who favoured this view, and who first brought organic chemistry in its application to agriculture and physiology under the notice of a large circle of readers, introduced this branch of the subject with the designation of the chemistry of compound radicles, inorganic or mineral chemistry being termed the chemistry of simple radicles. The radicles were, according to Liebig, the true elements of organic chemistry. The binary system of Berzelius received another attack led by the celebrated French chemists Laurent and Gerhardt, with whom Dumas temporarily allied himself. It was about the year 1840 that the idea of "substitution" entered the list of formulæ by which chemical philosophers attempted to systematise and simplify the ever-growing number of definite compounds, supplied mainly by organic analysis.[1] It was

18. Liebig's definition of organic chemistry.

19. Substitution.

[1] Even before that time the views of many eminent chemists had been greatly influenced by the discoveries and experiments of two great natural philosophers of this country who kept themselves free from the theoretical considerations which had led Berzelius in the elaboration of his electro-chemical and binary system. These were the researches of Davy regarding the so-called hydro-

found that one or more atoms in an organic compound, notably of hydrogen, might be replaced by an equal number of atoms of other elements, and that such products of substitution retained similar qualities, and could be mutually converted into each other, the type of the compound remaining the same. The process of substitution led to the conception of "Types," which remained the same whilst the individual compounds varied according to the different elements which were introduced.

gen acids of chlorine, bromine, and iodine, and the investigations of Graham into the salts of phosphoric acid and its different modifications. Davy, though together with Berzelius the founder of the electrochemical theory, had found it necessary to modify the oxygen theory of Lavoisier—*viz.*, that oxygen was necessarily the acid-forming element: he, and after him Dulong in France, had explained the so-called oxygen acids like sulphuric acid as hydrogen compounds of certain compound radicles (SO_4) exactly as hydrochloric acid is a hydrogen acid of the simple radicle chlorine. Graham's discovery of three modifications of phosphoric acid, and of the different power of saturation of these three modifications, led to long discussions as to what is really meant by a neutral salt. Liebig in the year 1838, in an important memoir gathering together the conclusions which these facts, not easily reconciled with Berzelius's system, had led him to, emphasised there the twofold possibility of regarding metallic salts either with Berzelius as binary combinations of oxides with anhydrous acids, or else as products of substitution of hydrogen compounds, hydrogen being replaced by metals. The choice might then depend on considerations of convenience: the one view might be more suitable for inorganic—notably metallic—compounds, the other for organic compounds. The hydrogen theory was thus introduced alongside of the oxygen theory; substitution was introduced alongside of simple combination. Though in this stage the radicle theory was already threatened, it was still possible to uphold the binary theory, though it was not necessary. Chlorine could act in the same way as oxygen, being an electro-negative element. But when, in pursuing the line of investigation opened out, it was found that chlorine, the electro-negative element, could take the place of hydrogen in organic compounds without changing their chemical character, the binary theory, based upon polar (electrical) contrasts, became insufficient as a means of explanation or even of classification. Dumas was the first to indicate this (1834), though he attempted to save the electrochemical or polar theory by stating that the two electrically opposite constituents of an organic compound might contain the same elements in the opposite electrical positions (Kopp, 'Entwickelung der Chemie,' pp. 564, 595, &c.)

Whilst the "Radicle" theory of Berzelius and Liebig sought to simplify the study of chemical compounds by reducing them to a definite number of complex atoms, the "Type" theory of Laurent and Gerhardt sought to attain the same object by establishing a small number of simple formulæ, corresponding to well-known simple substances, under which the vast number of organic compounds could be grouped.[1] The conception of a "type" exhibiting

20. Type theory.

[1] The type theory was slowly and hesitatingly developed. Dumas, whose researches about 1835 prepared the way, did not himself draw the immediate consequences; this was done by Laurent, "who maintained that the structure and chemical character of organic compounds are not materially altered by the entrance of chlorine and the separation of hydrogen" (E. v. Meyer, 'History of Chemistry,' p. 261). Laurent then elaborated his theory of "Nuclei." They remind one of Berzelius's and Liebig's radicles. The nuclei were the groundwork of organic compounds; they were not unalterable as the radicles had been considered to be. Dumas, who at first repudiated Laurent's ideas, was later on, through his own experimental discoveries, led to adopt similar views. The "radicle," as the permanent constituent in organic compounds—corresponding to the elements in inorganic chemistry—had given way to the changeable nucleus, which only preserved its form; the unchangeable principle was found in the form, the structure or type, instead of in the substance of the simple or composite constituents. This led to an extensive study of the forms of chemical compounds—as expressed by their formulæ, and apart from the study of the properties of the original constituents. Types were invented, frequently in a somewhat arbitrary manner. "The ultimate result was that an empty scheme of formulation carried the day over what was really good in this doctrine" (ibid., p. 264). "The unitary conception was to step into the place of the dualistic. . . . Every chemical compound forms a complete whole, and cannot therefore consist of two parts. The chemical character is dependent primarily upon the arrangement and number of the atoms, and in a lesser degree upon their chemical nature" (p. 265). This is the beginning of the second great step which was taken in the elaboration of the atomic view of matter and nature. The atomic view first became a scientific instrument, when arithmetical relations of a definite and unalterable kind were suggested and proved to exist; it became a yet more useful instrument, when to the arithmetical there were added geometrical conceptions. Position, arrangement, and structure are conceptions which involve ideas of distance and space. It is true that for a long time these terms were used merely symbolically; the ultimate consequences of such conceptions can however not be avoided. The history of chemical theory in the second half of the nineteenth century is a proof of this.

certain stable qualities with a multitude of changing varieties was a notion familiar to other branches of natural history. The idea of substituting one element for another gave the death-blow to the theory of Berzelius, which assumed that elements paired with each other, according to some polar contrast. It was found, for instance, that the element chlorine, which stood on one side of the scale—the electro-negative—could take the place of the opposite electro-positive element hydrogen.

In the course of time the conception of types was much changed, and became more and more complicated; it had however the effect of finally destroying the binary view of chemical composition, and restoring in its place the older unitary conception.

All these attempts to simplify the study of chemical compounds, by reducing them to simple or complex elements, or to pairs of simpler combinations, or by ranging them according to types, were useful in many ways in extending the knowledge of bodies, in indicating new methods of inquiry, and in suggesting instructive experiments:[1] none of them were universally accepted in the

middle of the century.[1] It thus happened that a variety of circumstances combined to bring into prominence, and subsequently into general acceptance, the modern view of

21. Uncertainty in chemical theory about the middle of the century.

[1] About that time—so far as chemistry proper, *i.e.*, the study of compounds and of reactions was concerned—there existed two main currents of thought, the most illustrious and influential representatives of which were Kekulé (1829-96, first professor at Ghent, then since 1865 at Bonn), and Kolbe (1818-1884, first professor at Marburg, then since 1865 at Leipsic). As teachers and centres of academic influence, though in different, frequently opposite directions, these two eminent men continued the work started in Germany by Liebig, Wöhler, and Bunsen. To them as a third can be added the name of A. W. von Hofmann (1818-1892), who, through his twenty years' residence in London, did much to introduce a knowledge of German chemistry and German teaching methods in England, and who from 1865 established the modern Berlin school of chemistry. It would be impossible to enter here into details as to how—mainly through the influence of these three men—the work begun by Liebig and Wöhler was extended, and how especially also the great development of chemical industry in Germany was brought about; a creation almost as characteristic of German intellect, and probably more lastingly beneficial, than the political changes which mark the same period in history. More important for a history of Thought is it to note how Kolbe attached himself to the school of Wöhler and Berzelius, and tried to preserve the continuity of thought in developing the fruitful ideas contained in the writings of the latter. "He united the conclusions from his own researches with the declining theory of Berzelius; he endued the latter with new life by throwing aside whatever of it was dead, and replacing this by vigorous principles. From his own and other investigations he came to the conclusion that the unalterability of radicles, as taught by Berzelius, could no longer be maintained, since the facts of substitution had to be taken into account." He especially developed Berzelius's idea of paired compounds. (See E. v. Meyer's 'History of Chemistry,' p. 295.) Kolbe's joint work with Frankland was of the greatest importance to science. The influence of Kolbe was also largely of a polemical nature, inasmuch as he and some others, notably F. Mohr (whose name will have to be mentioned in a later chapter), protested energetically against the formal character of much of the writings and work produced by the French school which opposed the views of Berzelius. This school, of which Dumas, Laurent, and Gerhardt were the founders, and which exerted a very marked and beneficial influence through the teaching and the finished literary productions of Wurtz (1817-84), was closely allied with the school of Kekulé in Germany, who indeed began by logically developing Gerhardt's ideas, being afterwards led to special views and methods of his own, through which he became the real founder of the so-called structural formulæ, and of the doctrine of the linking of atoms. I must here especially record my indebtedness to the admirable historical essays of Wurtz ('Théorie atomique,' 7^{me} ed., 1893, and 'History of Chemical Theory,' transl. by Watts). For clearness and elegance of style, they are quite as marked as are Kopp's historical works for breadth, impartiality, and philosophical insight.

[1] The adherents of the theory of substitution and types, sometimes called the "modern," also the "French," school, urged against the followers of Berzelius, which adhered to the "electro-chemical" or "radicle" view, that since an electro-positive element could be replaced by a contrary one, there was no sense in upholding the polar difference. They pointed out that organic substances were not electrolytic; and they criticised the artificial invention and multiplication of new radicles which had no real existence, as arbitrary. On the other side, the followers of Berzelius objected to the entire ignoring by the new school of the really existing electro-chemical differences, and reproved them for having destroyed the connection between organic and inorganic chemistry, and for having introduced a purely formal systematisation according to merely externa differences. They rightly upheld

the "atomicity" or "valency" of chemical substances—be they elements or compounds. This most recent development of chemical systematisation originated in England,[1] whereas the "radicle" theory belonged more to the

the view that an understanding of chemical reactions must ultimately depend upon a study of the nature and degree of chemical affinity, and maintained that so far the connection of chemical with electrolytic phenomena afforded the only clue to the comprehension of the nature of chemical affinity. The atomic theory had now absorbed all interest, to the detriment of a physical theory of chemical affinity such as Berthollet had attempted. It was held that by ignoring the electro-chemical differences, the "modern" school lost the only remaining chance of explaining, and not merely classifying, chemical phenomena. A good exposition of the latter argument will be found in A. Rau, 'Die Theorien der modernen Chemie.'

[1] The number is small of the English names which about the middle of this century figured prominently in the discussions by which, in the German and French annals of science, correcter views on the constitution of chemical compounds were gradually elaborated. Kane's work was overlooked, but Williamson, Odling, and Frankland have had a very marked influence; and, as in so many other sciences, pioneer work in modern chemistry was done in this country, notably by Frankland. Liebig, after his visit to England in 1837, wrote to Wöhler: "I have traversed England, Ireland, and Scotland in all directions, have seen much that is astonishing, but have learnt little: whence is scientific knowledge to come in England, as the teachers are so inferior? Among older men, Thomson is still the best; among younger men, Graham: modest and unassuming, he makes the most beautiful discoveries. Nevertheless, a splendid nation," &c. &c. ('Liebig's und Wöhler's Briefwechsel,' vol. i. p. 113.) From what I stated above (chapter iii. p. 296, &c.), we are, however, quite prepared to find that the idea which more than any other has brought some order and system into modern chemical theory, and which has united the diverging currents of the foreign schools, has come from England. Frankland more than any other must be looked upon as the originator of the modern theory of the atomicity or valency of chemical elements and compounds. The history of this conception can be well studied in the collection of scientific papers which he published with valuable introductions in 1877 ('Experimental Researches in Pure, Applied, and Physical Chemistry,' London, van Voorst). His researches commenced in those years when great confusion existed in organic chemistry, "when the wildest theories of the constitution of organic compounds created but little surprise; the assertion, for instance, that an atom of carbon was united with four atoms of hydrogen and two of chlorine would scarcely have been considered intrinsically improbable, and certainly not impossible" (*loc. cit.*, p. 26). The idea existed that bodies could enter into combination with other bodies, notably organic radicles, and could still retain in such combination their original affinities unimpaired; a new term, that of "conjugate," "copulated," or "paired" compounds, had been invented and adopted by Berzelius.

German, and the "type" theory to the French, school of chemists. But the idea of the "atomicity" and "valency" or saturating capacity of the element of any substance was not possible without the clear notion of the "molecule" as distinct from the "atom." This idea had lain dormant in the now celebrated but long forgotten law of Avogadro, which was established in the year 1811, almost immediately after the appearance of Dalton's atomic theory.

22. Two aspects of the atomic theory.

The atomic theory may be regarded in two distinct ways, and it is instructive from the point of view of the history of thought to see how these two different aspects of the theory have gradually presented themselves. The older and vague atomic theory professed to be a theory of the constitution of bodies, and to afford the basis for an explanation of physical phenomena; in order to do this, forces of attraction and repulsion between the particles of

It appears that this theory was largely based upon a compound prepared by Bunsen, and called "cacodyl." This compound was one of the few organic radicles which contained a metal—arsenic. Frankland, partly alone, partly in union with Kolbe, entered upon a series of researches which had two distinct objects. Both these objects were foreign to that school which had given up the radicle theory, and which, by looking upon organic compounds as essentially different from inorganic compounds, had lost that important clue—the connection of the two branches of chemistry. These objects were the isolation of the so-called radicles or compound elements and the preparation of other "organo-metallic" bodies. The latter research led to new insight into the nature of chemical combinations. "I had not proceeded far," says Frankland, "in the investigation of the organo-metallic compounds before the facts brought to light began to impress upon me the existence of a fixity in the maximum combining value or capacity of saturation in the metallic elements which had not before been suspected. . . . It was evident that the atoms of zinc, tin, arsenic, antimony, &c., had only room, so to speak, for the attachment of a fixed and definite number of the atoms of other elements, or, as I should now express it, of the bonds of other elements. This hypothesis, which was communicated to the Royal Society on May 10, 1852, constitutes the basis of what has since been called the doctrine of atomicity or equivalence of elements; and it was, so far as I am aware, the first announcement of that doctrine" (ibid., p. 145).

matter had to be assumed, and elaborate calculations as to the integral or resultant effect of these elementary forces had to be instituted, or at least formulated. An interesting and typical case of these attempts was the theory of Boscovich, referred to in the last chapter.[1] In looking back on the history of science, it can now be safely stated that, ingenious as those theories were, they led to no results in the direction of the calculation of the molar and molecular properties of bodies, or if they did, they yielded none which could not be gained by the opposite view which regarded matter as continuous. The atomic theory, however, did good service from another point of view, when through Richter, Dalton, Proust, and Berzelius the fact that bodies combine only in definite proportions of weight, or their simple multiples, became firmly established. The authors of this discovery were driven to the atomic view

[1] See also Berthollet, 'Statique chimique,' 1803, vol. i.: "Les puissances qui produisent les phénomènes chimiques sont toutes dérivées de l'attraction mutuelle des molécules des corps, à laquelle on a donné le nom d'affinité, pour la distinguer de l'attraction astronomique. Il est probable que l'une et l'autre ne sont qu'une même propriété" (p. 1). "Il y a des sciences qui peuvent parvenir à un certain degré de perfection sans le secours d'aucune théorie, et seulement par le moyen d'un ordre arbitraire qu'on établit entre les observations des faits naturels, dont elles s'occupent principalement; mais il n'en est pas le même en chimie, où les observations doivent naître presque toujours de l'expérience même et où les faits résultent de la réunion factice des circonstances qui doivent les produire. Pour tenter les expériences, il faut avoir un but, être guidé par une hypothèse. . . . ainsi les suppositions plus ou moins illusoires et même des chimères qui sont aujourd'hui ridicules, mais qui ont engagé aux tentatives les plus laborieuses, ont été nécessaires, au berceau de la chimie. Par leur moyen les faits se sont multipliés, un grand nombre de propriétés a été constaté, et plusieurs arts se sont perfectionnés" (p. 4). "Si les propriétés chimiques des différentes substances sont dues à leur affinité et à leurs dispositions particulières, celles des combinaisons qu'elles forment dépendent de la saturation respective, des changements de constitution qui sont dus à l'action réciproque, du degré de la force qui maintient la combinaison; ainsi les propriétés des substances simples sont non seulement la cause des combinaisons, mais encore celle de leurs propres affections" (vol. ii. p. 552).

of matter as the most convenient method of expressing the formulæ of chemical compounds. Ever since that time the atomic view has served as a kind of symbolism by which different chemical elements could be characterised, their compounds described, and the actual weights practically calculated. And here we must note the reserve with which some of the greatest representatives of chemical science expressed themselves up to the middle of the century regarding the actual physical existence of those elementary particles with which they operated so freely in their formulæ, and which they even represented by balls and coloured discs in their demonstrations.

23.
A convenient symbolism.

Wollaston, one of the first who accepted Dalton's[1] views

[1] Dalton does not seem to have been troubled by any philosophical doubts or by the anticipation of the mathematical difficulties which would stand in the way of a consistent development of the atomic view. He was led to formulate and employ his atomic theory by pondering over the most convenient manner in which certain chemical facts—the facts of definite and multiple proportions—and certain physical discoveries—the separate existence of aqueous vapour from the other constituents of the air—could be represented, and he adopted the view suggested by Newton in his 'Queries,' "that matter was formed in solid, massy, hard, impenetrable, movable particles" (see Sir H. Roscoe, 'John Dalton,' Century Series, p. 128, &c.) Wollaston and Davy were much more cautious: the former foresaw the complicated and far-reaching mathematical problems which were involved in the atomic view, the latter thought the generalisation premature. His labours had been largely in the direction of showing that bodies which had been looked upon as elementary were compound, and he "doubts whether we have yet obtained elements" (ibid., p. 155). Even as late as 1826, in his award to Dalton of the Royal Medal, he speaks of his "Development of the Chemical Theory of Definite Proportions, *usually called the Atomic Theory*," he emphasises its practical usefulness, "making the statics of chemistry depend upon simple questions in subtraction or multiplication, and enabling the student to deduce an immense number of facts from a few well authenticated, accurate, experimental results." He refers to Wollaston's table of equivalents, which "separates the practical part of the doctrine from the atomical or hypothetical part." It has, in fact, been maintained that the hesitancy which Wollaston displayed on this subject deprived him of his well-deserved share of the glory which the introduction of the atomic view of matter has shed upon Dalton and Berzelius. (See Peacock, 'Life of Dr Young,' p. 469.)

as to fixed and multiple proportions, expressed himself with great reserve as to the value of the atomic hypothesis, and when drawing up a table of atomic weights, he preferred to call them equivalents—a term used already by Cavendish—as implying no other meaning than that they fix the proportions in which bodies combine into, or separate out of, compounds. Davy was hesitating and reluctant to admit any hypothesis as to the ultimate constitution of matter. Liebig [1] and Faraday,[2] at a somewhat

[1] "In endeavouring to develop the theory which at present prevails respecting the cause of the unchangeableness of chemical proportions, let it not be forgotten that its truth or falsehood has nothing whatever to do with the natural law itself. The latter is the expression of universal experience; it remains true, invariably and immutably, however our notions respecting its cause may from time to time vary and change." Thus wrote Liebig ('Familiar Letters on Chemistry,' 1844) at a time when great confusion existed as to the real atomic or smallest combining weights which should be assigned to the chemical elements; when in consequence many chemists preferred to discard the word "atomic weight" altogether, and to revert to the term equivalent (see Kopp, 'Entwickelung der Chemie,' p. 718, &c.) Dumas in 1840 declared that the term atomic weight did not deserve the confidence with which chemists made use of it: if he could he would banish the word atom from chemistry, convinced as he was that science should not transgress the limit of that which could be known by experience. Liebig, in 1839, about the time when his important memoirs on the constitution of organic bases and acids appeared in his 'Annals,' emphasised likewise the fact that equivalents never change; but he doubted whether chemists would ever agree as to the relative atomic weights, and he hoped the time was not far distant when they would all return again to equivalents (ibid., p. 438). In France an influential school, headed by the eminent M. Berthelot, up to the present day limits itself to the use of equivalents. See Berthelot, 'La Synthèse chimique,' 7^me^ éd., p. 164 n.

[2] The objections which Faraday urged against the notion of atom and atomic weight seem to come from a different quarter. In 1834, when explaining his researches on electro-chemical action, he says ('Exper. Res.,' No. 869): "If we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom;* for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration." Ten years later, in his 'Speculation touching Conduction and the Nature of Matter' (see 'Exper. Res.,' vol. ii. p. 285), he says: "The word atom, which can never be used without involving much that is purely hypothetical, is often *intended* to be used to express a simple fact. . . . There can be no doubt that the words definite proportions, equivalents, primes, &c., which did and do express fully all the *facts* of what is usually called the atomic theory in chemistry, were dismissed because they were not expressive enough, and did not say all that was in the mind of him who used the word atom in their stead; they did not express the hypothesis as well as the fact." He then enlarges on the necessity of the atomic view, and expresses his preference for the form which Boscovich had given to it over "the more usual notion," as according to the latter "matter consists of atoms and intervening space," whilst with the former "matter is everywhere present, and there is no intervening space unoccupied by it." (ibid., pp. 290, 291). It is evidently the objection to action at a distance, uncommunicated action, which is implied in the ordinary atomic view of matter, that makes Faraday jealous of the term atom. This objection was quite foreign to the chemists abroad who in the middle of the century elaborated the atomic view of matter and nature; it belongs to a different direction of thought, which will occupy us in a later chapter.

later date, appeared similarly averse to admit the physical existence of atoms in the older sense, and warned chemists against the introduction of unnecessary and unproven hypotheses. Even Gerhardt, as late as 1856, opposed the idea that chemical formulæ could express the actual constitution of substances: they were merely a convenient symbolism, a kind of alphabet, by which reactions between different elements or compounds could be conveniently described, and the proportional weights of the constituents or the products could be ascertained.[1] Accordingly, it was also maintained that formulæ could be written in very different ways, expressive of the different processes and reactions which had in special cases to be considered.[2]

Although, therefore, chemical research was governed all through the century by the atomic view of matter, it does

[1] In his 'Traité de Chimie organique,' which he brought out as a continuation of the French edition of Berzelius's 'Treatise of Inorganic Chemistry' in the years 1853 to 1856. See Kopp, 'Entwickelung der Chemie,' pp. 747, 796, 800, 809, 819, 834.

[2] Even the combining weight or equivalent of an element, that datum upon which—since Richter and Dalton—the whole system of chemistry has been built up, was

not appear that philosophers considered the existence and usefulness of chemical formulæ as a proof of the physical existence of atoms, or of smallest indivisible particles of matter, in the older sense of the theory. Hand in hand with this purely formal and experimental treatment of chemical phenomena went the almost absolute neglect with which questions referring to chemical affinity were treated. The word was little more than a name for an unknown something.

24. Neglect of the study of affinity.

How it came to pass that substances had more or less affinity for each other, what was meant by a chemical compound, symbolically expressed by writing two or more letters, near or above each other, in a square or in a circle, united by parentheses or brackets, did not seem to trouble chemical philosophers at all. To compare the problem of chemistry with that of astronomy, the former for a great part of our century resembled that phase of astronomical knowledge in which stellar maps and catalogues, plans of orbits and orreries, were considered sufficient, giving a picture of a certain constellation of the heavenly bodies, but no idea of how these configurations were maintained and altered. In fact, chemistry was for a long time a science purely of numbers, to which was attached a natural history of the substances to which these numbers belonged. The geometrical arrangement of the formulæ was usually looked upon as only symbolical: of the dynamical changes which take place in time, and imply the knowledge of

considered to be represented by more than one number in instances where the same metal had several basic or acid oxides, as in the case of nitrogen and phosphorus (ibid., p. 805). Laurent in 'Comptes Rendus,' 1844, vol. xix. p. 1099, says: "Le même corps simple se présente tantôt avec certaines propriétés, tantôt avec d'autres, il entre dans les corps composés, tantôt avec un certain poids, tantôt avec un autre."

forces or movements, few took any notice whatever. In spite of the enormous accumulation of well-arranged knowledge, and the marvellous practical achievements of chemistry, the foremost historian of that science could, as late as 1873, write as follows: "No theory has as yet been formed in chemistry which, starting from a definite principle, attempts to deduce the results of experience as necessary consequences. The doctrines which have been termed in chemistry theoretical are still only such as permit us to bring connection into the results which practical chemistry has gained in special directions; or to form a picture how we might think of them as mutually related."[1]

25. Kopp on chemical theory in 1873.

[1] Kopp, 'Entwickelung der Chemie,' 1873, p. 844. A generation earlier Dumas had written ('Comptes Rendus,' vol. x., 1840, pp. 171, 176, 178): "Dans les vues de l'électrochimie la *nature* de leurs particules élémentaires doit déterminer les propriétés fondamentales des corps, tandis que dans la théorie des substitutions, c'est de la *situation* de ces particules, que les propriétés dérivent surtout. . . . La théorie des types . . . explique ce que la loi des substitutions se contente de préciser. Elle envisage les corps organiques comme étant formés des particules, qui peuvent être déplacées et remplacées, sans que le corps soit détruit, pour ainsi dire. . . . Voilà donc en présence deux systèmes: l'un qui attribue le rôle principal à la nature des éléments, l'autre qui la réserve pour le nombre et l'arrangement des équivalents. Poussé à l'extrême chacun d'eux . . . se trouverait conduire à l'absurde." In 1861 Kekulé, in his 'Lehrbuch der organischen Chemie' (vol. i. p. 95), declares that, "besides the laws of fixed and multiple proportions of weight (and in gaseous bodies also of volume), chemistry had as yet discovered no exact laws, . . . and all so-called theoretical conceptions were merely points of view which possessed probability or convenience." And Wurtz ('La Théorie atomique,' 1863) speaks of the atomic hypothesis in terms which might lead one to think we were on the eve of an entirely different conception of the phenomena of nature: "Nous retiendrons l'hypothèse aussi longtemps qu'elle permettra d'interpréter fidèlement les faits; de les grouper, de les relier entre eux et d'en prévoir de nouveaux, aussi longtemps, en un mot, qu'elle se montrera féconde" (p. 2). "Les considérations sur la valeur de combinaison des éléments survivraient à l'hypothèse des atomes si celle-ci venait à être remplacée un jour par une hypothèse plus générale. Mais ce jour n'est pas arrivé; c'est vainement qu'on chercherait à discréditer la première aussi longtemps qu'elle se montrera féconde. Et sa fécondité, sa puissance éclatent dans les progrès incessants de la science. C'est elle qui vivifie les découvertes les plus récentes, comme elle a été depuis Dalton son immortel auteur,

This statement implies that even as late as the end of the third quarter of the century, foremost thinkers hesitated to attach a more than provisional importance to chemical symbolism and the various elaborations of the atomic theory, as chemical text-books then exhibited them. Similar merely provisional theories have existed in other branches of science. The theory of the two fluids in electricity did good service for a long time in enabling philosophers to define their ideas, to describe, calculate, and predict phenomena. In optics, the so-called corpuscular theory of light is still used with advantage as a convenient means of summarising the laws of reflexion and refraction; similarly, in treatises on the conduction of heat, the old caloric theory still holds a place alongside of the more modern dynamical views. It may be questioned

26. The periodic law.

whether the celebrated periodic law of Newlands, Lothar Meyer, and Mendeléeff, which has brought some order into the atomic and other numbers referring to the different elements, and has even made it possible to predict the existence of unknown elements with definite properties, stands really in a firmer position than the once well-known but now forgotten law of Bode,[1] according to

l'instrument le plus parfait pour les conceptions élevées de la théorie et le guide le plus sûr pour les recherches expérimentales" (p. 241). And quite mournfully does Kopp report at the close of his historical survey of the development of chemistry ('Entwickelung,' &c., p. 829) how that science about 1860 again "turned into the course which it had tried so often, and had so often abandoned as hopeless, endeavouring to gain a knowledge how the elementary atoms are arranged in the smallest particles of their compounds."

[1] According to the relation, first observed by Christian Wolff and Daniel Titius, that the distances of the planets from the sun obey approximately the formula $0{\cdot}4 + 0{\cdot}3 \times 2^n$, where n for Venus, Earth, Mars, &c., assumes the values 0, 1, 2, &c., the planet corresponding to $n=3$ was missing. When, on the discovery of Uranus in 1781, it was found that this planet's distance also agrees approximately with the formula, Bode and von Zach drew attention to this fact, and suggested a systematic search for the missing planet, "à chercher une aiguille dans une botte de foin." About the same time that this search was contemplated Piazzi found the first of the small planets, which—like the other subsequently discovered asteroids—corresponds very nearly with the expected position in the system. The periodic system of the elements, according to which the physical and chemical properties of all the elements show a periodic dependence upon the atomic weights, was first systematically stated by Newlands (in 1864) and by Lothar Meyer and Mendeléeff on the Continent. The latest edition of Meyer's treatise on "Modern Theories of Chemistry," of which only the first part, with the title 'Die Atome und ihre Eigenschaften' has been published (posthumously by the author's brother, Breslau, 1896), gives a good idea of how from small beginnings these statistics of the atomic theory of matter have grown into a great accumulation of interesting facts, upon which a system of inorganic chemistry can now be based which compares with the system of organic chemistry founded upon the types of Gerhardt in their original or in some modified form, and upon the "homologous" series of hydrocarbon compounds. As the typical arrangement of organic compounds, or rather of carbon compounds (for many real organic compounds are not easily classed by these methods), led to the suggestion of the existence of many compounds which were not known at the time, and have since been prepared, so the periodic arrangement enabled Mendeléeff to predict the properties of missing numbers of the periodic series. And although this mapping out of the elements according to their atomic weights does not indicate how and where the missing numbers are to be found, as is the case with the law of Titius and Bode, and still more so with the homologous series of carbon compounds, still it is interesting to be able to state that in several instances—notably on the discovery of the new elements, gallium (by Lecoq de Boisbaudran in 1878), scandium (by Nilson in 1880), and germanium (by Winkler in 1886)—the properties of these substances confirmed to a very great extent the predictions of Mendeléeff. And when in 1894 Lord Rayleigh and Professor Ramsay announced their discovery of a new element in atmospheric air, which, from its inertness, was called argon, interesting suggestions as to its properties were drawn from speculations regarding its probable position in the periodic curve (see Lothar Meyer, *loc. cit.*, p. 165). It is true that these numerical regularities, which for some minds possess a great fascination, are, so far, purely statistical. It is possible to arrive by interpolation or extrapolation at valuable suggestions in statistics, in meteorology, and in mining operations; but so long as the actual cause or intrinsic connection is not known, which explains the necessity of these regularities, they are apt to be misleading, and have to be used with great caution. Still, the fact alone that they bring some order into a bewildering mass of figures and data makes them almost indispensable. For similar reasons many chemists adopted Gerhardt's types and homologous series as affording a ready method of classification, though not a rational explanation of phenomena.

which the gap in the series which gives the distances of the planets from the sun indicated the existence of a

planet between Mars and Jupiter, anticipating the discovery of the Asteroids, which have accordingly been regarded as the fragments of the missing planet.

27. Difference between chemical and physical reasoning.

It thus appears that purely "chemical reasoning," as it has been called, has proved insufficient to establish the atomic view of nature on the same firm basis as has supported the mechanical or astronomical view ever since the age of Galileo and Newton.[1] In the second half of the century, the atomic view of matter has however been put forward from a different side, and independent researches have, in combination with the older chemical theories, introduced so much definiteness into this line of thought that "the Newtonian theory of gravitation is

[1] "Many diagrams and models of compound molecules have been constructed. These are the records of the efforts of chemists to imagine configurations of material systems by the geometrical relations of which chemical phenomena may be illustrated or explained. No chemist, however, professes to see in these diagrams anything more than symbolic representations of the various degrees of closeness with which the different components of the molecule are bound together. In astronomy, on the other hand, the configurations and motions of the heavenly bodies are on such a scale that we can ascertain them by direct observation; . . . the doctrine of universal gravitation not only explains the observed motions of our system, but enables us to calculate the motions of the system in which the astronomical elements may have any values whatever" (Clerk Maxwell, "On the Dynamical Evidence of the Molecular Constitution of Bodies," June 1875, 'Scientific Papers,' vol. ii. p. 418). "The chemists ascertain by experiment the ratios of the masses of the different substances in a compound. From these they deduce the chemical equivalents of the different substances, that of a particular substance being taken as unity. The only evidence made use of is that furnished by chemical combination. It is also assumed, in order to account for the facts of combination, that the reason why substances combine in definite ratios is, that the molecules of the substances are in the ratio of their chemical equivalents, and that what we call combination is an action which takes place by a union of a molecule of one substance to a molecule of the other. This kind of reasoning, when presented in a proper form, and sustained by proper evidence, has a high degree of cogency. But it is purely chemical reasoning; it is not dynamical reasoning. It is founded on chemical experience, not on the laws of motion" (Id. article "Atom," 'Ency. Brit.,' 1875; ibid., vol. ii. p. 456).

not surer to us now than is the atomic or molecular theory in chemistry and physics—so far, at all events, as its assertion of heterogeneousness in the minute structure of matter, apparently homogeneous to our senses, and to our most delicate direct instrumental tests."[1]

This side of the atomic view of matter has been developed by the study of the properties of bodies in the gaseous state, and, in its modern form, goes back to the experiments of Gay-Lussac, which were almost simultaneous with those of Dalton.[2] It is interesting to note how little the latter recognised the importance of these researches, when he rejected the so-called law of volumes, according to which gases, under the same pressure, and at equal temperatures, enter into, or separate out of, chemical combination in definite and very simple proportions of their volume. As, according to the law of definite proportions, bodies (including gases) combine only

28. The kinetic theory of gases.

[1] Lord Kelvin on "Capillary Attraction," 1886. See 'Popular Lectures and Addresses,' vol. i. p. 4.

[2] The first results referring to the combining volumes of oxygen and hydrogen gas in forming water were given by Gay-Lussac and Humboldt in a joint memoir. Their experiments were carried on in 1805. Gay-Lussac continued the experiments alone, extended them to gaseous compounds, and published his results in 1809 in the second volume of the 'Mémoires d'Arcueil.' This was one year after the publication of Dalton's 'New System of Chemical Philosophy,' and two years after Thomas Thomson had published a sketch of the atomic theory in his text-book on Chemistry. The law of equal expansion of all gases with temperature was published by Dalton in 1801; the law of pressures—that the volume of a gas, at the same temperature, is inversely as the pressure—was published by Boyle in 1662. It goes on the Continent under the name of Mariotte, who first made it generally known about twelve years later (see on this the fourth appendix to the 2nd edition of Tait's 'Properties of Matter,' 1890). The law of temperatures was published in 1802 by Gay-Lussac in the 'Annales de Chimie et de Physique' (vol. xliii. p. 137), where he remarks that Charles, Professor of Physics at the "Conservatoire," had fifteen years earlier noted the property indicated by this law. Both these so-called laws of gases are only accurate within certain [not very wide limits of temperature and pressure.

according to definite proportions of their weight, it follows that in the gaseous state these combining weights of bodies have either equal volumes or such as stand in very simple proportions. Now the amount of matter (measured by weight) in the same volume is called the density of a gas. It therefore follows, by putting Dalton's and Gay-Lussac's discoveries together, that the combining weights of gases are either directly proportional to their densities or to a simple multiple thereof. Some years after this discovery in 1809, Gay-Lussac extended his statement so as not only to embrace elementary gases, such as hydrogen, oxygen, and nitrogen, but also compounds, such as ammonia, carbonic acid, hydrochloric acid, and showed how, if they enter into chemical combination, they likewise do so in the simple proportions of one volume of one, to one or two volumes of the other.

Whilst chemists such as Gay-Lussac, Berzelius, and others[1] recognised in the facts discovered by the first a

[1] Dalton was the only person who doubted the correctness of Gay-Lussac's figures, although both Thomson and Berzelius pointed out to him the great support they afforded to the atomic theory. Berzelius also saw the usefulness of the law of volumes in fixing the smallest combining or atomic numbers in cases where the reference to weight alone left the matter undecided. Thus he correctly inferred that the formula of water should be H_2O, as we write it to-day, because two volumes of hydrogen combined with one of oxygen. But it was unfortunate that, through his want of appreciation of Avogadro's further expositions, he was unable to reconcile more completely the appeal to volume with that to weight, and that in consequence great uncertainty reigned for a long time in these matters. This induced L. Gmelin to disregard the volumetric relations in his system of equivalents, to the great detriment of those who in the middle of the century were brought up with very vague and unsatisfactory explanations on this subject—different numbers being used in books on organic and inorganic chemistry. A great confusion existed at that time, Gerhardt showing good reasons, based upon his observations of the substitution of hydrogen in organic compounds and the system of classification which he introduced, why several of Gmelin's figures should be doubled; but the matter was not cleared up till Cannizzaro

method for determining the combining weights of elements or their simple multiples, they did not draw the natural consequences as to the physical constitution of bodies in the gaseous state which followed from these and other facts which had been known before. It had been known since the time of Boyle and Mariotte that equal volumes of different gases under equal pressure change their volumes equally if the pressure is varied equally, and it was also known through Gay-Lussac himself that equal volumes of different gases under equal pressure change their volumes equally with equal rise of temperature. The like behaviour of equal volumes of different gases towards pressure, temperature, and chemical combination suggested to Avogadro, and almost simultaneously to Ampère, the very simple assumption that this is owing to the fact that equal volumes of different gases contain an equal number of smallest independent particles of matter. This is Avogadro's celebrated hypothesis. It was the first step in the direct physical verification of the atomic view of matter, and if maintained by further experience, it was destined to be one of the most important proofs of this view. But this assumption or hypothesis had to be reconciled with facts. It was, for instance, observed that a given quantity of hydrochloric acid gas occupied the same

29. Avogadro's hypothesis.

showed the real meaning and importance of Avogadro's hypothesis. A good exposition of the difference of opinions which were held at that time will be found in A. Wurtz, 'La Théorie atomique,' p. 55, &c. See also Prof. Bedson's 'Memorial Lecture' on Lothar Meyer (1896), in the 'Journal of the Chemical Society,' p. 519, &c., and especially the graphic description by L. Meyer himself of the meeting held in September 1860 at Carlsruhe for the purpose of ventilating these important theoretical questions (L. Meyer's translation of Cannizzaro's 'Sunto di un corso di filosofia chimica,' in Ostwald's 'Classiker der exacten Wissenschaften,' No. 30, Appendix, p. 58).

volume as did each of the equivalent quantities, hydrogen and chlorine, out of which it was compounded, and it appeared that accordingly double the number of atoms were condensed into the same volume. To explain this, and yet maintain his original hypothesis, Avogadro was forced into the conception of compound atoms or particles —*i.e.*, into the assumption that the smallest independent particles need not be the elementary atoms of hydrogen and chlorine themselves, but might be made up of two or more of such atoms, chemically connected in such a way that the expansion of the gas under increasing temperature or decreasing pressure did not affect this complex of elementary particles.[1] Such a compound

[1] Avogadro published his memoir in the 'Journal de Physique' in 1811, and Ampère expounded similar views three years later in the form of a letter to Berthollet in the 'Annales de Chimie.' Neither the celebrity of Ampère nor the exhaustive explanations of Avogadro, who was then an unknown author, prevented this hypothesis, which is now looked upon as a cornerstone of the atomic view, from falling into oblivion. Whewell does not mention it. Even Kopp, whose labours for many years covered a field little cultivated by most other chemists, that of physical chemistry, makes no mention of Avogadro's and Ampère's hypothesis in his great work on the History of Chemistry, published between the years 1843 and 1847. In his later work ('Die Entwickelung der Chemie,' 1873) he enters elaborately into the causes which made chemical philosophers overlook so valuable a suggestion (p. 353, &c.) Like Whewell's History, Poggendorf's Dictionary (1863) was silent about Avogadro. The distinction between molecules and atoms seemed to complicate matters; besides, the new hypothesis was not launched in conjunction with any new experimental discoveries, as had been the case with Dalton's, Davy's, and Gay-Lussac's theories. The first who again drew attention to the subject was Dumas, who in 1826 began his investigations regarding the specific weight of vapours—*i.e.*, of bodies in a gaseous state. He there drew attention to the necessity of distinguishing between chemical and physical particles, but he does not yet consistently use the terms atom and molecule to denote the former and the latter. In the meantime, however, a very important step had been taken in the development of the atomic view. In 1819 Dulong and Petit published their experimental researches concerning the specific heat of a large number of elementary bodies—*i.e.*, the measured quantities of heat (compared with a standard substance) which were required to raise a number of metals by one degree in tempera-

atom or complex was termed a molecule, and it was assumed that molecules, or smallest individual particles of chemical substances, might be made up of one or more atoms of the same or of different substances. Avogadro was able in this way to explain how a certain number of molecules of hydrogen—each made up of two atoms—combine with an equal number of molecules of chlorine; these being likewise composed of two atoms of chlorine, in order to form an equal number of molecules of hydrochloric acid, each of these consisting of two atoms—*viz.*, one of chlorine and one of hydrogen. This view, which 30.
Ampère likewise adopted, did not recommend itself to Neglect of same.
chemists for many years; not indeed till, about the year 1840, several eminent chemists—notably Laurent—were independently led to consider chemical compounds as formed by what is termed substitution instead of simple combination.[1] For, according to Avogadro's view, the for-

ture. They then found that these quantities stood very nearly in inverse proportions to the atomic or combining numbers. They at the same time pointed out the uncertainty which—in consequence of the law of fixed multiple proportions—existed regarding the smallest figure which was to determine the combining weights; they chose those numbers which brought out clearly the physical regularity and coincidence which they had discovered; and they expressed their result in the rule that the atoms of all elementary bodies have the same capacity for heat. Whereas Berzelius ignored the theoretical discussions of Avogadro and Ampère, he hailed the experimental data of Dulong and Petit as most useful in helping to fix correctly the real equivalent numbers, a task to which, as the fundamental requisite of all chemistry, he devoted so much time and labour. It must, however, be noted that the law of Dulong and Petit, now universally accepted as a fundamental fact in the atomic theory, is, as little as the laws of Boyle, Charles, and Gay-Lussac, rigidly correct: it obtains within certain limits. The experiments of Dulong and Petit were extended to compounds by F. Neumann in 1831. The connection of the specific heat or thermal capacity of compounds and that of their constituents was fully investigated by Regnault. A statement of the difficulties and anomalies which still exist will be found in L. Meyer's 'Die Atome und ihre Eigenschaften' (p. 73, &c.)

[1] A very important influence in contributing to the gradual recog-

mation of the molecule of hydrochloric acid depended upon an exchange of places of the atomic constituents in the molecules of the elementary substances, an atom of chlorine being substituted for an atom of hydrogen in the hydrogen molecule, and *vice versâ* in the chlorine molecule.

About the middle of this century the conviction was thus firmly established in the minds of chemical philosophers that the simple symbolism by which Dalton and Berzelius expressed chemical combinations and processes was insufficient for the purpose of systematically arranging the

nition of the difference between atom and molecule belongs also to Gerhardt, who emphasised a fact known already to Berzelius—*viz.*, that hydrogen according to his notation appeared to combine with other bodies always in paired atoms. This fact remained unnoticed if the atomic number of hydrogen was put at 1, oxygen at 8, as was done by English chemists and reintroduced by Gmelin. Berzelius did not attach a fundamental importance to this fact. Blomstrand ('Die Chemie der Jetztzeit,' 1869, p. 30) has shown that this originated in his clinging to Lavoisier's oxygen theory. Oxygen was made the centre and measure of everything in chemistry, also of the equivalence of substances: Berzelius thus started from a unit which was too large, and with which the smaller value of hydrogen could not be measured. Gerhardt fully recognised the importance of this fact; showed in many examples that the combining or atomic weight of hydrogen had been fixed too high; and proposed to halve most of the organic formulæ. In this way he proposed to bring harmony into the theory of combining volumes and the atomic theory. He partially succeeded in doing so, although in the case of inorganic elements he went too far. This important step, which has been extolled by some, and depreciated by other historians of chemistry, is lucidly expounded by Rau in his 'Theorien der modernen Chemie' (vol. ii. p. 107, &c.) Wurtz ('Théorie atomique,' p. 64) considers Gerhardt's influence as a reform, and alludes to it as bringing again into view the hypothesis of Avogadro: "Voilà le thème d'Avogadro et d'Ampère, qui revient à l'horizon, comme une étoile dirigeante, après une longue éclipse. Et pourtant on ne peut pas dire qu'elle ait été pour Gerhardt, à cette époque du moins, un guide exclusif. Les considérations maîtresses qu'il a invoquées sont plutôt d'ordre purement chimique. Elles étaient justes, et il s'est trouvé qu'elles concordaient avec une idée également juste, et qui était tombée dans l'oubli. La distinction entre deux espèces de petites particules, molécules et atomes, qu'Avogadro et Ampère avaient introduite inutilement dans la science, que M. Dumas avait essayé de faire revivre dans sa Philosophie chimique, cette distinction était peut-être faite dans l'esprit de Gerhardt, mais elle n'apparaissait pas encore dans son langage."

growing volume of chemical knowledge; that the conception of the atom must be extended and more closely defined; that the proportions of weight were inadequate for the purpose of distinguishing and identifying the many organic compounds; and especially that the relations of volume and the arrangements of particles of matter in space must be taken notice of, if the atomic view of matter was to be made further serviceable for scientific purposes. That purely geometrical relations, such as can be grasped only by our space conceptions, are of importance in the chemical composition of substances, was very evident, for instance, in some of the optical properties of crystallised organic substances. The discoveries of Pasteur, published in 1850, mark in this respect an epoch in science.[1] He showed that there exist chemical substances which are different, but only as a right-hand glove differs from a left-hand one, a right-handed screw from a left-handed,

31. Development of the atomic view.

32. Pasteur's discovery of "chirality."

[1] A special line of "physical" or "mechanical" reasoning which bears upon the atomic view of matter began with Biot's discovery in 1815 that certain fluids—notably organic—have the property of rotating the plane of polarisation of light which passes through them. Later on he extended this observation to the vapours formed by such fluids. Faraday found in 1846 that substances which are optically "inactive" become active in the manner described under the influence of powerful electro-magnets. An explanation of the phenomenon by Fresnel, which was based upon crystalline structure, would—for liquids and vapours—have to be applied to the structure of the molecule itself. Pasteur found in 1850 that there exist two modifications of tartaric acid, which differ in this only, that one of them turns the plane of polarisation to the right, the other to the left, and that a mixture of both in the proper proportions is inactive. As far back as 1860, in his 'Leçons de Chimie,' he put the question, "whether the atoms in tartaric acid are arranged like the turns of a right-handed screw, or situated in the corners of an irregular tetrahedron, or have they any other asymmetrical grouping? . . . There can be no doubt that the atoms have an unsymmetrical arrangement after the fashion of mirrored images which cannot be made to fall into each other" (quoted by Van't Hoff, 'Die Lagerung der Atome im Raume,' German translation, 2nd ed., p. 9).

the image in a mirror from the original. Was it possible any longer to suppress the conviction that the smallest particles of matter, in forming chemical compounds, do so not only in definite proportions of weight, but also in definite geometrical distances and positions ?

About the middle of the century the atomic view of matter had thus received considerable modifications. Originally suggested only to explain, describe, or symbolise the fact that different substances combine in fixed, and especially in fixed multiple proportions, it had to be modified by a recognition of the fact that in gases at least a distinction exists between particles which are closely knit together—as it were, geometrically inseparable—and such as can move away from each other. The latter explain the increase of volume under increasing temperature or decreasing pressure. Geometrical distance came in as the means of distinguishing the molecule from the atom. And lastly, about 1850, the phenomena of right- and left-handedness,[1] discovered by Pasteur, suggested the idea of geometrical position as well as of distance. The
33. Atom and molecule.
atom had become a molecule, with a definite geometrical arrangement.

It took, however, a full generation before, in the second half of the century, these different suggestions for a modification of the atomic view became clear, before philosophers took seriously the opinion that molecules and atoms existed in reality, and were not merely a convenient symbolism, as many great chemists during the first half of the century were inclined to think. This change in the habit of chemical thought has no doubt been greatly

[1] Called by Lord Kelvin "chirality."

brought about by the development of the so-called kinetic theory of gases in the second half of the century. This is a physical, not a chemical, theory.

The kinetic theory of gases, invented for the purpose of explaining the pressure which all bodies in the gaseous state exert on the walls of the containing vessels, will always be identified with the two names of Clausius in Germany and Clerk Maxwell in England.[1] But if we

[1] Before the atomic view of matter had, in the course of the last fifty years, closely and definitely allied itself with the kinetic view, it had been allied with the astronomical view of matter. In the last century and the earlier decades of the present century we frequently find the behaviour of a complex of molecules or atoms compared with that of a planetary system ; but in addition to the forces of attraction, those of repulsion had to be resorted to in order to explain the expansiveness of gases. Heat was then considered to be a material substance, the particles of which repelled each other. Dalton favoured this view in the introduction to his 'New System of Chemical Philosophy' ; so did Berthollet and most of the French physicists who were brought up in the school of Newton and Laplace. Lasswitz, in his 'Geschichte der Atomistik' (2 vols., Hamburg, 1890), has traced the 'Decline of Kinetic Atomism' in the seventeenth century under the influence of the 'Corpuscular Philosophy.' The kinetic view of matter was allied with the Cartesian physical philosophy, which was dispelled by Newtonianism in France and by Kant's philosophy in Germany. In consequence, when in Germany A. Krönig published his 'Grundzüge einer Theorie der Gase' in 1856, philosophers who had been speculating in the direction of a Newtonian atomism (see Fechner's 'Atomenlehre,' 1855 ; Redtenbacher's 'Dynamiden System,' 1857 ; and other publications quoted by Rosenberger, 'Geschichte der Physik,' vol. iii. p. 536, &c.) were much taken by surprise. It had the immediate result of inducing R. Clausius, who had been occupied with similar researches since 1850, to publish his celebrated memoir, 'Ueber die Art der Bewegung welche wir Wärme nennen' (Poggendorf's 'Annalen,' vol. c., 1857). These two publications first called general attention to the subject. Joule's paper, which appeared in the 'Memoirs of the Lit. and Phil. Soc. of Manchester,' had remained unnoticed, but was reprinted by him, at the request of Clausius, in the 'Philosophical Magazine' (4th ser. vol. xiv.) in 1857. Subsequently, the researches of Paul du Bois-Reymond and others unearthed a whole list of authors who, in more or less definite ways, had resorted to the hypothesis of a rectilinear translatory motion of the molecules in order to explain the phenomena of pressure and other properties of gases. Among these, Daniel Bernoulli (in his 'Hydrodynamica,' 1738) seems to have expressed the clearest views, and he is now usually named as the father of the hypothesis. The fullest statement of the historical data will be found in the posthumous second edition of

agree to date the real birth, not the incubation, of any scientific idea from the moment when it was set forth in definite figures, and with mathematical precision permitting of a precise verification by actual test, the modern theory of gases was born in Manchester in the school

34. Joule's calculations.

of Dalton, when Joule in 1857 actually calculated the velocity with which a particle of hydrogen at ordinary atmospheric pressure and temperature must be moving, assuming that this atmospheric pressure is equilibrated by the rectilinear motion and impact of the supposed particles of the gas on each other and the walls of the containing vessel. This meant taking the atomic view of matter in real earnest, not merely symbolically, as chemists had done. Joule gave up the older and vague ideas of a rotatory or a vibratory motion of the particles of a gas which had been floating about since the time of Hooke[1] in various theories, and adopted the suggestion of Daniel Bernoulli, known to him through Herapath, that all particles of gaseous matter are in a natural state of rectilinear motion, which is changed only by the encounter with other particles or by the walls of the containing vessel on which they impinge, and from which they rebound.[2]

Clausius, 'Die mechanische Wärmetheorie' (Braunschweig, 1889-91, p. 2, &c.) See also O. E. Mayer, 'Die kinetische Theorie der Gase' (2nd ed., Breslau, 1895, part i. p. 11).

[1] See Tait, 'Properties of Matter,' 2nd ed., p. 289, also J. P. Joule's Memoir on 'Heat and the Constitution of Elastic Fluids,' 1848, reprinted in 'Scientific Papers,' vol. i. p. 290, &c.

[2] The real proof that the kinetic, in contradistinction to what we may call the Newtonian, view of the motion of the molecules of a gas is the correct one, and that Newtonian (attracting and repelling) forces play only a subordinate, if any, part in the observable phenomena of gaseous bodies, is based upon Joule and Thomson's experiments made in 1853. It belongs to quite a different line of reasoning, neither chemical nor mechanical, but going upon the principle introduced into scientific thought about the middle of the century, that heat and work are convertible terms and equivalent quantities. Now, it was generally assumed, before Joule and Thomson made their careful experiments, that if gaseous bodies were allowed to expand, without doing work, no change of temperature took place—*i.e.*, that heat neither appeared nor disappeared. This would mean that no work of either repelling or attracting forces was done. Joule and Thomson showed that there was indeed a very slight cooling, indicating that a small amount of heat or energy was used up in doing work against attracting forces—the forces of cohesion. Had repelling forces existed, their work would have shown itself in a rise of temperature. This line of reasoning will occupy us in a subsequent chapter (see O. E. Meyer, 'Theorie der Gase,' vol. i. p. 7, &c., also Joule's 'Scientific Papers,' vol. ii. p. 216, &c.)

This idea of the rectilinear motion of the particles of matter in a free, *i.e.*, a gaseous, state (the first attempt to explain the physical properties of matter by giving a numerical value to a molecular, not molar, quantity) was not regarded by chemists, for it was indeed of little use in explaining chemical combinations and reactions. It, however, very soon received an important addition under the treatment of Clausius.[1] 35. Clausius's first memoir.

The kinetic theory of gases had not been propounded for the purpose of explaining chemical phenomena; it had grown out of repeated attempts to explain the nature of heat, and the fact, established about ten years earlier by Mayer and Joule, that heat can be transformed into the mechanical energy of molar motion. The idea suggested itself that if heat can disappear and be replaced by the measurable motion of molar (measurably large) masses, and *vice versâ*, heat itself may be merely the energy of the directly immeasurable movements of molecular (immeasurably small) masses; and as every body

[1] How little chemical and physical reasoning went hand in hand before the middle of the century is seen from the fact that only after Clausius had published his first paper (see note, p. 433), in which he came to the conclusion that the molecules or smallest physical particles of simple (elementary) substances consist of several atoms, was his attention drawn to the fact that some French chemists, notably Dumas, Laurent, and Gerhardt, had already, by different arguments, arrived at the conclusion that the molecules of simple (elementary) gases consist of several atoms (see Clausius, *loc. cit.*, p. 22, &c.)

in the gaseous state shows the mechanical energy which we call pressure or expansiveness, the attempt was made to explain the phenomena of expansion, pressure, and temperature of gases by a purely mechanical hypothesis. This answered remarkably well. On the assumption that the particles of a perfect gas possess a rectilinear motion, the experimental formulæ of Boyle and Mariotte, of Dalton, and of Gay-Lussac, could be theoretically deduced. It also became evident that under this conception the forgotten statement of Avogadro must be correct, according to which equal volumes of different gases, under equal pressures and at equal temperatures, contain an equal number of freely moving particles.

36. Internal energy of molecules.

And when Clausius showed further that in perfect gases only a portion of the quantities of energy which are measured as motion or as heat can be explained by the assumed rectilinear motion of the particles of gases, and that an internal motion of the particles themselves must be assumed, the new ideas became still more exactly defined; they included the conception familiar to chemists of compound atoms or molecules. The smallest individual particles of matter in the free state were themselves not simple bodies, but systems of still smaller particles; they were molecules composed of atoms; the symbols of chemists became descriptive of real physical conditions; the vague notions of radicles, types, or compound atoms began to acquire geometrical and mechanical definiteness.

Thus the atomic theory, known to the ancients, revived by Dalton in the early years of the century, and employed by chemical philosophers for half a century as a

convenient symbolism, had, about the year 1860, been accepted by physicists, and used not merely as a convenient symbolism, but as a physical reality.

37. The atomic theory accepted as a physical theory about 1860.

Joule had actually calculated the velocity of a particle of hydrogen gas. The atomic view of nature was now taken in real earnest. To establish it still further, there were required definite numerical data[1] as to the size of the smallest particles (henceforth sometimes called atoms, sometimes more correctly molecules) and their number, and also clearer views as to the composition of the molecules out of their elements, the chemical atoms.

The interest which attaches to this latest development of the atomic theory is very great: it has brought about a union of the researches of chemists and physicists, and has made chemistry a province of natural philosophy.[2] No one has done more than the late Professor Clerk Max-

[1] Numerical data regarding the size and number of smallest physical particles contained in a given volume of matter have been supplied by various methods or various "lines of reasoning." The best summary will be found in Lord Kelvin's lecture, "On the Size of Atoms" (1883: reprinted in 'Popular Lectures and Addresses,' vol. i. p. 147 *sqq.*) The four lines of reasoning are founded on the undulatory theory of light, on the phenomena of contact electricity, on capillary attraction, and on the kinetic theory of gases. They "agree in showing that the molecules of ordinary matter must be something like the one ten-millionth, or from the one ten-millionth to the one hundred-millionth of a centimetre in diameter."

[2] "We can distinguish two kinds of motion, atomic motion and molecular motion. . . . To this distinction corresponds the division of natural philosophy into physics and chemistry, not rigidly, yet in so far as chemistry is mainly occupied with the equilibrium of the atoms, physics with the mechanics of the molecules. Chemical equilibrium, unchanged condition of the molecules, exists if the affinity which holds together the atoms equilibrates the forces which tend to loosen the composition of the molecule: these forces consist in the motion of the atoms. . . . As accordingly in a chemically stable compound the atomic motions remain in lasting dynamical equilibrium with the chemical forces, . . . there remains for the examination of the purely physical phenomena in the first instance only the molecular movements" (O. E. Meyer, 'Die kinetische Theorie der Gase,' vol. i. p. 6).

well to develop the novel conceptions which here force themselves upon us. Especially are we indebted to him for the idea—marking an epoch in the history of scientific thought—of the difference between historical knowledge of natural phenomena and a merely statistical summary of average results.[1] If the atomic view of nature has to be adopted seriously, as the development of the kinetic

38. Clerk Maxwell. The statistical view of nature.

[1] See Clerk Maxwell's memoir, 'Illustrations of the Dynamical Theory of Gases' (1859: reprinted in 'Scientific Papers,' vol. i. p. 377). Clausius had in his second paper, "On the average mean path of a particle" (Poggendorf's 'Annalen,' 1858), given an expression for this quantity as depending on the average distance of two particles and on the average diameter of the sphere of action of a particle. As these quantities are all only mean or average quantities, he had been obliged to resort to a method which was then novel in physical science, the method of averages and the calculus of probability, which is its mathematical expression. He had calculated the probability of a certain motion of a particle. Maxwell, who had in 1856 been engaged in writing his Adams prize essay "On the stability of the motion of Saturn's rings," had there considered the possibility of these rings being composed of a cloud of scattered particles moving with all possible velocities towards each other and round some attracting centre: he was thus familiar with physical problems in which the given data could be only average quantities. He now undertook to develop systematically the methods necessary for treating such problems, of which we have only statistical knowledge, and he there developed his famous law which gives the distribution of different velocities in a crowd of particles moving at random and in their collisions obeying the condition of the conservation of energy. This investigation marks an epoch in mathematical physics and in the history of the atomic view of nature. Like all theorems connected with the theory of probability, it has provoked a large literature, the foundations of the proof and the different steps in the logic of the deductions having been examined and criticised in the most searching manner. The expression given by Maxwell has stood all these criticisms,—"he has demonstrated the possibility of calculating in a strict manner the averages which before him had only been estimated, but which were required for a further development of the theory of gases." See O. E. Meyer, 'Die kinetische Theorie der Gase,' 2nd ed., vol. i. p. 45, &c., where also a complete account is given of the various steps by which the doubts which attached to Maxwell's theories and his proofs were at length removed, and the "variety of traps and pit-falls" avoided "which are met with even in the elements of the subject" (see Tait, "On the Foundations of the Kinetic Theory of Gases," 'Trans. of the Royal Soc. of Edinburgh,' 1886, vol. xxxiii. part 1, p. 66). In a later chapter of this history I intend to trace the development of the statistical view of nature, and shall then have occasion to revert to this subject.

theory of gases suggests, we begin to realise the enormous numbers of individual elements of matter with which we have to do in any physical or chemical operation or experiment. The step which enabled mathematicians to calculate molar and cosmical phenomena by looking upon them as made up of an immeasurably, nay infinitely, large number of elementary parts, be these of space or time, was taken by Newton and Leibniz: its result was the invention, development, and application of the infinitesimal calculus. Our fundamental notions applied only to integrals, to a summation of these differential properties. It was the problem of the new calculus to deduce from the simple differential properties, expressed in what is called the differential equation, the results of finite observable quantities. This was done by a process of summation or integration. In this process the elements were, however, all considered to be equal. This was an assumption which, for the purposes of simplicity, might be safely made in a first approximation. When, however, the kinetic theory of gases took seriously into account the motion, velocity, number, and size of the constituent particles of matter contained in any finite measurable volume, or portion of matter, two distinct views presented themselves: the one which looks only at the total or average result and aspect of the phenomena, the other which looks at the actual behaviour and properties of the component parts, be these ever so numerous or ever so small. These latter could no longer be regarded as differentials which lose their independent existence in the process of summation: they had individual properties, which were not lost in the aggregate. It is evident that chemists had been

studying those properties of matter that are preserved distinct in ever so large a number of individuals which are characteristically and specifically alike: while physicists had been mainly studying the properties of distance, motion, velocity, and size, which, if added together, merge themselves into a common sum, integral or average. It does not follow that, even so far as these latter properties are concerned, the numberless individual particles of matter behave alike; their sizes, velocities, and movements may be very different: indeed it is evident that, in a large crowd of moving particles, they must be widely different.

39. Doctrine of averages. In assigning numbers to these data, it was therefore clear that only average or mean values could be meant, and that our actual physical knowledge of the individual elements resembles that statistical information which we possess, for instance, regarding the mortality, average age, and general properties and ways of the members of a great population. It is statistical knowledge, it is not individual, historical, or biographical knowledge, that we possess.

The individual behaviour of the single molecules, their sizes, their velocities, the length of their paths, their vibrations, rotations, and internal motions, remain unknown. What can be known is only the average magnitudes of these quantities, and possibly the extreme limits within which these individual magnitudes vary. The great differences exhibited by larger portions of different kinds of matter—*i.e.*, the chemical differences or qualities—were reduced to the actual composition and qualities of the molecules and atoms themselves. Chemists and physicists were now alike compelled to venture on some more definite hypothesis, descriptive of the great variety of constitution which the molecules of chemically distinct substances exhibit. These molecules show in their combining numbers, and in their physical properties, great fixity, excluding apparently all gradual transitions. The manner in which they enter into, and again separate out of, combinations and compounds, always regaining and showing their original characteristics, forced more and more upon natural philosophers the conviction that compounds were merely geometrical arrangements of individually independent atoms, and that these atoms must possess geometrically different forms and figures, enabling them, without loss of their individuality, to enter into varying configurations. 40. Geometrical arrangement of atoms.

The conception of the molecule as a system of atoms, geometrically arranged, had gradually grown from vague suggestions in the minds of physicists as well as chemists —*i.e.*, of students of the quantitative as well as of those of the qualitative properties of substances. To the former it was especially the forms of crystals, to the latter the different degrees of saturation of chemical substances, that suggested a geometrical arrangement of atoms as the constitution of the smallest particles or molecules of different substances.

Ever since the study of the regular forms of minerals or of artificially prepared crystals was reduced to an exact science by the labours of Haüy, at the end of the last century,[1] the forms of these regular shapes have been valued by investigators, for two distinct rea- 41. Crystallography.

[1] See above, chapter i. p. 116.

sons. They seemed to afford a practical means of recognising and obtaining in the laboratory substances in their qualitative or chemical purity, if they were elements, or in identical chemical combinations, if they were compounds. And secondly, these regular, recurring forms, which, in many cases, exhibited characteristic and geometrically fixed arrangements of plane surfaces, appeared the only means by which we could gain an insight into the grouping and the shape of the ultimate particles, out of which, according to the atomic view, molar substances were constituted. If the particles of any substance, when set free to follow their most natural movements by solution, by fusion, or by volatilisation, meet again during the process of solidification in definite, always recurring forms, the conclusion seems obvious that the individual and ultimate particles possess marked peculiarities in the different directions of space. And it is almost inconceivable that these peculiarities should consist in anything else than in distinct primitive forms, arranged in varying, but geometrically definable, meshes of a network. Accordingly, different systems have been elaborated ever since the age of Haüy, which have the object of easily classifying, recognising, and measuring crystalline structures, or, more ambitiously, of discovering the number of simple forms and arrangements of networks of which our spatial conceptions admit. It is satisfactory to be able to state that investigations of the latter kind, carried on from seemingly different beginnings, have resulted in the recognition of a certain limited number of forms of symmetry. This symmetry is referred to points, called

centres, or to lines, called axes, or to planes of symmetry.[1] French and German investigators have deduced in different ways the different possible forms of symmetry, and have shown that in all thirty-two different forms of symmetry or groups are geometrically possible. These thirty-two fundamental groups of crystals can be gathered up into six classes or types, according to the different systems of crystallographic axes or the number of planes of symmetry belonging to them.[2]

[1] The question may be raised, to what extent crystallography is obliged to assume a molecular structure of matter, or what support does the atomic view receive from it? On this point see Ostwald's 'Allgemeine Chemie,' vol. i. p. 855, &c. The geometrical forms of crystals can either be derived from elementary polyhedra, as Haüy attempted to do by his "molécules intégrantes" and his theory of decrescences, space being in this system considered as continuously filled; or the elementary particles may be considered to consist of meshes of points geometrically arranged in the corners of a primitive figure in three dimensions; or elementary spheres or ellipsoids may be supposed to be piled on each other like cannon-balls. The two latter systems assume vacant spaces; the first view refers the crystalline shape to some primitive crystal, and, therefore, does not explain it. It has accordingly been said that "the structure of crystals is one of the principal supports of the molecular theory. In assuming continuous matter without at least points which are geometrically or kinematically distinct, the anisotropic structure of crystals is quite unthinkable" (Lehmann, 'Molecularphysik,' vol. ii. p. 376). This view does not agree with what Ostwald says ('Allgemeine Chemie,' vol. i. p. 868); he considers that the structure of crystals affords no proof for the molecular constitution of matter, as the data of elasticity by no means necessarily require a molecular arrangement, but formally can be ascribed as easily to continuous matter. "Nevertheless the molecular view has the advantage of greater evidence, and leads to the same results with much greater simplicity, and hence more convincingly." It seems, however, that if chemical facts and physical theory force upon us the atomic view, crystallographic phenomena force us to complete it by some conception of geometrical arrangements.

[2] This purely geometrical treatment was introduced by Bravais in his 'Études crystallographiques' (1851), the much earlier work of Hessel ('Krystallometrie,' 1831) having been forgotten. It was further developed by L. Sohnke ('Entwickelung der Theorie der Krystallstructur,' 1879), and completed by Curie (1884) and Minnigerode (1886). A concise summary will be found in Liebisch, 'Physikalische Krystallographie,' Leipzig, 1891, pp. 3 to 50; also Groth, 'Physikalische Krystallographie,' Leipzig, 1895, p. 324, &c.

42. Analogy between crystallographic and atomic laws.

An analogy has been pointed out[1] between the atomic theory in chemistry, by which Dalton explained the fixed simple and multiple proportions of the combining weights of various substances, and the molecular theory of crystalline structures, by which the fundamental forms of crystals are defined and the accessory forms derived from them. It has been found that if once a crystal has been defined by a fundamental plane referred to three axes at fixed angles, all other planes or faces can be defined by simple multiples of the numbers which belong to the fundamental plane, and which are called the parameters of the crystal. This fundamental rule or law of crystallisation, termed by Haüy the law of derivation, stands thus in the same relation to the corpuscular theory of the structure of bodies as the law of fixed multiple proportions stands to the original atomic view of matter, and it is thought that it may in the future lead to important results.[2]

43. Isomorphism.

Another very remarkable discovery had been made by Mitscherlich in 1823.[3] This is the property which various compounds possess of crystallising in the same forms, although they contain different elements—such elements being, however, joined together by similar formulæ. The elements are, as it were, interchangeable. This phenomenon has been called isomorphism. The discovery has been of great practical value, as well as theoretical interest. If the definite and invariable form of existence which the crystal exhibits is considered as a proof of the purity of a chemical substance, and if in the same crystal one elementary substance can be replaced by one or several other substances, then this substitution must take place in definite proportions of weight, in the equivalent proportions. Thus the production of such isomorphous crystals affords a method of determining the relative atomic weights or equivalents. As such it was hailed by Berzelius; the more so, as in no case did the equivalents thus obtained contradict the numbers he had found by other methods.[1] Theoretically, the property of isomorphism acquired a still greater interest when Mitscher-

[1] See Ostwald, "Allgemeine Chemie,' vol. i. p. 870.

[2] A question arises in this connection as to the accuracy of the crystallographic law of the fixity of the angles. In respect of this Ostwald says: "On examining the validity of the fundamental laws of crystallography, it becomes evident that they are only approximate, or perhaps more correctly, that there exist numerous circumstances which permit them to show themselves only in a somewhat disturbed manner" (*loc. cit.*, vol. i. p. 890). This I understand to mean that, if disturbing circumstances could be removed, the law of the fixity of angles and the simple multiples of the indices would obtain with the same accuracy as do the combining numbers and their multiples in chemical combinations.

[3] See *supra*, chap. ii. p. 191 and note.

[1] In the early days of the atomic theory as developed by Berzelius, great uncertainty existed as to the numbers which were to be chosen for the atomic weights of the elements. This was owing to the property of fixed multiple ratios—it remaining undecided which was the smallest submultiple of a given combining ratio in which any special element could enter into combination. Other methods were then used to assist in deciding this point. The law of volumes, and later the properties of isomorphism, were therefore hailed by Berzelius as welcome aids in fixing the atomic numbers. Both these methods are still used, though the latter is not always decisive. The most important method according to the present state of our knowledge is the determination of the vapour density, where such can be got, and that of the specific heat in the solid state. It is mainly owing to Cannizzaro (1858) that the apparent contradictions, which were supposed to exist in the numbers arrived at by various methods, were explained by reverting to Avogadro's forgotten hypothesis. The periodic law or arrangement of the elements into classes showing similar physical properties is likewise of use. A complete, lucid, and exhaustive statement of the most recent position of our knowledge of the true atomic weights of the elements will be found in Lothar Meyer's posthumous tract, 'Die Atome und ihre Eigenschaften,' Breslau, 1896. In this valuable book, as also in Ostwald's 'Allgemeine Chemie,' vol. i., will also be found an account of the degree of accuracy which attaches to our present knowledge of the atomic and combining numbers, which form the solid foundation of all quantitative chemistry and all practical applications.

lich discovered another crystalline property of certain chemically pure substances. He found that some substances can crystallise in more than one distinct and definite form. The alums and vitriols are typical of isomorphism. As typical of the second property, which was termed by him dimorphism or polymorphism, we have the well-known mineral calc-spar, which is dimorphous with aragonite, both having the same chemical constitution and properties. A typical example of dimorphism is the mineral rutile, which is chemically the same substance as the mineral anatase, both being chemically pure titanic oxide. Among the elements, pure sulphur crystallises in two different forms. The property of dimorphism seemed at first to contradict the inference which Mitscherlich had drawn from his first discovery—*viz.*, that the crystalline shape is expressive of the number and chemical connection of the smallest particles or atoms; but the further discovery, that if of two isomorphous bodies one is dimorphous, the other is likewise so, gave again a great support to the geometrical conception of atomic complexes—*i.e.*, to the idea that chemical individuality is ultimately to be explained not only by the number, but also by the mutual fixed position and shape, of the atoms. And yet it seemed a long way, and is a long way still, from the external, visible, and well-marked shape of a crystal, with its peculiar and well-defined geometrical, elastic, optical, and thermal properties, to the primitive molecule, made up of still more simple atoms, in the form, number, and arrangement of which we are again and again tempted to see the nature of chemical or qualitative individuality. To obtain a clear view in this way would be to work our way from

44. Polymorphism.

outside inward—a method which has rarely led to definite results in scientific research.

45. Structural and stereo-chemistry.

A department of chemical science called structural chemistry—which has quite recently developed into stereo-chemistry—has during the last fifty years of the century been working by the opposite method. Even those organic chemists who ridiculed the notion that a chemical formula, which on the surface of the paper on which it is written cannot help making use of geometrical position and proximities, is in any way a picture of the arrangements of atoms in real space, were nevertheless forced to avail themselves of this symbolism. About the middle of the century, especially through the researches of Frankland, followed by those of Couper and Kekulé, the phenomenon of multiple proportions was explained by introducing the notion of saturation. An element which can combine with one or more atoms of the same or of different elements or definite chemical compounds was looked upon as having a chemical affinity which might be wholly or only partially satisfied. The different compounds arising out of such combinations would then represent different degrees of saturation of the first element; and it was evident that elements as well as compounds could be arranged according to the degrees of saturation of which they were capable. A compound containing elements which possessed a greater capacity for saturation than the combination afforded was called unsaturated. The term valency was introduced to denote the degrees of saturation of elements and compounds, which were therefore mono-, di-, or poly-valent, according to the compounds existing in fixed simple or

46. Valency.

fixed multiple proportions. In a table of the valencies or saturating capacities of elements and compounds, the element hydrogen forms the unit and point of reference, as it does in the scale of the atomic or combining weights, and very remarkable relations and analogies have been established between the periodic law of Mendeléeff and the valency of the different elements. Nevertheless it must be remarked that the valency of an element or compound does not, according to our present knowledge, show such absolute fixity as the equivalents or combining weights do, or as the angles of crystallisation of chemically pure substances do.[1]

The introduction of the conception of valency has had an enormous influence on the development of the science of chemistry, and this in a twofold direction. Its practical use was demonstrated by Kekulé, when he placed the idea of the tetravalency, or fourfold saturating capacity, of carbon in the front of his treatise of organic chemistry,[2] and by so doing gave a great impetus to organic research. One of the first symbols used to denote

[1] Not only are many of the elements, such as oxygen and phosphorus, classed differently by different chemists according as their valency or saturating capacity is put at a higher or lower multiple, but compounds which are universally considered to be saturated compounds, such as neutral salts and water, form chemical combinations according to their combining numbers, which are quite definite and stable: such are the hydrated crystallised salts and the double salts. These compounds are called "molecular compounds." Various explanations have been attempted, but the fact remains that "no characteristic distinction has been found, either in physical or chemical behaviour, between the ordinary compounds and the molecular compounds; and therefore, strictly speaking, from the phenomena exhibited, at present no other conclusion can be drawn except that chemical compounds do undoubtedly exist which cannot be included in the structure scheme which is based on the doctrine of a constant valency" (see Nernst, 'Theoretical Chemistry,' transl. by Palmer, London, 1895, p. 246).

[2] A. Kekulé (1829-1896), 'Lehrbuch der organischen Chemie,' 1st ed., Erlangen, 1859, and later.

the valency of an element was to attach to it as many lines as it possessed capacities of saturation. The capacities of saturation or valencies thus appeared very early as points of saturation, and the saturation itself as a linkage. These geometrical artifices or expressions were, for a long time, used merely as symbols, and to the present day many eminent chemists refuse to attach to them any real meaning: formulæ of this kind were called formulæ of structure, not of constitution.

47. Atomic linkage.

One of the most remarkable instances of the exact use of linkages to explain the difference of a series of organic compounds, all closely connected with each other, is the theory of the so-called aromatic compounds, derived from benzene, which we owe to Kekulé. It has stood the criticism of more than a quarter of a century, and has led to the most wonderful practical knowledge of a large number of old and new compounds.

It is not astonishing if, in the face of these remarkable strides which geometrical symbols have led to, an attempt has been made to form an actual conception of the geometrical figure and grouping of the atoms of which chemical molecules and compounds are made up.

Space relations are the only ones in which the difference of symmetry and asymmetry can be at all conceived by us; and when chemical compounds were discovered which show no other difference than that one of them turns the plane of polarisation of a ray of light passing through it to the right, the other to the left side, the time seemed ripe to seek an explanation of this in a purely stereometrical difference of form or grouping.

In 1874 two chemists, Le Bel and Van't Hoff, suggested independently a picture of the tetravalent carbon atom, which would explain how it could enter with its four points or capacities of saturation into two compounds having the same saturating substances, but arranged in ways which were not geometrically superposable, but only symmetrical, like a right- and left-hand glove, or the images in a mirror. The suggestion amounts to this,

48. The carbon tetrahedron.

that the carbon atom has the shape of a tetrahedron, the four corners representing the four valencies or capacities of saturation.[1]

The carbon tetrahedron is the last step which has been taken in the development of the atomic view of matter and of nature. No book on organic chemistry can now well avoid introducing this and other similar ways of representing chemical relations. On the further specialisation of this conception will probably depend to a large extent the future of our chemical theory—*i.e.*, of our attempts to grasp the qualitative nature of different substances. It is clear that we are far on the way to realising Wollaston's prophecy of the year 1808—*viz.*, "that the

[1] This speculation was at first looked upon with very great doubt. Only few chemists of note took it up; others, such as Kolbe, who led a consistent opposition to the ideas and developments of structural chemistry, treated it with ridicule. Van't Hoff, ten years after the publication of the first edition of his pamphlet, 'La Chimie dans l'Espace' (Rotterdam, 1875) reviewed the position in his 'Dix Années dans l'Histoire d'une Théorie' (translated by Marsh, Oxford, 1891), and, after reproducing the two opposite reviews, with which the original theory was met by Wislicenus and Kolbe, was able to state "that the theory in question now forms part of elementary chemical teaching, and is to be found enunciated in the most widely used text-books" (translation, p. 19). Further applications of the theory, especially to the compounds of nitrogen, will be found in the 2nd edition of the German translation 'Die Lagerung der Atome im Raume' (Braunschweig, 1894).

atomic theory could not rest contented with a knowledge of the relative weights of elementary atoms, but would have to be completed by a geometrical conception of the arrangement of the elementary particles in all the three dimensions of solid extension."[1]

But though a further development of the atomic view, not only "pondere" but also "mensura," may be expected in the near future, the progress of chemistry, which has benefited so much by this view of nature, will not de-

49. Defects and insufficiency of the atomic view.

pend exclusively upon this line of thought, nor perhaps to so large an extent as it has done during the greater part of the century. We have seen how the atomic theory of Dalton rose to the position of being more than a convenient symbolism, and how it became a physical theory of matter and of nature mainly by the support which it received from a different line of reasoning.

The development of this line of reasoning led to the employment of the statistical method, a view quite foreign to other branches of physical science.

The kinetic theory of gases itself had been elaborated in connection with still another line of reasoning, with the endeavour to get a clearer and more comprehensive view of the nature of the different forces which the astronomical as well as the atomic views had merely accepted as given quantities without further examination. We are thus necessarily led on to trace the history of

[1] See Wollaston's memoir, "On Super-acid and Sub-acid Salts," read before the Royal Society, Jan. 8, 1808 ('Phil. Trans.,' 1808, p. 96, &c.), where he even suggests the examination of the stability of aggregates of particles in different configurations, mentioning the tetrahedron, since become celebrated through Pasteur and Van't Hoff.

these other views of nature, which up to the middle of the century had grown up independently.

The next chapter will accordingly deal with the kinetic view of nature.

At the time when the atomic theory was firmly established and defined, the great founders of chemical science were well aware that the investigation and measurement of chemical forces, of what was termed affinity, was just as important a problem as the fixing of the combining weights and the formulæ of chemical compounds.

Accordingly we find men like Bergmann, Berthollet, Davy, Berzelius, and Faraday all propounding or suggesting theories of chemical affinity, some of which, like the electro-chemical theory, remained long in use. The difficulty, however, which was experienced in defining, and still more in measuring, chemical affinity, and the absence of a general system for the computation and calculation of all physical quantities, retarded the progress of this line of research compared with the study of the weights or proportions of mass which existed in chemical processes, and which were more easily ascertained by means of the balance, and made intelligible by the atomic theory.

50. Theories of chemical affinity.

The tendency of chemical reasoning during the first half of the century lay therefore in the direction of a one-sided development of the knowledge of matter, its definite constituents and infinite compounds, rather than in a study of that equally important but more subtle quantity, now called energy, which appears or disappears, but is never created or destroyed in physical or chemical processes.

A clear recognition of this fundamental doctrine—nay,

even a name for the thing implied—did not exist before the middle of the century. How both were gradually introduced will be shown in another of the following chapters.

The atomic view or theory which gave such good help in classifying and in studying the characteristic feature of all chemical processes—the fact that they take place according to definite proportions of weight—had also the effect of promoting a somewhat one-sided habit of thought, in the domain of chemical science itself.

The search for the elements, the fixing of their combining weights and properties, absorbed a great deal of time, labour, and ability.

51. Practical influences.

The practical demands of the arts stimulated the preparation of metals, of acids, and of alkalies, all of which possessed useful properties in their isolated, as distinguished from their natural, condition. This gave a stimulus in practice to the invention of processes of disintegration, and in reasoning to processes of analysis. The synthesis or putting together was expected to take place easily, if once the elements or constituent parts were got. In mineral chemistry and metallurgy this is indeed very frequently the case. It was soon found that it is not so in organic chemistry, and that when in organic chemistry a synthesis is effected, the product is frequently unlike that original natural substance from the analysis or disintegration of which the constituents or elements were procured.

It soon became evident that synthesis does not mean merely addition. A certain order had to be observed in the way of putting together, and this led to the introduction of structural, further of geometrical, formulæ. Even then, however, it was found that if a synthesis succeeded,

it did not always produce a natural, but frequently a purely artificial, compound. The practical effect of this discovery has been remarkable, not to say astonishing. New industries have been founded, and a branch of science has been created called "organic chemistry," but more correctly the "chemistry of carbon compounds," which was undreamt of in the beginning of the century.

52. Change in definition of organic chemistry.

At that time "organic chemistry" meant that branch of the science which dealt with the compounds which were found in the structures of the vegetable and animal kingdoms, and which were peculiar to them.[1] This meaning of the term "organic chemistry" has disappeared; but the branch of science which deals specially with the substances contained in living matter has not disappeared. Only the development of chemistry on the lines pre-eminently prescribed by the atomic view of nature has diverted the attention of many investigators and philosophers from the original problems of organic chemistry—the study, the analysis, and the reproduction or synthesis of such compounds as are immediately connected with living matter.

To the extent that these problems which have not lain

[1] The merit of having upheld the twofold aspect of organic chemistry and of having urged the necessity of two distinct ways of analysing organic substances, belongs in this century pre-eminently to Chevreul. Not only are his 'Recherches sur les Corps gras d'Origine animale,' carried on from 1813 to 1823, a model work of great theoretical and practical value; but he has in various writings, notably in his historical memoirs ('Journal des Savants,' 1852-60), insisted on the necessity of studying what he terms, after Fourcroy, "les principes immédiats, qui constituent les végétaux et les animaux." This study is based upon quite a different method from that usually called "analyse élémentaire." Chevreul's great work has been continued and developed by M. Berthelot in his celebrated book, 'Chimie organique fondée sur la Synthèse,' 1860, two vols.

specially on the lines marked out by the atomic view of nature have, in the course of time, reasserted themselves, the atomic view itself has been regarded with less favour by students who have made these problems their especial study. In fact, one meets not infrequently with an inclination to disparage the atomic theory, to point out that it is merely a hypothesis, and that as such it should only assist, but not govern, scientific research.[1]

In the domain of specially chemical reasoning we meet with severe criticisms of the one-sided and formal development to which the atomic view has led, of the playing with symbols and of their empty formalism; notably structural chemistry and stereo-chemistry have not escaped severe ridicule.[2] Whilst it is not very evident how the school from which these criticisms proceed can in the long-run escape those logical consequences which are embodied in stereo-chemistry, other criticisms claim our attention

53. Criticisms of the atomic view.

[1] See Berthelot, 'La Synthèse chimique,' 7me éd., 1891, p. 167. 'Le principal reproche, que l'on puisse adresser à la théorie atomique, comme à toutes les conceptions analogues, c'est qu'elles conduisent à opérer sur ces rapports numériques des éléments et non sur les corps eux-mêmes, en rapportant toutes les réactions à une unité type, nécessairement imaginaire. Bref elles enlèvent aux phénomènes tout caractère réel, et substituent à leur exposition véritable une suite de considérations symboliques, auxquelles l'esprit se complaît, parce qu'il s'y exerce avec plus de facilité que sur les réalités proprement dites . . . les symboles de la chimie présentant à cet égard d'étranges séductions par la facilité algébrique de leurs combinaisons et par les tendances de l'esprit humain, naturellement porté à substituer à la conception directe des choses . . . la vue plus simple . . . de leurs signes représentatifs."

[2] The late eminent Professor Hermann Kolbe of Leipsic, whose labours both alone and jointly with Frankland have done so much to break down the formalism of the older type theory, was especially conspicuous by his virulent attacks on the representatives of 'Modern Chemistry.' The controversy is elaborately and lucidly treated by A. Rau in 'Die Theorien der modernen Chemie' (Braunschweig, 1877-84, 3 parts), which contains very valuable historical references. I am afraid it is greatly owing to this party spirit that Kolbe's own greatness is hardly sufficiently known in this country.

because they follow from distinctly defined and independent lines of reasoning. The three criticisms can be summed up in three distinct arguments, all three demanding our special and exhaustive study. These three arguments may be summarised as follows:—

First. The atomic view is a hypothesis resting upon the fact that substances combine in fixed and fixed multiple proportions, and upon the further observation that bodies both in the solid and liquid state show different properties in different directions of space. But as to the nature of the differences of the elements the atomic view gives no information; it simply asserts these differences, assumes them as physical constants, and tries to describe them by number and measurement.

The atomic view is therefore at best only a provisional basis, a convenient resting-place,[1] similar to that which Newton found in physical astronomy, and on which has been established the astronomical view of nature.

Second. The atomic view in its present development gives us no insight into the nature of those forces on which depend the formation or destruction of chemical compounds. It neglects the study of chemical affinity. This must be conducted on different lines of observation and reasoning.[2]

[1] As these and other points referred to here will be taken up and fully treated in future chapters of this work, I abstain from giving exhaustive references, limiting myself to such writings as will give the reader a general idea of the various attempts which have been made to go beyond or behind the Atomic View of Nature or to supplement it by other views. Very suggestive in the first instance is Lord Kelvin's address to the mathematical and physical section of the British Association in 1884, reprinted in the first volume of his 'Popular Lectures and Addresses,' p. 218, &c., "Steps towards a Kinetic Theory of Matter."

[2] In respect of this the Introduction to the first edition of Lothar Meyer's 'Modern Theories in Chemistry,' written in 1862 and reprinted in the subsequent editions and also in the English translation by Bedson and Williams (London, 1888), gives a very lucid summary of the historical developments. The publication of Meyer's book, by the controversies it produced, did a great deal to give "theoretical" or "physical" chemistry a distinct and independent position. Separate chairs and laboratories for physical chemistry have since been inaugurated, first at Leipsic and subsequently at other German universities. See Ostwald's article on "Physikalische Chemie," in Lexis, 'Die deutschen Universitäten,' vol. ii. p. 50, &c. Professor Ostwald is also the editor, since 1857, of the first periodical devoted to physical chemistry. To his great work, entitled 'Allgemeine Chemie,' which, since its first appearance in 1884, has done so much for "general" as distinguished from "systematic" chemistry, and to his numerous suggestive addresses, I shall frequently have occasion to refer.

Third. The atomic view, as developed in chemical formulæ, has unduly favoured and promoted the analytical tendency of research and thought, limiting synthesis to such compounds as can be artificially prepared, but neglecting that kind of synthesis by which compounds are formed in nature, and especially in living organisms.[1]

As representative of these three lines of argument, leading beyond or outside of the atomic view of nature, I mention the three names of Lord Kelvin in England, coupled with the kinetic—specially the vortex—theory of matter; of Professor Ostwald in Germany, coupled with the modern doctrines of chemical affinity; and of M. Berthelot in France, as especially identified with the development of modern synthetical methods in chemistry. In the next chapter I shall take up the line of thought embodied in the first of these developments—the kinetic view of nature. In order to understand the history of this view, we shall have to go back to opinions held

[1] See the works of M. Berthelot, quoted above, pp. 454, 455; also an address by Prof. Meldola before the chemical section of the British Association in 1895.

already in antiquity; just as I showed that the astronomical and atomic views of nature grew out of vaguer theories of older times, and that they owe their revival and scientific usefulness to the fact that they have received in recent days the precise treatment of exact measurement and mathematical reasoning.

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PREFACE.

In this second volume I have carried out the programme which I put forward in the preface to the first volume, thus finishing the first portion of my undertaking—The History of Scientific Thought in the Nineteenth Century. The two volumes form a work complete in itself, and for this reason I have attached an alphabetical index.

In addition to the names I mentioned in the preface to the first volume, I have to add those of other friends who have been of great help to me in the course of my work. With Professor Sampson, F.R.S., of Durham University, I have had many helpful discussions on the subjects of this volume, notably on chapters viii. and xiii., which he read in proof. Mr Arthur Berry, M.A., of King's College, Cambridge, has read over chapter xiii., and made valuable suggestions. Mr Archibald S. Percival, M.B., of Cambridge, has read over chapters vi. and x. Professor F. G. Weiss, D.Sc., of Victoria University, has read chapters viii. and ix. Mr Thomas Whittaker has continued his revision, much to the benefit of the book; and Dr Spence Watson has given the finishing

touches to the last pages, in which I endeavour to secure in advance the interest of my readers for the subsequent portions of this work. To all these friends I wish to express my sense of obligation and my sincere thanks. I find it impossible to express how much this book owes to my beloved wife, my constant helpmate on the long course of this arduous enterprise.

It is unnecessary for me to lighten the work of my critics by pointing out the many defects of which I myself am painfully conscious; but, in the case of the last chapter on "The Development of Mathematical Thought," I wish to say that this is—so far as I know—the first attempt to give to this abstract region of thought a place in a general history of intellectual progress. I sincerely hope that it will be followed by other and more successful attempts to perform this very difficult task. It is now abundantly clear that mathematical thought will play an increasingly important part in the progress of science and culture, and it is no longer permissible to consider it merely an interesting specialty apart from the general course of intellectual development. A due appreciation of its importance and power will in future be expected, not only from the practical thinker who applies science, but likewise from the philosopher who assigns to science its place in the comprehensive scheme of human culture.

J. THEO. MERZ.

THE QUARRIES,
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(*CONCLUDED*).

CHAPTER VI.

ON THE KINETIC OR MECHANICAL VIEW OF NATURE.

1\. The idea of motion in ancient philosophy.

It was a favourite idea with the philosophers of antiquity that everything is in motion, that rest is to be found nowhere in nature, and that the entire process of life and sensation in particular is brought about by the communication and transference of minute movements of a purely mechanical kind. Out of the deep conviction that everything around us and in us is in a perpetual flux—a doctrine which is usually fathered upon Heraclitus of Ephesus[1]—two distinct problems resulted, and occupied the thinkers of antiquity: the problem of explaining the apparent rest and permanency of many observable pheno-

[1] The doctrine of Heraclitus (B.C. 500) is placed by Zeller ('Philosophie der Griechen,' vol. i.) in direct opposition to that of the Eleatic School (Parmenides, Zeno) and of Pythagoras. The Eleatics argued from the unity of all existence to the impossibility of the multiplicity and the change of things. Heraclitus sets out from the conception that everything is in continual motion and flow (κινεῖσθαι, ἐν κινήσει εἶναι). Our knowledge of Heraclitus is derived mainly from references in the writings of Plato and Aristotle. A very full account is given by Zeller, and by E. Pfleiderer ('Die Philosophie des Heraklit von Ephesus,' Berlin, 1886), who sums up the fundamental idea in the beautiful verses of Goethe (Gedichte, "Eins und Alles":—

"Und umzuschaffen das Geschaffne
Damit sich's nicht zum Starren waffne,
Wirkt ewiges, lebendiges Thun.
Und was nicht war, nun will es werden,
Zu reinen Sonnen, farbigen Erden.
In keinem Falle darf es ruhn.
Es soll sich regen, schaffend handeln,
Erst sich gestalten, dann verwandlen;
Nur scheinbar steht's Momente still.
Das Ewige regt sich fort in Allen:
Denn Alles muss in Nichts zerfallen,
Wenn es im Sein beharren will."

mena and properties of natural objects, and the higher ethical problem of fixing upon that which is lastingly real and important in the continuous change of sensation and opinion. The latter formed the central interest of that course of reasoning which began with Socrates and culminated in Plato and Aristotle; the former was the problem of natural philosophy of which Epicurus and Lucretius stand out as the great representatives. In a well-known passage of the second book of his great poem, Lucretius explains the apparent rest of natural things by the simile of a flock of lustily dancing sheep, which at a distance looks like a white spot on a green hillside.[1] This tendency of philosophic reasoning to see motion where common-sense only sees rest, to reduce theoretically the apparently permanent properties of things to a play of intricate but imperceptible modes of motion, has governed still more markedly modern scientific thought. I shall comprise all efforts to give more definite[2] expression to this general idea under

[1] 'De Natura Rerum,' ii. 308—

"Illud in his rebus non est mirabile, quare,
Omnia cum rerum primordia sint in motu,
Summa tamen summa videatur stare quiete,
Præterquam siquid proprio dat corpore motus.
Omnis enim longe nostris ab sensibus infra
Primorum natura jacet; quapropter, ubi ipsa
Cernere jam nequeas, motus quoque surpere debent;
Præsertim cum, quæ possimus cernere, celent
Sæpe tamen motus spatio diducta locorum.
Nam sæpe in colli tondentes pabula læta
Lanigeræ reptant pecudes quo quamque vocantes
Invitant herbæ gemmantes rore recenti,
Et satiati agni ludunt blandeque coruscant;
Omnia quæ nobis longe confusa videntur
Et velut in viridi candor consistere colli."

[2] This more definite expression is entirely a question of mathematics. It is interesting to note how Le Sage, in his 'Lucrèce Neutonien' (Berlin Acad., 1782), "argues that if Epicurus had had but a part of the geometrical knowledge of his contemporary Euclid, and conceptions of cosmography the same as those of many then living, he might have discovered the laws of universal gravity, and not only the laws, but, what was the despair of Newton, its mechanical cause" (Munro, 'Lucretius,' vol. ii. p. 135). Lionardo da Vinci (1452-1519) says: "There is no certainty in science where some mathematics are not applicable" (quoted by Lasswitz, 'Geschichte der Atomistik,' 1890, vol. ii. p. 11); and Leibniz, in a letter to Foucher dated 1693, condemns his earlier tract entitled 'Hypothesis Physica' as a "juvenile attempt of one who had not yet fathomed mathematics" (Gerhardt's edition of Leibniz's 'Philosophische Schriften,' vol. i. p. 415).

the name of the kinetic[1] theory or view of nature. It has frequently been placed in opposition to the atomic theory, and the history of the natural philosophy of the earlier ages, down to Newton, has in recent years been written from this point of view.[2] If everything is motion, there must still be something that moves, and the question arises, What is it that moves? The system of Epicurus, and the great poem in which it has found a classical expression, are really more occupied with describing the final elements of matter—the so-called nature of things—than with studying the different modes of their motion. In the atomic theory, in the conception of an infinite number of moving particles, the kinetic tendency of thought repeatedly found both in ancient

[1] The word "kinetic" seems to have been introduced into scientific literature by Ampère, who uses the term "cinématique" to denote that portion of mechanics where "les mouvements sont considérés en eux-mêmes, tels que nous les observons dans les corps qui nous environnent, et spécialement dans les appareils appelés machines" ('Essai sur la Philosophie des Sciences,' 1834). In English text-books the term kinematics, following Thomson and Tait ('Natural Philosophy,' Preface), is used to denote what French writers call "cinématique pure," formerly called "phoronomie," the doctrine of the purely geometrical properties of motion, without reference to the cause of motion; the consideration of the latter being the special study of "kinetics," which, together with "statics," is comprised in the term "dynamics." The acceptance of the word "kinetic" to denote the view that motion is at the bottom of all natural processes dates probably from the writings of Thomson (Lord Kelvin), Tait, and Clerk Maxwell, who, under the influence of Newton and the great French school of Lagrange, Ampère, Poinsot, Poncelet, and others, have reformed English, and subsequently also German, thought and nomenclature in these subjects.

[2] I refer to the highly interesting and important work of Professor Kurd Lasswitz, 'Geschichte der Atomistik vom Mittelalter bis Newton,' 2 vols., Hamburg and Leipzig, 1890.

and modern times a convenient resting-place; but the repose which it afforded has never been long enjoyed; every new attempt to attach permanent, ultimate, or intrinsic properties to matter, or to its particles, has provoked the desire to explain these properties by going still farther back, and to see in them, through the dissecting microscope of the mind's eye, a still more hidden motion. Two of the most suggestive ideas by which physical science has benefited in the nineteenth century are the successful explanation of the dead pressure of gases by a rapid translational, and of the rigidity of solid bodies by a rapid rotational, motion of matter. The second of these suggestions is far from being exhausted in its capabilities; the working out of the ultimate problems which it suggests will be one of the principal tasks of the coming age.

2. Descartes' development of the kinetic view.

The kinetic view of nature, however useful and suggestive it may have shown itself to be in recent times, did not yield any fruits of real knowledge either in the hands of the ancients or even in those of the first great philosopher of modern times, in those of Descartes. Just like attraction and atomism, the kinetic theory had to be worked out by the instruments of measurement and calculation, by the exact method, before it led to any actual results. The kinetic view of nature was made scientifically possible when Newton, in the First Book of the 'Principia,' laid down for all time the laws of motion. And yet we can hardly say that Newton himself developed this promising vein of exploration; for, even while opening out an endless vista of research, he also, in the enunciation of the so-called law of gravitation, afforded only

one of those convenient resting-places, those preliminary or provisional bases of thought, from which definite problems could be attacked and solved. His immediate influence lay, therefore, rather in discountenancing the attempts towards a kinetic view of nature, which belonged to the school of Descartes, and found an eminent exponent in Huygens as well as in others of his contemporaries and rivals;[1] in fact, he launched into existence what I have termed the astronomical view of nature, under the sway of which the promising beginnings of the kinetic view were for a long period almost forgotten, but which has the merit of having built up the most perfect of all physical sciences, namely, physical astronomy.

3. Huygens and Newton.

4. Revival of the kinetic view in the nineteenth century.

The sporadic beginnings of a genuine kinetic view of natural phenomena, after having been cultivated with more or less success by Huygens and Euler,[2] and early

[1] Among these, of whom Lasswitz gives an exhaustive account, must be mentioned specially Robert Hooke (1635-1703). "In the history of the corpuscular theory Hooke represents quite an original idea, which would have been of the most far-reaching importance if Hooke himself had got beyond a mere sketch to an exhaustive theory, or if his conceptions had, through Huygens' principles of dynamics, been domiciled in science. The deviation from kinetic theories caused by Newton's discoveries brushed away, with much useless hypothetical rubbish, likewise Hooke's more valuable and legitimate suggestions. The doctrine owing to which we place Hooke between Borelli and Huygens is his vibratory theory of matter. It is given in various writings, but most clearly in his Lectures 'De Potentia Restitutiva, or of Spring explaining the Power of Springing Bodies,' London, 1678" (*op. cit.*, vol. ii. p. 329 *sq.*)

[2] Leonhard Euler (1707-83), one of the greatest analytical talents of all times, whose writings contain the beginnings of a very large portion of subsequent mathematical work in pure and applied science, was in physics a great opponent of Newton's philosophy as it was then generally expounded on the continent of Europe. There it was identified in mechanics with the theory of action at a distance, and, in optics, with the corpuscular theory of light. To both Euler opposed his ether theory, of which he gave a popular account in his celebrated 'Lettres à une princesse d'Allemagne [Princess of Anhalt-Dessau] sur quelques sujets de physique et de philosophie' (Petersburg, 1768-72, 3 parts). He had given a scientific exposi-

Young and Fresnel.

in the nineteenth century by Rumford and Young, were united into a consistent physical theory by Augustin Fresnel, who has been termed the Newton of optics, and who consistently, and all but completely, worked out one great example of this kind of reasoning. He has the glory of having not only established the undulatory theory of light on a firm foundation, but still more of having impressed natural philosophers with the importance of studying the laws of regular vibratory motion and the phenomena of periodicity in the most general manner. His work was carried through, as was that of Newton, by a combination of observation, measurement, and calculation; of experimental skill with mathematical ability.

tion of the same twenty-five years before in his Berlin memoir, "Sur la lumière et les couleurs" (1745). Euler was as much opposed to Descartes' and Leibniz's views as he was to those of Newton, and though he admits having forerunners, he hardly refers to the principal one, viz., Huygens, whose well-known and useful principle he absolutely ignores. In fact, in spite of his great name and reputation, his ideas on the ether as continuously filling space, and his attempts to explain the phenomena of light, heat, magnetism, and even gravitation by means of this continuum remained isolated, and had hardly any influence on physical science. His great friend and correspondent, Daniel Bernoulli, remained a firm believer in action at a distance, and thought Euler had put forward his hypotheses with too much assurance. It is, nevertheless, remarkable how closely the terms in which Euler, in his posthumous work 'Anleitung zur Naturlehre' (edited by the Petersburg Academy in the second volume of the "Opera posthuma . . . anno 1844 detecta," 1862), describes his ether as continuously filling empty space and existing in a strained (*gewaltsam*) condition, agree with quite modern ideas on the subject. Accordingly Euler's ether theory has in recent times been studied again by several writers abroad, of whom I will only mention E. Cherbuliez, 'Ueber einige physikalische Arbeiten Eulers' (Bern, 1872); F. Rosenberger, 'Die Geschichte der Physik' (vol. ii. 1884, p. 333 *sqq.*); C. Isenkrahe in 'Zeitschrift für Mathematik und Physik' (Hist. Lit. Abth., vol. xxvi.) and ('Abhandlungen zur Geschichte der Mathematik,' vi.; and E. Miething, 'L. Eulers Lehre vom Aether' (Berlin, 1894). The first-mentioned author tries to answer the question why Euler's ideas remained so isolated. He says (p. 49): "If we combine the results of Huygens' and Euler's investigations, we see that in the 'fifties of the eighteenth century the undulatory system formed a largely developed scientific doctrine. . . .

There is not, indeed, to be found in Fresnel's work any central and simple formula—like the gravitation formula of Newton—out of which everything else flows with mathematical necessity. His work lay rather in combining a number of fruitful suggestions thrown out by contemporary or earlier writers into a consistent whole, correcting and enlarging them as was found necessary, and following them out into their logical consequences. Thus he was able to reveal in a special branch of physical science new phenomena which had remained unobserved or unexplained till that time. In order to understand how the kinetic view of nature has become firmly established in the minds of physicists it will be useful to enum-

In a certain sense Euler carried further the work of Huygens, . . . but as he neglected the useful idea of a wave-surface and anxiously avoided Huygens' principle, he made the theory which he wished to defend unfruitful. . . . We think that Euler did more harm than good to the progress of that theory. . . . Euler's theory of light had no great number of followers." In England Euler's theory was known and generally condemned. Priestley, in his 'History of Optics' (1772), refers to it at some length. In the well-known attacks in which Lord Brougham treated so unfairly and superficially the discoveries of Dr Young, it is suggested that the latter borrowed his ideas from Euler, whose natural philosophy is held in little esteem. The fact is that Young really went back to Huygens and Newton, and that he well knew that his own opinion, as stated in the first Bakerian Lecture (1802), "was precisely the theory of Hooke and Huygens, with the adoption of some suggestions made by Newton himself as not in themselves improbable" (Young's 'Miscellaneous Works,' ed. Peacock, vol. i. p. 200). In spite of the great admiration which Young had for Euler as a mathematician, he admits that Euler "added no argumentative evidence whatever to the [undulatory] theory, but has done a real injury to the cause which he endeavoured to support" ('Lectures on Natural Philosophy,' ed. Kelland, vol. i. p. 380). A more recent and well-informed writer on this subject, M. Verdet, says of Euler: "Bien qu'il a donné de la plupart des phénomènes connus de son temps les explications les plus inexactes, il ne mérite pas moins de conserver dans l'histoire de l'optique une place éminente pour avoir dit d'une manière expresse que les ondulations lumineuses sont périodiques comme les vibrations sonores, et que la cause des différences de coloration est au fond la même, que la cause des différences de tonalité" ('Œuvres de Fresnel,' vol. i. p. xix).

erate shortly the different suggestions which Fresnel assimilated and worked up into his celebrated physical theory of light.

That light consisted in the motion of something was in the beginning of the nineteenth century a generally accepted notion among natural philosophers. It had been so ever since Olaus Römer[1] in the seventeenth century, from the observation of the hitherto unexplained delay in the disappearance of Jupiter's satellites during eclipses, had inferred, and Bradley[2] had later on confirmed, that light takes time to travel from one point in space to another. Wherever time is involved in a phenomenon, motion of something is suggested, and this something, as well as the nature of its motion, become subjects of speculation. At the beginning of the nineteenth century two distinct theories existed regarding these matters. Both had succeeded in explaining and calculating satisfactorily a large number of the phenomena of light as exhibited by mirrors and lenses, as well as in optical instruments and crystals. One of these theories, the so-called emission, emanation, or corpuscular theory of light, held that luminous bodies send out minute particles which travel in straight lines, and, impinging upon the eye, create the sensation of light. The rival hypothesis, the undulatory or vibratory theory, held light to consist in the periodic wave-motion of a substance called ether, which was supposed to exist everywhere, filling all space and interpenetrating all ponderable matter. Both theories are kinetic or mechanical theories, and for their development require the analysis of certain modes of motion. Both had to formulate their respective notions as to the something that moved. Both could point to analogies in other domains of natural science. There existed at that time similar corpuscular explanations of the phenomena of heat, of electricity

5. Undulatory and emission theories.

6. Both theories kinetic.

[1] The moons of Jupiter, of which two are visible to the naked eye, were clearly seen and described as one of the first discoveries with his telescope by Galileo in 1610, and published in his 'Sidereus Nuncius.' Owing to their continual and rapid change of position and their frequent eclipses, they were very soon considered to furnish a valuable means of determining the longitude at sea, and were repeatedly and very minutely observed. In the course of such observations by Cassini and Römer at Paris, the latter found, in 1675, that the period of occultation of the nearest moon varied. This variation he traced to the fact that the earth was moving towards or away from Jupiter. If light takes time to travel, the visibility of the phenomenon is necessarily thus anticipated or postponed. This was the first occasion on which data for the calculation of the velocity of light were forthcoming; the terrestrial experiments of Galileo having been inconclusive. Römer's explanation and calculation were accepted by most astronomers; they were confirmed by

[2] the phenomenon of aberration, discovered by Bradley. It is analogous to the observation we can make in a moving railway train if it rains; the drops at the window, though they be descending perpendicularly, yet appearing in a slanting direction, in proportion to the velocity of the train. Both phenomena involve the motion of light itself and the motion of the observer, who receives the luminous impression and locates it in space and time. The principle involved in Römer's discovery was later enunciated by Doppler, who maintained that the very short periods which belong to different colours of the spectrum, according to the undulatory theory, must suffer (like the longer periods in Römer's occultations) by the motion of the luminous object or of the observer in the line of sight. Although this theory was admitted in acoustics, it took some time before it was admitted in optics. Bolzano, Professor of Religious Philosophy and a colleague of Doppler at Prague, foretold as early as 1842 the great utility of the principle, and wrote: "I foresee with confidence that use will be hereafter made of it in order to solve—by observing the changes which the colour of stars undergoes in time—the questions whether and in which direction and with what velocity they move, how distant they are from us, and much else besides," a prediction which, since the invention of spectrum analysis and various controversies connected with the subject, has been brilliantly verified by the discoveries of Sir William Huggins (1868), Fox-Talbot, and others. That Doppler's principle is really none other than Römer's was remarked by P. G. Tait in 'Light' (2nd ed. p. 220). See also Rosenberger, 'Gesch. d. Physik,' vol. iii. p. 708 *sqq.*

and of magnetism. On the other side there was the highly developed theory of sound, which had succeeded in explaining and analysing the properties of sounding bodies by studying experimentally and mathematically the vibrations of sounding strings, membranes and plates, and also of the air in organ-pipes and other musical instruments. Acoustics, the branch of science which treats of these phenomena, was, next to physical astronomy, the furthest developed and best founded of the physical sciences. By following up the elementary and primitive experience, known already to the ancients, that sound is everywhere to be traced to the vibrations or the tremor of some body which has been struck or otherwise excited, a very complete theory, substantiated by many experiments, had been built up. Common-sense and everyday experience had originally suggested this line of inquiry and explanation.[1] No other physical science was so early in possession of the right road of inquiry. In astronomy and optics the suggestion of common-sense, which regards the earth as stationary and light as an emission travelling in straight lines, had indeed allowed a certain amount of definite knowledge, based upon measurement and cal-

7. Undulatory theory prepared by acoustics.

[1] Acoustics is probably the only physical science where this has been the case; as is well remarked by Whewell in his 'History of the Inductive Sciences.' He there contrasts acoustics with astronomy and optics. He might have added dynamics, where Galileo's principle of inertia similarly reversed the dicta of common-sense. Whewell says (vol. ii. p. 237) of acoustics: "Instead of having to travel gradually towards a great discovery, like universal gravitation, or luminiferous undulations, we take our stand upon acknowledged truths, the production and propagation of sound by the motion of bodies and of air; and we connect these with other truths, the laws of motion, and the known properties of bodies, as for instance their elasticity. Instead of epochs of discovery, we have solutions of problems."

culation, to be accumulated. A real physical theory, however, was impossible until the notions suggested by common-sense were completely reversed, and an ideal construction put in the place of a seemingly obvious theory. This was done in astronomy at one stroke by Copernicus; in optics only gradually, tentatively, and hesitatingly. The purely geometrical relations of straight lines, which light seemed to resemble; of pencils of rays, which were bent back or altered in their direction at the surface of plane or curved mirrors and of transparent bodies; seemed to flow quite easily and naturally when in the seventeenth century the simple law of refraction had been added to that of reflexion, known already to the ancients. The sciences of catoptrics and dioptrics, with their application to the telescope and microscope, were thus so complete and useful that to many it must have seemed difficult and unnecessary to plunge into a new theory;[1] especially

[1] It has always been the aim of "geometrical optics" to free itself from every hypothesis on the physical nature of light, and to deduce properties of light from a few simple geometrical constructions. Precisely in the same way all geometrical and many physical properties of the stellar system can be deduced from the kinematical formula of attraction, without discussing the nature of gravitation. This desideratum—so far as optics is concerned—was before the mind of Sir W. R. Hamilton, when, during the years 1824-33, he discovered and elaborated the theory of the "characteristic function, by the help of which all optical problems, whether on the corpuscular or on the undulatory theory, are solved by one common process" (Tait, 'Light,' 2nd ed., p. 160). Owing to the difficulties which have more and more presented themselves in the fundamental conceptions of the wave-theory and the vibrating ether, of which we shall learn more in the sequel of this chapter, the desire to bring the phenomena of refraction under a purely geometrical formula, and to emancipate the optics of crystals from physical hypotheses, has become very pronounced. Huygens' geometrical construction of the ordinary and extraordinary rays in uniaxial crystals answered well. For biaxial crystals Fresnel had introduced the wave-surface, to which corresponds Hamilton's characteristic function. For didactic purposes, and for the practical applica-

as that theory failed for a long time to explain the apparently fundamental fact, viz., that light travels in straight lines, accompanied by well-marked shadows. The contrary view, according to which light is a tremor propagated like sound, was unable to explain the existence of clearly marked shadows. And so it came about that Newton, to whom both theories were quite familiar, and to whom we owe great discoveries telling severally in favour of each of these theories, in the end threw the weight of his authority into the scale of the corpuscular or emission theory. For many this was quite sufficient to suppress for a long time all claims which the tremor or wave theory put forward, the fact being forgotten or overlooked that Newton himself had pronounced the pure emission theory to be insufficient, and had modified and complicated it by

8. Newton's authority on the side of the emission theory,

tion to crystallography, it became a desideratum to reach the geometrical conception of the wave-surface by purely geometrical methods. This has been done in an admirable treatise entitled 'The Optical Indicatrix,' by Mr L. Fletcher. He has shown that the construction of the ray, a conception easily defined geometrically, gives an easier approach than the construction of the wave, which introduces physically doubtful definitions; and he demonstrates how "a simple generalisation, involving no reference either to the constitution of the luminiferous ether or to the nature of the physical change involved in the transmission of light," will lead to the ray surface (p. 18). For his purpose he starts from a surface of reference, which in singly refractive substance is a sphere, in uniaxial crystals a spheroid, and by inference in biaxial crystals an ellipsoid with three unequal axes. This beautiful construction was arrived at, as the author tells us, before the detailed history of Fresnel's theory had come to his notice. It is now known through Verdet, one of the editors of Fresnel's 'Works' (1868), that Fresnel arrived at his wave-surface by a purely geometrical generalisation of Huygens' construction, and that the conception of the ether was subsequently fixed so as to allow the wave-surface to be deduced therefrom (p. 24); surely an interesting case in the history of scientific thought. As to the insufficiency of purely geometrical optics for explaining the phenomena connected with optical instruments, see Czapski, 'Theorie der optischen Instrumente,' Breslau, 1893, p. 2.

suggesting that the rays of light were possessed of fits of easy transmission and reflexion, *i.e.*, of regular periodic changes which could be measured and numbered. To this amplification of the simple geometrical emission theory Newton was driven by his own immortal researches, which revealed the wonderful regularly arranged colours of thin plates known as Newton's rings. In reading, after the lapse of nearly two centuries, the reflections of Newton on the nature of light, reflections which he never gathered up into a compact and exhaustive treatise, as he did the theory of gravitation,[1] we recognise that he had clearly before his mind the two fundamental phenomena peculiar to light, namely, its property of travelling in straight lines, and its periodicity, as revealed by certain delicate experiments of his own. Which of the two theories should in the end prevail depended on the more intimate knowledge—to be gained by experiment and calculation—of the two kinds of motion involved; of rectilinear motion of particles under the influence of contending forces, and of the more complicated periodic motion peculiar to waves, tremors, or oscillations. The first kind of motion, being more easily studied and also more nearly related to other prevailing studies, received earlier attention; the second—especially so

9. but also suggests the other theory.

[1] It is now sufficiently known and recognised that Newton, both in the theory of gravitation and that of light, did not propose to do more than give a preliminary formulation which was applicable as a basis for experiment and calculation. His further speculations are contained mostly in the well-known 'Queries' to the 'Opticks,' which were extended in later editions, and among which, "to show that" he "did not take gravity for an essential property of bodies," he added one question concerning its cause, choosing to propose it by way of a question, because "he was not yet satisfied about it for want of experiments" (Advertisement to second edition, 1717).

far as the mathematical side was concerned — was studied later. The former theory has been furthered more by the ingenuity of physical observers, the latter more by mathematical reasoning applied to the invention of crucial experiments which pure observation would probably never have suggested. Since the time of Newton, whose name has been used in a one-sided way to discredit the vibratory theory, although, as already stated, his discoveries contributed equally to the formation of both views, the development of the corpuscular theory owes most to the experimental labours of Biot in France and Brewster in this country; whilst no doubt Laplace's great predilection for atomic and astronomical explanation of all natural phenomena gave it great support in the eyes of his many followers and admirers. The vibratory theory was first made the subject of detailed study by Huygens, Newton's contemporary; it was accepted on purely mathematical grounds by Euler; the lines of reasoning on which its ultimate success depended were elaborated by Lagrange's and d'Alembert's mathematical study of vibrations; but the first great step in advance, based upon experiment and calculation alike, was taken by Dr Young, who from 1793 onward studied the subject, and who in 1801 published his 'Principle of Interferences.' Young was led to his reflections on the phenomena of light by an inquiry into the nature of sound,[1] a province where

10. Biot, Brewster, and Laplace against the undulatory theory.

11. Euler the successor of Huygens.

12. Young.

the theory of vibrations had already achieved so much. He was thus more interested in the physical nature than in the geometrical properties of rays of light. He was impressed by the analogies which exist between many phenomena of sound and light, and acquainted with the writings of the Continental mathematicians, among whom Euler was conspicuous as favouring the undulatory or ether theory of Huygens. He noticed that in Newton's writings were to be found the germs of both theories, also that the arguments by which Newton convinced himself that a theory of undulations could not explain the rectilinear propagation of light, were untenable.[1] On reflecting in May 1801 on Newton's beautiful experiments,

[1] In his 'Reply to the Edinburgh Reviewers' (published as a pamphlet in 1804, see Works, ed. Peacock, vol. i. pp. 192-215), Young gives the following history of his speculations: "When I took a degree in physic at Göttingen, it was necessary, besides publishing a medical dissertation, to deliver a lecture upon some subject connected with medical studies, and I chose for this the Formation of the Human Voice. . . . When I began the outline of an essay on the human voice, I found myself at a loss for a perfect conception of what sound was, and during the three years that I passed at Emmanuel College, Cambridge, I collected all the information relating to it that I could procure from books, and I made a variety of original experiments on sounds of all kinds, and on the motions of fluids in general. In the course of these inquiries I learned to my surprise how much further our neighbours on the Continent were advanced in the investigation of the motions of sounding bodies and of elastic fluids than any of our countrymen; and in making some experiments on the production of sounds, I was so forcibly impressed with the resemblance of the phenomena that I saw to those of the colours of thin plates, with which I was already acquainted, that I began to suspect the existence of a closer analogy between them than I could before have easily believed" (p. 199). This led to his 'Outlines of Experiments and Inquiries respecting Sound and Light' (ibid., p. 64).

[1] Works, vol. i. p. 200. "Newton's arguments from experiment appear to me to have been sufficiently obviated by what Lambert has advanced in the 'Memoirs of Berlin.' . . . The demonstration is attempted in the 'Principia': to me it appears to be defective. . . . The celebrated Laplace, in comparing the opinions respecting light, is contented to call the Newtonian doctrine a hypothesis, which, on account of the facility of its application to the phenomena, is extremely probable. If he had considered the undulatory system as demonstrably absurd, he would not have expressed himself in so undecided a manner. . . . Much as I venerate the name of Newton, I am not therefore obliged to believe that he was infallible. I see . . . with regret that he was liable to err, and that his authority has, perhaps, sometimes even retarded the progress of science," &c., &c.

he "discovered a law which appeared to account for a greater variety of interesting phenomena than any other optical principle that had yet been made known."[1] This principle he familiarly illustrated by the well-known observation that two series of waves of water entering a channel reinforce or destroy each other according as their elevations coincide or alternate in time. He maintained that similar effects take place whenever two portions of light are thus mixed, and this he called "the general law of the interference of light." He showed[2] "that this law agrees most accurately with the measures recorded in Newton's 'Opticks,' relative to the colours of transparent substances, and with a great diversity of other experiments never before explained."[3] In three papers Young entered "minutely into the consequences of the law of the interference of light." Especially in the case of the remarkable phenomena discovered by Grimaldi, where light seems to bend round the edge of screening surfaces, he showed how under certain conditions light added to light would create darkness, and, if removed, would leave light; and he boldly generalised the undulatory theory by maintaining that[4] "a luminiferous ether pervades the universe, rare and elastic in a high degree," that the sensation of

13. His "general law of the interference of light."

14. Theory of the luminiferous ether.

[1] Works, vol. i. p. 202.

[2] Ibid., p. 203.

[3] "This, I assert, is a most powerful argument in favour of the theory which I had before revived: there was nothing that could have led to it in any author with whom I am acquainted, except some imperfect hints in those inexhaustible but neglected mines of nascent inventions, the works of the great Dr Robert Hooke, which had never occurred to me at the time that I discovered the law" (ibid., p. 203).

[4] The sentences in quotation marks are the headings of the different paragraphs in the "Bakerian Lecture" of November 12, 1801. Works, vol. i. p. 140 *sqq.*

different colours depends on the different frequency of vibrations excited by light in the retina, and "that all material bodies have an attraction for the ethereal medium by means of which it is accumulated within their substance." In all his conclusions, while differing from Newton's doctrines, he sees the strongest proofs of the admirable accuracy of Newton's experiments, "but scarcely any remaining hope to explain the affections of light by a comparison with the motions of projectiles."[1] Although Young thus established "a theory of the nature of light which satisfactorily removes almost every difficulty that has hitherto attended the subject,"[2] his view was only tardily accepted. Wollaston,[3] with the hesitancy which also characterised his adhesion to the atomic theory of Dalton, did not avowedly adopt Young's views, though he furnished some capital experimental support for the vibratory theory of light.[4]

Brougham, in the 'Edinburgh Review,' ridiculed Young's theories, and persuaded the public that they stood in contradiction with Newton's discoveries, on which they were really as much founded as those of the opposite school. Through such disfavour, arising largely from a want of skill in grasping the intricate mathematical problems which were involved, the doctrine of the interference of light, the mainstay of the undula-

15. Brougham's attack on Young.

[1] Works, vol. i. p. 169.

[2] 'Lectures,' ed. Kelland, Preface, p. ix.

[3] "Whatever disposition Dr Wollaston may have felt to view this theory with favour, he was restrained from adopting its conclusions by the habitual caution of his character, or rather by the want of that bold and enterprising spirit of speculation which is more or less essential to those who make great revolutions in science" (Peacock, 'Life of Young,' p. 375).

[4] Ibid., p. 374.

tory theory was, like the atomic theory of Dalton, driven out of the country. Little was heard of it, or of Young's great contribution, till it was taken up abroad, and in the very place where the brilliant development by Laplace of one side of Newton's suggestions had given plausibility to that form of the projectile theory of light according to which its material particles were supposed subject to attractive forces when they arrived in the neighbourhood of ponderable matter. Young had indeed shown that the introduction of such forces could easily be dispensed with as a basis of many of Laplace's calculations, and that the results could be got without making use of molecular attraction. He had emancipated himself from a belief in the infallibility of Laplace's methods.[1] He was also one of the first to

[1] On the 20th December 1804, Young presented to the Royal Society his important "Memoir on the Cohesion of the Fluids." It was printed in the 'Transactions' in 1805. In December 1805 Laplace read before the Institute of France, and subsequently published in a supplement to the 'Mécanique céleste,' his celebrated theory of capillary attraction. Young bases his investigation entirely on the existence of a surface tension, an observable and measurable property; whereas Laplace falls back upon the hypothesis of an attraction of the smallest particles of matter, just as he had employed the idea of an attraction of matter on the smallest particles of light to explain atmospheric refraction according to the projectile theory adopted by him. In the sequel this attraction is reduced to an action which is insensible at sensible distances. In a supplement to his memoir, which appeared anonymously in the first number of the 'Quarterly Review' (1809), Young, evidently annoyed that some of his results had been reproduced without acknowledgment (see Peacock, 'Life of Young,' p. 205), reviewed the treatise of Laplace "with a severity which, though excessive, can hardly be considered unprovoked or unmerited" (ibid., p. 206). *Inter alia* he says: "The point on which M. Laplace seems to rest the most material part of his claim to originality is the deduction of all the phenomena of capillary action from the simple consideration of molecular attraction. To us it does not appear that the fundamental principle from which he sets out is at all a necessary consequence of the established properties of matter; and we conceive that this mode of stating the question is but partially justified by the coincidence of the results derived from it with experiment, since he has not demonstrated that a similar coincidence might not be obtained by proceeding on totally different grounds" ('Quarterly Review,' No. 1, p. 109).

emancipate himself from the astronomical view of phenomena. In France the matter stood quite differently, and nothing better proves the genius of Augustin Fresnel than the fact that he ventured against the opposition of great authorities to go his own way, starting from the beginning and devising many ingenious appeals to nature herself—*i.e.*, to experiment—in order to establish a correct view. It is well known that his labours had to wait many years for their deserved appreciation.[1] It is, however, only just to remark that Arago, an admirer of Laplace and an intimate friend of Biot, the great supporter of the projectile theory of light, was the first to recognise the importance of Fresnel's work, and that it was largely owing to his co-operation and influence that the undulatory theory of light triumphed in the end. Fresnel's own labours began with the study of the same phenomena which had led Young to the discovery of "interference"—viz., the bands and coloured fringes observable round the shadows of small screening objects, or the images of small apertures through which rays of light are allowed to enter: the phenomena of diffraction or inflection of light. But whilst Young still explained these phenomena as arising from the interference of direct "portions" of light and such as were reflected at the edge of the screening obstacle, Fresnel showed that the principle of interference had a much wider application, that it was adequate to explain why a periodic wave-motion, such as was conceived by Huygens, only sent out rays of

16. Augustin Fresnel.

[1] See the first volume of this work, p. 241 note[2].

light in the direction which was in a straight line from the origin or centre of light; that the lateral or secondary waves destroyed each other almost entirely by interference or overlapping; and that the so-called inflection, bending, or lateral spreading of light, was occasioned by an incomplete coincidence or overlapping of these lateral undulations. It appears that about the year 1815 Fresnel had, through a study of the phenomena of diffraction, arrived at a conviction, entertained by Young fifteen years earlier, that the projectile theory of light could not explain them. He had also, by a more rigorous and minute study of Young's principle of interference, explained the reason of the rectilinear propagation of light. Yet these results did not materially affect the adherents of the projectile theory, who had been during late years very active in studying another class of optical phenomena, those of polarisation—the power which light possesses of acquiring, either by refraction or reflexion, a difference not discernible merely by the eye. This difference consists in the fact that a ray of light very frequently—as Newton had already expressed it—possesses "sides," just as a flat strip or narrow tape has sides if compared with an ordinary thread or wire, which has no sides; or as a wire drawn through a specially shaped die acquires sides or edges. This property was later termed polarity,[1] a term which implies that the particles of light

17. Difficulties presented by the polarisation of light.

have unequal properties in different directions; and the process of revealing it was termed polarisation. Huygens had discovered this property, which he found was given to rays of light if they passed through certain crystals, notably through Iceland spar, which has the capacity of dividing the rays so that objects seen through them appear double. He could not explain it on his hypothesis of undulations, though he had invented a geometrical construction of the double refraction which had led him to its discovery. Malus showed in 1808 that double refraction was not a necessary accompaniment of polarisation, but that ordinary reflexion was enough to give these sides to rays of light. Although the projectile theory gave no complete explanation of this property, still the supposition that this one- or many-sidedness was owing to certain geometrical shapes of the projected particles suggested that double refraction might be explained by the different attraction or repulsion which these particles suffered according to the aspect

[1] The word "polarity" was introduced by Malus in 1810. It is unfortunate, as it suggests the corpuscular nature of light. Newton's conception of "sidedness" ("laterality," formed by analogy on Lord Kelvin's term "chirality" to describe right- or left-handedness, see vol. i. p. 432) is a better description of the phenomenon. It is contained in the 26th query to the second edition of the 'Opticks' (1717). Huygens had long before, in his 'Traité de la Lumière' (written in 1678, published in 1690), after having given a correct rule for determining the course of the ordinary and extraordinary rays in Iceland spar, described the phenomenon fully, admitting at the same time that he could not explain it. When Malus discovered that light might acquire this peculiar property by reflexion, Young wrote in a review ('Quarterly Review,' May 1810): "The discovery . . . appears to us to be by far the most important and interesting that has been made in France, concerning the properties of light, at least since the time of Huygens; and it is so much the more deserving of notice, as it greatly influences the general balance of evidence in the comparison of the undulatory and the projectile theories of the nature of light" (Works, vol. i. p. 247). And Malus himself, in writing to Young as Foreign Secretary of the Royal Society, by whom he had been awarded the Rumford Medal, says: "Je ne regarde pas la connaissance de ces phénomènes comme plus favorable au système de l'émission qu'à celui des ondulations. Ils démontrent également l'insuffisance des deux hypothèses; en effet comment expliquer dans l'une ou dans l'autre pourquoi un rayon polarisé peut traverser sous une certaine inclinaison un corps diaphane, en se dérobant totalement à la réflexion partielle qui a lieu à la surface de ces corps dans les cas ordinaires?" (quoted by Peacock, 'Life of Young,' p. 248 note).

which they presented when approaching ponderable or attracting substances. Nothing of this kind seemed imaginable on the undulatory theory, which, reasoning from the analogy of sound, considered light to consist in a rapid to-and-fro motion of the ether in the direction of the rays of light. Sidedness or "laterality" seemed inconceivable. Rays of light possessing this property would (as Fresnel and Arago showed in 1816) eventually even lose their capability of interference, that main property discovered by Young, the principal argument for the vibratory theory. "Every day in that remarkable period—when so many great observers were endeavouring to outstrip each other in the career of discovery—was making known modifications and phenomena of polarised light which no existing theory was yet competent to explain. It was polarisation which still continued to cast a dark cloud over the hopes and fortunes of the undulating theory."[1] Thus it was natural that the representatives of the astronomical view of nature, who, headed by Laplace, had given so many real and some apparent explanations of complicated phenomena, and to whom the conceptions of the projectile theory of light seemed more promising, should think it time to attack the very stronghold of the vibratory theory, namely, the phenomena of interference, exhibited mainly in diffraction, and, by a minute experimental and mathematical analysis, show whether these phenomena could not be brought within the pale of their fundamental conceptions. For the discoveries of Young and Fresnel had not shaken them. Accordingly the Paris Academy of

[1] Peacock in 'Life of Young,' p. 383.

Sciences in 1817 issued for the competition on the grand mathematical prize for 1819 the subject of Diffraction, "persuaded that a deeper investigation of these phenomena, which seemed opposed to their cherished doctrine, would give occasion for new triumphs."[1] In this they were doomed to disappointment. At the request of Arago and Ampère, Fresnel entered for this competition, and his 'Mémoire sur la Diffraction' was crowned the following year. In it he viewed the subject from a much more general point of view, examining the two rival systems—that of emission and that of undulations—as to their capacity for explaining the phenomena of diffraction. The result seemed decisive in favour of the latter theory, and the impression produced was all the greater because Poisson,[2] one of the judges and a believer in the emission theory, drew certain apparently very paradoxical conse-

18. Fresnel's Memoir on Diffraction.

[1] Verdet in 'Œuvres de Fresnel,' vol. i., Preface, p. xxxv., &c.

[2] The commission consisted of Biot, Arago, Laplace, Gay-Lussac, and Poisson. Arago drew up the report, which is published in the first volume of the 'Œuvres de Fresnel,' No. 13. It closes with the following note: "M. Poisson, depuis le rapport de la commission, ayant fait remarquer à M. Fresnel que l'intégrale qui représente l'intensité de la lumière diffractie peut aisément s'obtenir pour le centre de l'ombre d'un écran ou d'une ouverture circulaires, celui-ci fit le calcul pour ce dernier cas, et trouva que l'expression générale d'intensité devenait alors semblable à celle de la lumière réfléchie dans le phénomène des anneaux colorés; que ses minima étaient tout à fait nul et devaient présenter ainsi un noir à peu près parfait dans une lumière sensiblement homogène; du moins pour les trois premiers ordres, où le défaut d'homogénéité de la lumière rouge employée ne se faisait pas encore trop sentir: c'est aussi ce que l'expérience a confirmé; en plaçant le foyer de la loupe du micromètre aux distances calculées on appercevait comme une tache d'encre au centre de l'ouverture circulaire. . . . On peut regarder cette expérience comme une vérification des formules de M. Fresnel," &c. ('Œuvres,' vol. i. p. 245). See also the note which Fresnel attached to his memoir (ibid., p. 365). The memoir was crowned in 1819, but not published till 1826. An abstract of the first and a reprint of the second part had been published in the 11th vol. of the 'Annales de Chimie et de Physique.' Fresnel sent two copies to Young, 19th September 1819.

quences from Fresnel's calculations : Fresnel was invited to prove by experiment these astonishing results, and he found them actually confirmed. So far as the phenomena of diffraction—erroneously termed inflection—are concerned, this work of Fresnel established the fact "that the theory of undulations foretells the phenomena as exactly as the theory of gravitation foretells the movements of the heavenly bodies."[1] It was, however, quite different if we consider that other larger class of phenomena[2] which revealed the fact that rays of light

[1] See Schwerd, 'Die Beugungserscheinnugen aus den Fundamentalgesetzen der Undulations-theorie analytisch entwickelt' (Mannheim, 1835), Preface, p. x.

[2] The history of the final establishment of the wave theory of light has been written by Whewell in the second volume of the 'History of the Inductive Sciences.' The main sources which existed at that time were the memoirs of Young and Fresnel, and the 'Life of Dr Young' by Peacock. This history has been written again with ampler materials by M. Verdet as an introduction to the edition of the complete works of Fresnel, published in 1866. It is well to read both accounts, as some points which remain obscure in the earlier are fully explained in the later. There is no doubt that Young suggested that the phenomena of "sidedness," which rays of light exhibit, lead to the conception of a lateral or transverse movement ; he also hinted that in biaxial crystals the shape of the wave might be that of an almond or an amygdaloid (article "Chromatics," reprinted in Works, vol. i. pp. 317, 322), what we now call an ellipsoid ; but M. Verdet is right in characterising Young's suggestions as vague, and vindicating for Fresnel the full merit of having defined transverse vibrations and of having introduced the ellipsoid of elasticity as a geometrically perfect means of finding by construction the paths of rays in biaxial crystals. The method was quite independent of the theoretical views regarding light which were contained in the same memoir, the consideration of which was referred to a commission consisting of Ampère, Arago, Fourier, and Poisson. Of these Ampère had suggested transverse vibrations as a means of explaining the phenomena of polarisation ('Œuvres de Fresnel,' vol. i. p. 394). Arago, though a great friend of Fresnel and a believer in the wave theory, never to the end of his life accepted the hypothesis of transverse vibrations (ibid., p. lv.) Poisson, a supporter of Laplace's molecular theory, retired from the commission ; and Arago, who composed the Report to the Academy, confined himself to pronouncing on the experimental portion, which fully confirmed the general law of double refraction announced by the author ; refraining from the expression of any opinion as to the theoretical portion, it being premature to do so (see 'Œuvres de Fresnel,' vol. ii. p. 463). Im-

have sides, the phenomena of "laterality" (misleadingly called polarisation). The believers in the emission theory studied them with predilection, Biot at their head. Although to Young their explanations were unconvincing, their results were so perplexing that he wrote to Brewster in September 1815, "With respect to my own fundamental hypotheses respecting the nature of light, I become less and less fond of dwelling on them, as I learn more and more facts like those which Mr Malus discovered; because, although they may not be incompatible with these facts, they certainly give us no assistance in explaining them."[1] When Young wrote this, Fresnel had not yet presented his first memoir on Diffraction to the Institute; his own labours on that matter were more than ten years old; the phenomena of polarisation had meantime absorbed the attention of opticians. In the summer of 1816 Arago and Gay-Lussac paid a visit to

mediately after the reading of Arago's report, Laplace, "who had thought for a long time that his analysis had made the phenomena of double refraction depend on his emission theory," proclaimed the great importance of the memoir, and declared that he placed these researches above anything that had for a long time been communicated to the Academy ('Œuvres de Fresnel,' vol. i. p. lxxxvi., and vol. ii. p. 459). We are indebted to M. Verdet for having shown that the discovery of this law by Fresnel is independent of the theoretical considerations by which he tried synthetically to prove it. On this point he says : "En révélant la série de généralisations et de conjectures par lesquelles Fresnel est arrivé peu à peu à la découverte des lois générales de la double réfraction, ils font disparaître une difficulté qui ne pouvait manquer de résulter de toute étude tant soit peu approfondie de ses écrits imprimés. . . . On a vu au contraire que cette loi s'est manifesté à Fresnel comme le résultat d'une généralisation toute semblable aux généralisations qui ont amené la plupart des grandes découvertes. Lorsqu'il a voulu ensuite se rendre compte de la loi par une théorie mécanique, il n'est pas étonnant qu'il ait, peut-être à son insu, conduit cette théorie vers le but qu'il connaissait d'avance, et qu'il ait été déterminé, dans le choix des hypothèses auxiliaires moins par leur vraisemblance intrinsèque que par leur accord avec ce qu'il était en droit de considérer comme la verité" (ibid., vol. ii. p. 327. Cf. vol. i. p. lxxxiv.)

[1] Works, vol. i. p. 361.

England and to Young, who learnt from them that, mainly owing to Fresnel's labours, his own researches had "attracted much more notice in Paris than in London, . . . leading to some very warm discussions among the members of the Institute on some public occasions."[1] It is likely that this visit, as well as the discovery of Arago that rays of light when polarised —*i.e.*, possessed of laterality—lose under certain conditions their power of interference, induced Young to resume seriously the consideration of the subject. In

19. Young and Fresnel introduce the conception of transverse vibrations.

January 1817, long before Fresnel had made up his mind to adopt a similar conclusion (suggested to him by Ampère), Young announced in a letter to Arago that in the assumption of transverse vibrations, after the manner of the vibrations of a stretched string, lay the possibility of explaining polarisation or "laterality," and the non-interference of rays whose sides are perpendicular to each other. By introducing this conception of a lateral or transverse movement into physical optics —a conception shortly afterwards adopted by Fresnel— the data were provided for a complete mechanical or kinetic explanation of all phenomena of homogeneous rays of light—*i.e.*, of such rays as, on passing through refracting substances, are not divided into several colours.

Two great problems now presented themselves, one of which Fresnel attacked with great success. The other is hardly yet solved. Inasmuch as these two problems have largely occupied physicists and mathematicians all through the century, and guided their reasonings in other

[1] Peacock, 'Life of Young,' p. 389.

branches of research, it will be useful to define them more clearly.

Ever since Newton laid down the general laws of motion, it has been seen with increasing clearness to be the object of mathematical physics to describe the existing observable or supposed forms of motion in nature by having recourse to the fundamental laws of motion coupled with the smallest possible number of assumptions as to the ultimate constitution of matter or of the moving substance. As soon as any definite assumption was made, it became necessary to follow it into all possible consequences, and not to make any new assumptions so long as the capabilities of the old ones were unexhausted, or so long as it was not shown either that the new assumption was based upon observable facts, or did not involve latent contradictions with those already admitted. Newton had led the way by making one great assumption in addition to laying down the laws of motion. This was the property of gravitation. Heedless of Newton's warning that this assumption, though proved by experiment, did involve certain seeming absurdities which called for further examination, philosophers like Boscovich, and mathematicians like Laplace, busied themselves with drawing all the consequences of the assumption, and they saw the most hopeful way of further progress in an extension of it into the realm of molecular physics. Young was probably one of the first to see the futility or the mere semblance of truth in the astronomical view of nature. He approached both by experiment and mathematically the great class of phenomena of small, extremely rapid, periodic move-

ments; and he applied his results for the purpose of gaining a new basis for the theory of light. His speculations were, however, not confined to this. He had started by studying sound and had shown its analogy with light; but when he ultimately ventured on the bold assumption of a lateral to-and-fro tremor, he showed where the nature of light differed from that of sound.

20. Mechanical difference between light and sound.

It was in this: that the tremor of sound was that of an elastic fluid such as air, or of any substance in which the movement is carried forward by alternate compression and expansion. But the phenomena of light seemed to require for their explanation two seemingly incompatible assumptions: first, a substance more subtle than air, incapable of impeding the motion of matter in it; and, secondly, a substance having vibrations resembling the tremors of what we term solid bodies, *e.g.*, stretched strings. Young is one of the founders of the theory of elasticity.[1]

[1] The history of the theories of elasticity has been written by Isaac Todhunter and continued by Professor Karl Pearson. A perusal of the earlier portion of the work shows how imperfect were the ideas which existed at the time when Fresnel approached the problem in the interest of the wave theory of light. The greatest mathematicians, like Euler, had handled the subject, and had damaged their reputation, especially in this country, by serious errors or by conclusions which agreed ill with experience. Young was one of the earliest writers on elasticity in the nineteenth century; having given considerable attention to the subject in his Lectures on Natural Philosophy (delivered in 1802, published in 1807). He there introduces the modulus of elasticity, a term which, with some change of meaning, survives in modern treatises. His name, as well as that of Hooke ("Ut tensio sic vis"), appears accordingly at the portal of the science. Young, though Todhunter has a significant remark on his obscurity of style, stands out prominently, if compared with contemporary writers in this country, by his thorough knowledge of the labours of Continental mathematicians, among whom he assigns special merit to Coulomb. In general, Todhunter has little to say in praise of English science in this department during the earlier part of the century, and he considers the "perusal of English text-books on practical mechanics published in the first half of the century a dispiriting task," in consequence of a "want of clear thinking, of scientific accuracy, and of knowledge of the work accomplished abroad" (vol. i. p. 105). "It is difficult to picture the remarkable scientific ignorance of practical men in England in the first quarter of the century. One can only trust that there may be a closer union of practice and theory in our own day" (p. 106). This passage was probably written in the 'seventies.

According to Todhunter, the true theory of elasticity was founded in France between the years 1820 and 1830, by Navier, Poisson, and Cauchy on the one side; by the experimental work of Savart on the other. It had been allied with theoretical acoustics since Euler's time. Chladni in Germany furthered that branch of the subject in three celebrated works: 'Theorie des Klanges' (1787), 'Akustik' (1802), 'Beiträge zur Akustik' (1817). Chladni influenced the brothers Weber, whose 'Wellenlehre auf Experimente gegründet' appeared in 1825. In it wave-motion, such as the theories of sound and light had made specially interesting and important, was experimentally examined and illustrated. The theory of elasticity now received a new ally, viz., the elastic theory of light or of the ether. Though suggested by Fresnel, its real founder was Cauchy.

He must have fully realised the difficulty of imagining a substance more subtle than air and yet endowed with the property of rigidity, known to us only in solid bodies. The elaboration of the theory of light pressed upon physicists and mathematicians a more careful study of the different states in which matter can exist. The different properties which this hypothetical substance called ether must possess had to be mathematically defined; and, further, it had to be shown whether it would be physically possible for a body, subject to the empirical laws of motion, to possess certain of the properties of what we term solids, and yet to be in other respects the very opposite of a solid. The solution of the first problem was a purely mathematical performance, in which many eminent mathematicians, such as Cauchy, Neumann, Green, M'Cullagh, and Stokes,[1] have been

21. The properties of the ether.

[1] The natural philosopher to whom we are most indebted for bringing clearness and definiteness into our ideas and our language in these very intricate subjects is Sir George Stokes. In two papers, published respectively in 1845 and 1849 (see 'Mathematical and Physical Papers,' vol. i. pp. 75-129, and vol. ii. pp. 8-13), he has done more than any other writer to fix for nearly half a century the conceptions and the

engaged. The solution of the latter problem involved experiment as well as calculation. The different states and properties of matter had to be studied from quite novel points of view: they had to be defined in terms of the different kinds of motion and of inertia, *i.e.*, resistance to motion or capacity for motion. The popular conceptions of solidity, rigidity, fluidity, expansion, pressure, weight, required to be translated into the language of ordinary dynamics, that it might appear to what

vocabulary of physical optics. He has, however, whilst working independently, been careful to point out to what extent his views agree with or are anticipated by the important writings of Cauchy and Poisson in France. Up to his time the ether was universally spoken of as a fluid. Stokes led up to the "elastic solid" and the "jelly" theory of the ether. "Undoubtedly," he says, "it does violence to the ideas that we should have been likely to form *a priori* of the nature of the ether to assert that it must be regarded as an elastic solid in treating of the vibrations of light. When, however, we consider . . . the difficulty of explaining these phenomena by any vibrations due to the condensation and rarefaction of an elastic fluid such as air, it seems reasonable to suspend our judgment and be content to learn from phenomena the existence of forces which we should not beforehand have expected. . . . The following illustration is advanced, not so much as explaining the real nature of the ether, as for the sake of offering a plausible mode of conceiving how the apparently opposite properties of solidity and fluidity which we must attribute to the ether may be reconciled. Suppose a small quantity of glue dissolved in a little water so as to form a stiff jelly. This jelly forms, in fact, an elastic solid: it may be constrained . . . and return to its original form when the constraining force is removed, by virtue of its elasticity; but if we constrain it too far it will break. Suppose now the quantity of water to be 'increased' . . . till we have a pint or a quart of glue-water. The jelly will then become thinner. . . . At last it will become so far fluid as to mend itself again as soon as it is dislocated. Yet there seems hardly sufficient reason for supposing that at a certain stage of the dilution the tangential force whereby it resists constraint ceases all of a sudden. In order that the medium . . . should have to be treated as an elastic solid, it is only necessary that the amount of constraint should be very small. The medium would, however, be what we should call a fluid as regards the motion of solid bodies through it. . . . Conceive now a medium having similar properties, but incomparably rarer than air, and we have a medium such as we may conceive the ether to be, a fluid as regards the motion of the earth and planets through it, an elastic solid as regards the small vibrations which constitute light" ('Papers,' vol. ii. p. 11 *sqq.*)

extent these various properties could exist separately or were mutually dependent.[1]

In the domain of sound and light the early part of the century was thus, as we have seen, witness of a useful interpretation of these various modifications as merely different kinds of motion: both were considered to be vibrations, the frequency of which marked the position of a note or a tint in the musical or chromatic

[1] That is to say, the number of independent constants had to be fixed which would permit isotropic or anisotropic bodies (*i.e.*, bodies which are either equal in all directions, or unequal in the three directions) to be mathematically defined, and in consequence their behaviour studied, if subjected to strains and displacements. Over these definitions there arose the great controversies of those who believed in a small number of constants (one constant in isotropic and fifteen in anisotropic bodies against two and twenty-one respectively). A good account of these controversies and of their mathematical and physical significance will be found in the first volume of Todhunter's 'History of Elasticity,' by Professor Karl Pearson, p. 496 *sqq.* The former theory is termed the rari- (few) constant theory, the latter the multi- (many) constant theory. The rari-constant theory is based upon the assumption that a body consists of molecules, and that the action between two molecules . . . is in the line joining them. It is an outcome of the atomic and action-at-a-distance theory in vogue on the Continent, and is accordingly mainly represented by Naiver, Poisson, Cauchy, and others, notably Saint-Venant. The other school, mainly represented by mathematical physicists in this country, starts not from a mathematical formula (which, after all, loses its precision as the active forces are reduced to the vague statement that they act sensibly only at insensible distances) but from physical data. It is an analogue to Young's theory of capillarity as against Laplace (see above, p. 20, note). "The somewhat unsatisfactory nature of the results of those investigations produced, especially in this country, a reaction in favour of the opposite method of treating bodies as if they were, so far at least as our experiments are concerned, truly continuous. This method, in the hands of Green, Stokes, and others, has led to results the value of which does not at all depend on what theory we adopt as to the ultimate constitution of bodies" (Clerk Maxwell, 'Scientific Papers,' vol. ii. p. 253). "After the French mathematicians had attempted, with more or less ingenuity, to construct a theory of elastic solids from the hypothesis that they consist of atoms in equilibrium under the action of their mutual forces, Stokes and others showed that all the results of this hypothesis, so far at least as they agreed with facts, might be deduced from the postulate that elastic bodies exist, and from the hypothesis that the smallest portions into which we can divide them are sensibly homogeneous" (id. ibid., p. 449).

scale, and the amplitude or height of the wave-motion of which decided its intensity. There was floating about the vague idea that heat also was to be interpreted as a mode of motion; still vaguer were the kinetic notions as to electricity and magnetism; whilst some early attempts to explain gravity, not as an inherent property of matter, but as a consequence of the motion of matter itself, which was possessed merely of inertia, had been half forgotten.

22. Other kinetic theories.

There is no doubt that the successful development of the undulatory theory of light induced many minds to dream of an ultimate kinetic explanation or interpretation of all natural phenomena, when in the course of the third quarter of the century this direction of thought received a great impetus through three independent branches of research of a purely theoretical kind. These have led to a very remarkable development of the kinetic view of nature; in fact it is mainly through them that this view has become possible not only in special departments, but on a universal scale. They have, each in its own way, led to a great extension of our experimental knowledge; one of them has likewise led to many practical applications. What most interests us here is the peculiar direction which they have given to a great volume of mathematical and physical thought of our day.

23. Kinetic theory of gases.

The first of these lines of research was connected with, and grew out of, the atomic hypothesis. It culminated in the kinetic theory of gases, in which the names of Joule, Clausius, and Clerk Maxwell are prominent. Of this I have treated already in the fifth chapter. It rests on a study of the average effect produced by a

swarm of bodies, subject to a transverse movement in straight lines like projectiles, and continually encountering each other on their way. The second line of research in question is the study of bodies subject to rapid movement round an axis, but immersed in a medium which is itself movable like water, but not in a rotary but merely in a flowing motion. The whole series of investigations which started by defining vortex or whirling motion as distinct from transverse, flowing, or projectile motion, and from vibratory to-and-fro motion, was initiated by Helmholtz in 1857 in a purely mathematical paper, and then applied and greatly extended by Sir William Thomson in the conception of the vortex atom.

24. Vortex motion.

The third branch of research had its origin in experimental investigations carried on for many years on peculiar lines, and quite independently, by Faraday; it was put into mathematical language by Clerk Maxwell in his celebrated treatise on electricity and magnetism which appeared in 1872. It will be my object to show in how far these different investigations have confirmed and developed the kinetic view of natural phenomena. But before doing this it will be well to realise what specific problems presented themselves to theoretical physicists when once the undulatory conception of light had taken hold of their minds; what peculiar difficulties were involved; and into what distinct new lines of reasoning they were conducted.

25. Faraday's researches.

We saw above that when the gravitational explanation of a large class of phenomena had a century earlier gradually gained ground, a great variety of researches was suggested by it, and new lines of reasoning opened

out, which in the course of the eighteenth century combined to establish what I termed the astronomical view of nature. The undulatory theory of light, established by Young and Fresnel during the first quarter of the nineteenth century, was a breaking away from what then seemed to many Continental philosophers a promising line of thought, a unifying principle in natural philosophy. As long as light was thought to consist of particles, however minute, which were projected from luminous centres, the mechanical laws of impact, of attraction and repulsion, could be applied; and they went a considerable way in apparently explaining the ordinary phenomena of light, such as motion in straight lines, reflexion, and refraction. They failed indeed in the case of diffraction or inflection, and still more in those phenomena which were misleadingly grouped under the term polarisation. The new theory seemed specially adapted to these more recently discovered phenomena, but it had to be admitted that the explanation of reflexion and refraction of light at the surface of polished, transparent, or opaque bodies met with considerable difficulties.

26. Problems as to the nature of the ether.

The new theory had introduced the conception of an all-pervading, apparently imponderable substance, the ether. The reintroduction of this conception into physical science was repugnant to many thinkers of the then prevailing school,[1] and it became more so when it had—

[1] One of the crucial tests for deciding between the corpuscular and the wave theory of light was the relative speed with which light travels in air and in water, *i.e.*, in a refracting substance. Foucault, in 1850, by a very ingenious method, improved since by Mitchelson, measured the speed of light in various media. He proved that light moves faster in air than in water, whereas on the corpuscular theory the speed of light in water must be to its speed in air as 4 to 3 approximately. "This finally disposed of the corpuscular theory"

for the purpose of serving as the carrier of a definite kind of wave-motion—to be endowed with most mysterious, seemingly contradictory properties.[1] Nevertheless the development of this conception, the desire to define more minutely the properties of this fictitious substance of which we have no direct perception, came in the course of the century to guide more and more the work of experimentalists as well as theorists. We meet with objections in the beginning, when the conception was first introduced, such as were urged by many chemical philosophers when Dalton reintroduced and formulated

(Tait, 'Light,' p. 192). Sir G. G. Stokes tells us "that in a course of conversation with Sir David Brewster, who had just returned from France, where he witnessed the celebrated experiment by which Foucault had just proved experimentally that light travels faster in air than in water, he asked him what his objection was to the theory of undulations, and he found he was staggered by the idea *in limine* of filling space with some substance merely in order that 'that little twinkling star,' as he expressed himself, should be able to send his light to us" ('Burnett Lectures on Light,' p. 15).

[1] It is known that the two philosophers who in the middle of the century did more than any others to introduce the positive or exact spirit into general thinking and into philosophical literature, Auguste Comte and John Stuart Mill, were both opposed to the theory of an ether. Huxley, in speaking of Comte, exclaims: "What is to be thought of the contemporary of Young and of Fresnel who never misses an opportunity of casting scorn upon the hypothesis of an ether—the fundamental basis not only of the undulatory theory of light, but of so much else in modern physics, and whose contempt for the intellects of some of the strongest men of his generation was such that he puts forward the mere existence of night as a refutation of the undulatory theory?" (See 'Philosophie Positive,' vol. ii. p. 440, and Huxley, 'Lay Sermons,' p. 134.) The fourteenth chapter of Mill's 'System of Logic,' written originally in the beginning of the 'forties, but subsequently annotated with reference to some of Whewell's criticisms, contains a lengthy discussion of the hypothesis of an ether. Mill says (vol. ii. p. 21, seventh edition): "What has most contributed to accredit the hypothesis of a physical medium for the conveyance of light is the certain fact that light travels, that its communication is not instantaneous but requires time, and that it is intercepted by intervening objects. There are analogies between its phenomena and those of the mechanical motion of a solid or fluid substance. But we are not entitled to assume that mechanical motion is the only power in nature capable of exhibiting these attributes."

the atomic view of matter. Similar uncertainties in the definitions exist in both theories all through the century, down to the most recent times. There are those who still look upon both conceptions as merely convenient symbolisms, as ideal instruments of thought or scientific shorthand; and on the other side we have it as emphatically stated, that the question, What is ether? "is *the* question of the physical world at the present time," "that it is not unanswerable," in fact, "that it is not far from being answered," that "it is probably a simpler question" than the other question, What is matter? [1] The whole domain of physical science is even divided into two portions, the physics of matter and the physics of ether,[2] and the older, more empirical, and common-sense divisions, treating separately of light, electricity, and magnetism, are assembled in one great doctrine, the "doctrine of the ether." It is, indeed, somewhat astounding, if not disheartening, to hear at the same time from an authority who has done more than any other living philosopher to enlighten us in these

[1] Professor O. Lodge, in the Preface to the first edition of 'Modern Views of Electricity,' p. xi. "It is simpler," he continues, "partly because ether is one, while matter is apparently many; partly because the presence of matter so modifies the ether that no complete theory of the properties of matter can possibly be given without a preliminary and fairly complete knowledge of the properties and constitution of undisturbed ether in free space. When this has been attained, the resultant and combined effect we call matter may begin to be understood."

[2] See *inter alia* Professor Paul Drude's 'Physik des Aethers' (Stuttgart, 1894). In the Preface, p. vi, he speaks of the philosophical "desire of using the same fundamental conceptions for the physics of the æther as for the physics of matter, whereby it remains an open question whether it is more serviceable to reduce the equations in the physics of the æther to those expressions which can be got from the observable phenomena in the physics of matter (the equations of dynamics), or whether the opposite road can be chosen with advantage."

matters, that at the present moment he knows as little as to the true nature of these agencies or substances as he did fifty years ago.[1]

Viewed from the position which we occupy in this history of thought—*i.e.*, in relation to the development of ideas—the conception of an ether has, however, like the atomic theory, had the most marked influence on scientific research and reasoning. In digging for a hidden treasure, in trying to describe the atoms or the ether, many practically useful conceptions, applicable to tangible phenomena, have been discovered. The atomic theory led at once to an enormous increase of our knowledge of different forms of matter, the knowledge of the elements, and of their innumerable possible compounds. The conception of the ether has led similarly to an enormous extension of knowledge of the different possible forms of motion. It is in this sense that we are greatly indebted to these abstract conceptions: both have guided our ideas in trying to understand and grasp the endless variety of phenomena. Let us see how from the early years of the undulatory theory of light our knowledge regarding the different forms of motion has grown, how that theory has contributed to the kinetic view of nature.

[1] Lord Kelvin, in referring to fifty years of scientific labour, said (see the publication by James Maclehose & Sons of the proceedings at his jubilee in 1896, p. 70): "I know no more of electric and magnetic force, or of the relation between ether, electricity, and ponderable matter, or of chemical affinity, than I knew and tried to teach to my students of natural philosophy fifty years ago in my first session as professor. Something of sadness must come of failure; but . . . what splendid compensation for philosophical failures we have had in the admirable discoveries by observation and experiment on the properties of matter, and in the exquisitely beneficent applications of science to the use of mankind with which these fifty years have so abounded."

It was recognised by Young, and still more clearly by Fresnel, that the medium which they supposed to be the carrier of light could not have the ordinary properties of either a solid, a liquid, or a gas. It offered apparently no resistance to the motion of the heavenly bodies, its waves were not like those which in air produced sound; it propagated its waves at a speed much greater than any other velocity known at that time; at the same time the wave-motion was not that of a body possessing the properties of a gas—*i.e.*, an elastic, compressible fluid: it was that of a body offering resistance to change of form rather than to change of bulk. It was evident that the different properties, which we see roughly assembled to constitute the three forms of ponderable matter with which we are practically acquainted, the solid, the liquid, and the gaseous, cannot be assembled in any similar manner in this imponderable substance, the ether. It was bound to have inertia—*i.e.*, mass—otherwise the laws of motion could not be employed in dealing with it, and mathematical thinking about it would be impossible. A more perfect description of the elementary movements which constituted light evidently required a minute experimental study, and a closer mathematical definition of the different properties of matter, known popularly but not very clearly under the terms compressibility, rigidity, mobility, elasticity, viscosity, &c., and of the inter-dependence of these clearly defined properties one on the other. Just about the time when the vibratory theory of light began to be seriously entertained by natural philosophers, a beginning had also been made in this study: the theory of elasticity had been founded in

27.
The theory of elasticity.

France by Navier and Poisson. One of the greatest analysts of the century, Augustin Cauchy, had likewise applied himself to it; and when Fresnel, in the year 1826, brought out his great memoir on double refraction in crystals, in which he was obliged to enter more closely into the properties of the luminiferous ether and its relation to ponderable matter, Cauchy was induced to devote himself more specially to the mathematical problems which presented themselves. Before his time the theory of elasticity had been studied more as connected with questions of practical engineering, such as the strength of materials, the stability of buildings, the construction of machines, or with the properties of musical and sounding bodies. A new interest was created by Fresnel's researches.[1] The question arose, How are we to describe the vibrations of an imponderable substance, endowed with mass (density) and rigidity, and what conceptions can we form of the change of these vibrations if there is present likewise ponderable matter? Evidently upon the clearness and correctness of these notions depends the explanation of the phenomena observable when rays of light fall upon the surfaces of transparent or opaque bodies. We have to ask: In what terms (viz., of different kinds of motion) can we define and describe, and accordingly calculate the phenomena of reflexion, refraction, scattering (*i.e.*, dispersion), and absorption (*i.e.*, extinction) of light? A tolerably clear

[1] See Verdet in 'Œuvres de Fresnel,' vol. i. p. lxxx: "Les seuls écrits antérieurs à Fresnel où l'on trouve des notions justes sur les inégalités d'élasticité qui peuvent exister dans les corps et sur leur répartition régulière par rapport à certains axes ou plans de symétrie sont ceux du grand minéralogiste allemand Samuel Christian Weis" ('Mém. de l'Acad. de Berlin,' 1815).

definition of the kind of motion constituting a pencil of homogeneous light in the free ether or in atmospheric air had been given by Fresnel. Experimentally the velocity of a wave-motion of this kind was known; it was subsequently ascertained that this speed was not the same in air as in the free ether, the so-called vacuum. It was also known that this speed in an elastic medium, such as the ether was supposed to be, depends upon the density and the rigidity of the medium. But when rays of light—*i.e.*, the wave-motions of the ether—arrive at the surface of liquid or solid bodies, various changes are known to take place. These changes had been to some extent described and brought into measurable terms by experiment, and it had been shown in a general way by Huygens, and more completely by Fresnel, how these observed changes of reflexion, refraction, and dispersion could be translated into the language of the vibratory theory. Complicated and yet very elegant geometrical constructions, at which Fresnel arrived by an intuitive or tentative process,[1] enabled the course of rays inside transparent, doubly-refracting substances, such as crystals, to be calculated; a whole geometry of rays was developed out of these representations; now phenomena

[1] The equation of the wave-surface was not explicitly given by Fresnel himself. M. Verdet says ('Œuvres de Fresnel,' vol. i. p. lxxv): "Fresnel n'a pu lui-même venir à bout de ces difficultés et n'a su obtenir l'équation de la surface de l'onde qu'en la supposant *a priori* du quatrième degré, et calculant la valeur de ses coefficients de manière qu'ils satisfissent à certaines conditions faciles à déduire de la considération des ondes planes normales aux trois plans de symétrie du milieu. Ampère est le premier qui ait effectué le calcul d'une manière rigoureuse." However, "the construction yields the wave-surface in such a way that its singularities are not obvious, and were only remarked by Sir W. R. Hamilton several years after Fresnel's death" (Fletcher, 'The Optical Indicatrix,' p. 31).

of refraction, such as conical refraction, were mathematically foretold and experimentally verified.[1] The real physical question, however, remained unanswered; and it remains only partially answered up to the present day.[2] How is it that the luminiferous ether, when existing inside ponderable matter, like air permeating a grove of trees—as Young put it—is so changed that its waves travel with variously altered speeds, that in different directions the rays acquire different properties, are differently maintained or partially extinguished (absorbed)? It was natural to suppose that the particles of ponderable matter must in some way affect the ether, changing its density or its rigidity, and that they themselves are affected by the movements of the ether which fills their interstices. The question can only be exhaustively answered by a complete know-

[1] The subsequent suggestion of the phenomena of inner and outer conical refraction, experimentally verified by Humphrey Lloyd in 1833 (see his 'Miscellaneous Papers,' No. 1, or Transactions, Royal Irish Academy, vol. xvii.), was popularly regarded as a complete proof of the correctness of the wave-surface, and of Fresnel's entire theory. But as to the first point, Sir G. G. Stokes showed (Brit. Assoc. Report on Double Refraction, 1862, p. 270) that conical refraction "must be a property of the wave-surface resulting from any reasonable theory." And as the wave-surface itself can be geometrically constructed without any reference to the mechanical theory of the ether (as Mr Fletcher has most exhaustively shown), the prediction of conical refraction cannot be regarded as a proof of Fresnel's theory. Todhunter-Pearson says: "But for Cauchy's magnificent molecular researches, it might have been possible for Fresnel to completely sacrifice the infant theory of elasticity to that flimsy superstition, the mechanical dogma, on which he has endeavoured to base his great discoveries in light. Cauchy inspired Green, and Green and his followers have done something, if not all, to reconcile Fresnel's results with the now fully developed theory of elasticity, the growth of which his dogma at one time seriously threatened to check" ('Hist. of Elasticity,' vol. i. p. 167).

[2] In 1862 Sir G. G. Stokes "expressed his belief that the true dynamical theory of double refraction had yet to be found" (Report, p. 268).

ledge of the mechanism of the ether on the one side, of ponderable matter on the other. Two ways are open by which a solution of this ultimate or fundamental problem can be solved. The one is purely mathematical. It means the analysis of all the possible modes of motion of a given mechanical system, and of the mutual influence which two interconnected mechanical systems, that of the ether and that of ponderable matter, exert on each other. This is a perfectly definite though a very intricate problem. It is a problem which can be compared with—though it transcends in complexity—the analytical problem suggested by the gravitational view of physical astronomy: to calculate mathematically the movements of any number of bodies attracting each other according to Newton's formula. The other way is the experimental method — to observe how under methodically altered conditions rays of light are modified in colour (wave-frequency), in direction, in intensity (amplitude of wave-motion), in laterality (polarisation), and in other ways; and then to translate these conditions and alterations into the now fairly well-established language of the vibratory theory; gaining in this way indications as to the changes which the wave-motion is capable of, and inferring from these possible changes the original constitution (usually called the constants) of the primary substances—the ether and the ponderable matter which come into interaction.

28. The problem of the ether may be treated mathematically,

29. or experimentally.

30. Necessity of combining the two methods.

It may in general be stated that neither of these two methods has for any length of time been pursued alone, but that progress has nearly always depended upon an alternating employment or a combination of both. On the one side we have a great volume of purely analytical reasoning begun by Cauchy in France, and pursued under varying assumptions by Green and MacCullagh in England, by F. Neumann and others in Germany. On the other side we have the purely experimental work beginning with Wollaston and Brewster in England, the refined methods for measuring the speed of light invented by Fizeau and Foucault, the beautiful contrivances for experimental research and verification of Jamin and many others. Out of so many fruitful conceptions which have resulted in an enormous accumulation of new knowledge of actual phenomena of light and wave-motion —the real and sole end and aim of all theory—I will for the purpose of illustration single out one which in the middle of the century opened out an entirely new field of inquiry, forming almost a new science by itself. I refer to spectrum analysis.

31. Spectrum analysis.

The phenomena of dispersion (rainbow scattering) and absorption (partial or complete extinction) of light were among the earliest known, and had been among the longest studied, properties of bodies. Being, besides, connected with the physiological, subjective, and artistic effects of light, they have always commanded special interest. And yet, so far as either the emission or the undulatory theory is concerned, they have always presented special difficulties. When the wave theory was first propounded, it was generally understood on the analogy of the phenomenon of sound that difference of colour depends upon difference of frequency, or where the velocity of propagation (as *in vacuo* or in atmospheric air) is the same, on the length of the waves. The diffi-

culty arose of explaining how in refracting substances, be they fluid, amorphous (singly refracting), or crystalline (including doubly refracting), these different rays, with different wave-lengths, come to travel with different velocities, and hence take different courses; how, further, some of these rays come to be extinguished or reflected (or both) in varying degrees.

Now, although the complete answer to this general question has not yet been given, a principle has been recognised which gives us a clue to the possible explanation of a large class of phenomena, and which is thus of remarkable fruitfulness. It was first laid down by Euler,[1] a pure mathematician, whose physical reasoning was frequently suggestive but never particularly clear and definite; it was probably first applied to optical phenomena by Sir George Stokes;[2] and it was later on used by him

[1] In the last section of his treatise on light and colours ('Berlin Memoirs,' 1745; published in Latin, 1746), Euler treats of luminous, reflecting, refracting, and opaque bodies, and he there mentions the analogy which exists with musical resonance. "The smallest particles [of opaque bodies] are similar to stretched strings, which are, as it were, specially receptive for certain vibrations, which they can assume without being struck, if only they are affected by the undulatory movement of the air. "In his expositions upon light and colours, Euler always starts with the analogy of sound and light; he follows it with absolute consistency" (Cherbuliez, 'Euler's physicalische Arbeiten,' p. 44). This analogy was exactly what was absent in the investigations of Brewster, who remained to the end an adherent of the projectile theory. Balfour Stewart came nearest to the true explanation in his memoir of 1858 ('Trans. of the Royal Society of Edinburgh,' 1861); but this referred to radiant heat and to Prévost's theory of exchanges. It contains the words: "The absorption of a plate equals its radiation, and that for every description of heat" (p. 13). Had this statement been distinctly applied to luminous rays, spectrum analysis would have been his discovery, although his theoretical proof might be regarded as insufficient (see Scheiner's treatise on Astronomical Spectroscopy, transl. by Frost, 1894, p. 112; also Rosenberger's 'Geschichte der Physik,' vol. iii., 1890, p. 482 *sq.*)

[2] See the references given on p. 277 of the first volume of this history.

in giving a mechanical explanation of the dark and bright lines of the spectrum, upon which Kirchhoff and Bunsen founded spectrum analysis about the year 1860. 32. The clue furnished by the phenomena on which it depends.

Wollaston[1] had in 1802, on examining the solar spectrum (the succession of rainbow colours expanded on a white screen placed behind a prism of white glass through which a narrow beam of sunlight is made to pass), noticed that with a sufficient enlargement black lines in great number could be detected. Fraunhofer,[2] in Munich, made a special study of them, named them by letters of the alphabet, and compared the solar spectrum with the spectra of artificial terrestrial sources where light is created by combustion or incandescence. He found that these spectra differed, the peculiar colour exhibited by various flames being defined in the spectra by special bright lines of different colours. Thus notably the two dark lines called by him D in the solar spectrum were replaced in the spectrum of a flame in which a volatile salt of sodium was present, by two bright lines: Brewster found the same coincidence of others of Fraunhofer's lines with the bright lines of a flame in which nitre was volatilised. Very similar and very accurate observations of A. Miller as to the identity of the dark lines D in the solar spectrum with the two bright lines of the sodium flame were explained by Sir G. Stokes about the year 1850 by the following theoretical reasoning: The sodium 33. Sir G. Stokes.

[1] "A method of examining refractive and dispersive powers by prismatic reflection" ('Trans. of the Royal Society,' 1802).

[2] Fraunhofer, whose epitaph, "approximavit sidera," describes beautifully his life-work, was led to the discovery of the lines named after him in his investigations of the "refractive and dispersive powers of various kinds of glass" for the purpose of improving the achromatic telescope ('Denkschriften der Münchener Akademie,' vol. i., 1814-15).

flame which emits the two bright lines in its own spectrum destroys them (replacing them by two dark lines) in the spectrum of a ray of light which passes through the sodium flame.[1] Foucault had in 1849 already shown the direct reversal of the sodium line in the spectrum of the electric arc. These earlier anticipations remained partly unnoticed, partly unknown, or were looked upon

34. Gustav Kirchhoff.

as isolated cases, and it was reserved for Gustav Kirchhoff to put this remarkable property of emission and absorption of special colours by coloured flames into practical language, and express it in a general way. He wrote in 1859:[2] "I conclude that coloured flames in the spectra of which bright lines present themselves, so weaken rays of the colour of these lines, when such rays pass through them, that in place of the bright lines, dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in which these lines are otherwise wanting." And when he concluded further that the dark lines of the solar spectrum which are not evoked by the atmosphere of the earth, exist in consequence of the presence in the sun's atmosphere of those substances which in the spectrum of a flame produce bright lines at the same place, "he at once gave

[1] From this he inferred that the presence of sodium vapour in the atmosphere of the sun would explain by absorption the two dark lines in the solar spectrum. Lord Kelvin reports that in consequence of this observation of Stokes he regularly taught his Glasgow students that sodium must be in the sun's atmosphere. See the reprint of the correspondence on this subject in the 'Gesammelte Abhandlungen' of Kirchhoff, 1882, p. 639, where it will also be seen that Sir W. Crookes claimed a similar anticipation for Miller in 1846. See also Sir W. Thomson's ninth Baltimore Lecture.

[2] See the translations of Foucault's and Kirchhoff's memoirs sent by Sir G. Stokes to the 'Philosophical Magazine' of March 1860, p. 194 *sqq*.

birth to two great applications of his principle—the search, through the study of the spectra of distant stellar sources of light, after the ingredients which are present in those distant luminaries, and the search, through the study of the flames of terrestrial substances, for new spectral lines announcing yet undiscovered elements."[1] Whilst in these two independent directions an enormous amount of new knowledge has been accumulated, the mechanical explanation through which Sir G. Stokes anticipated these phenomena, and the further applications of this principle by him, have done much to confirm the conviction, that in looking upon light as a vibratory mode of motion, we are on the road towards an adequate description of these phenomena.

[1] To this principle we owe the spectrum analysis of stellar atmospheres and the discovery of new chemical elements, of which no fewer than six have been identified by this method, beginning with cæsium and rubidium (found by Kirchhoff and Bunsen in the waters of some mineral springs). The suggestion of Doppler, mentioned above (p. 10, note), has only become fruitful through the invention of the spectroscope. Colour differences originating through the change of the frequency of vibrations depending on cosmical velocities in the line of sight, could not be discovered by the most sensitive eye. In the spectrum, however, shown by the spectroscope, "not only the colours of the bright lines have been altered, but their position in the spectrum relatively to a fixed point of reference as well. . . . The measurement of the displacement of spectral lines in consequence of the altered refrangibility of the rays is the only method yet known which possesses sufficient accuracy for determining the motions of objects in the line of sight. Thus far it has not been possible to produce in the laboratory velocities high enough to occasion a perceptible displacement of the lines" (Scheiner, *loc. cit.*, p. 148). And as Doppler's principle in acoustics was proved directly by Buys Ballot through the whistle on moving railway trains, so it has been proved directly in optics by observing the displacement in the lines of the solar spectrum, when this is derived from the outer rays of the sun's disc, the light-giving parts moving in the line of sight towards or away from the observer in consequence of the rotation of the sun round its axis. "The resulting velocity of the surface of the sun is found to agree very closely with the results of direct observations of the revolution of the spots, thus practically furnishing a proof of the correctness of Doppler's principle" (ibid., p. 149).

We have seen above how the vibratory theory of light was arrived at—mainly in the hands of Young—through dwelling on the analogy of certain optical phenomena, notably those of interference, with the properties exhibited by sound. Among the latter none were more remarkable than those known popularly as consonance and resonance. Sir George Stokes, on the appearance of Kirchhoff's memoir on the relation of emission and absorption of certain rays of light, gave the mechanical explanation in the following words :[1] "In describing the result of a prismatic analysis of the voltaic arc formed between charcoal poles, M. Foucault 'found that the arc presents us with a medium which emits the rays D on its own account, and which at the same time absorbs them when they come from another quarter.' . . . The remarkable phenomena discovered by Foucault, and rediscovered and extended by Kirchhoff, that a body may be at the same time a source of light, giving out rays of a definite refrangibility, and an absorbing medium extinguishing rays of the same refrangibility which traverse it, seems readily to admit of a dynamical illustration borrowed from sound. We know that a stretched string which on being struck gives out a certain note, is capable of being thrown into the same state of vibration by aerial vibrations corresponding to the same note. Suppose now a portion of space to contain a great number of such stretched strings, forming thus the analogue of a 'medium.' It is evident that such a medium, on being agitated, would give out the note above mentioned, while on the other hand, if that note were sounded in air at a distance, the incident vibrations would

[1] 'Phil. Mag.,' March 1860, pp. 194, 196.

throw the strings into vibration, and consequently would themselves be gradually extinguished, since otherwise there would be a creation of *vis viva*. The optical application of this illustration is too obvious to need comment."

Already ten years before Kirchhoff gave to the researches into the spectrum their popular celebrity and practical importance, Stokes[1] had made an extensive ex-

[1] The memoir of Sir G. Stokes "on the change of the refrangibility of light," in the 'Philos. Transactions' (May 1852), forms a landmark in optical science, and whilst dealing with the less obvious —though very frequent and general —phenomena of fluorescence and phosphorescence, really indicated the line of reasoning which has become so fruitful and suggestive in his own hands and in those of other eminent natural philosophers. On page 549 of that memoir he wrote: "All believers in the undulatory theory of light are agreed in regarding the production of light in the first instance as due to vibratory movements among the molecules of the self-luminous body. . . . Nothing then seems more natural than to suppose that the incident vibrations of the luminiferous ether produce vibratory movements among the ultimate molecules of sensitive substances, and that the molecules in turn, swinging on their own account, produce vibrations in the luminiferous ether, and thus cause the sensation of light. The periodic times of these vibrations depend upon the periods in which the molecules are disposed to swing, not upon the periodic time of the incident vibrations." Referring, then, to the dynamical difficulties which attach to such a view, he proceeds to point out "that we have no right to regard the molecular vibrations as indefinitely small. The excursions of the atoms may be, and doubtless are, excessively small compared with the linear dimensions of a complex molecule. It is well known that chemical changes take place under the influence of light, especially the more refrangible rays, which would not otherwise happen. In such cases it is plain that the molecular disturbances must not be regarded as indefinitely small. But vibrations may very well take place which do not go to the length of complete disruption and yet which ought by no means to be regarded as indefinitely small. . . . Certainly we cannot affirm that in the disturbance communicated back again to the luminiferous ether none but periodic vibrations would be produced having the same period as the incident vibrations. Rather, it seems that a sort of irregular motion must be produced in the molecules, periodic only in the sense that the molecules retain the same mean state; and that the disturbance which the molecules in turn communicate to the ether must be such as cannot be expressed by circular functions of a given period, namely, that of the incident vibrations." Stokes then refers to the probable internal vibration of the atoms in the compound molecules, as "it is chiefly among organic compounds . . . having a complicated structure that internal dispersion (fluorescence) is found."

amination into the question how vibrations of the luminiferous medium can be mechanically transferred to the compound molecules of a transparent body, and retransferred again to those of the ether itself — *i.e.*, the question of the absorption and emission of light. He showed that vibrations of a certain period, corresponding to a definite tint of colour, could eventually give rise to vibrations of altered period in the emitted light; that this period, however, must always be longer—*i.e.*, that the new colour must always be of a lower order in the scale of refrangibility. He was thus not only able to explain mechanically the peculiar luminosity which he termed fluorescence,[1] and which had been observed by Herschel and Brewster in certain minerals and solutions, and independently studied by E. Becquerel in France, but he also showed how, by means of such substances, rays of light which, owing to the frequency of their vibrations, transcend the perceptive powers of the human eye, can be made visible by giving rise to secondary waves of less frequency. The line of reason-

35. **Explanation of fluorescence.**

[1] The term fluorescence was coined by Sir G. Stokes by analogy with opalescence as involving no theoretical suggestion, in place of the earlier names of "internal dispersion" or "epipolised light" used by Brewster and Herschel. He, however, very soon favoured the term "degraded light," suggested by William Thomson (Lord Kelvin) (see the second memoir, 1853, p. 387). The latter was at that time occupied with his celebrated and not less epoch-making researches referring to the dissipation or degradation of energy, of which more in the next chapter. If we remember that fifty years ago the term radiation was not yet generally used to embrace the invisible chemical (ultra-violet) and caloric (infra-red) rays; that photography, which more than any other process has familiarised us with chemical radiation, was a comparatively recent invention; that the ideas of conservation, conversion, and degradation of energy were quite new; that the general term energy had not even been invented,—we must indeed regard the words of Sir G. Stokes as containing a prophetic programme of the ideas and problems of the whole subsequent period down to quite recent times.

ing here employed gave the clue to all subsequent attempts to deal with the difficult problem of the interaction of the ether and ponderable matter; of the possible alteration of the density or the rigidity (called the elastic constants) of the ether when filling the interstices of transparent bodies; of the mechanical differences which make some bodies transparent for some and opaque for other rays of light. Many possible modifications were theoretically foreseen, giving rise to remarkable unexpected phenomena, and these were frequently verified by subsequent experience. The whole theory of light entered upon a new phase as it became more and more evident that the study of the vibrations of the elastic medium was not sufficient, but that it must be supplemented by that of the interaction of two vibrating systems, the ether and the molecules of the ponderable substance, which give rise to the phenomena of partial reflexion, refraction, dispersion, and partial or complete absorption. This more complicated problem in the theory of elasticity had already presented itself in its simpler form in the theory of the pendulum. To the principle of optical consonance which had been employed to explain the phenomena of absorption of light was added, in order to explain the phenomena of dispersion, the principle of the free and forced vibrations of a vibrating system.[1]

[1] "If to the bob of a pendulum, executing horizontal vibrations, another pendulum be attached, executing vibrations of a slightly shorter period, the effect of the latter will be to increase the period of the former and *vice versâ*" (see A. S. Percival, 'Optics,' 1899, p. 181). Anomalous dispersion such as was foreseen by Sellmeier and Lord Kelvin and discovered by Christiansen and Kundt depends on the change of wave frequency independent of the change of wave length in refracting media.

36. View of the ether as an "elastic solid."

The latest discussion of this form of the elastic-solid theory of light, which was gradually developed from independent beginnings in the three countries,[1] is to be

[1] In France and Germany, where even in the middle of the century the labours of English natural philosophers like Green, M'Cullagh, Stokes, were only very imperfectly known, the necessity was equally felt of studying the interaction of the ether and ponderable matter. In France the school of the eminent "elastician," Barré de St Venant, produced in M. Boussinesq the author of the earliest published attempt to solve the difficulties which the older methods of Cauchy had not overcome. In a lucid review of the state of physical optics, Saint Venant himself ('Ann. de chimie et de Physique," 4^{me} série, vol. 25, 1872) hails with delight the researches of M. Boussinesq from 1865 onward, where the idea that the ether in the interstices of transparent bodies has different elastic constants is given up, and the participation of the ponderable matter in the vibrations is introduced in its place. "En effet," he says, "il est bien difficile de concevoir, d'une part, que l'éther puisse être agité au sein d'un corps dont la densité est probablement bien supérieure à la sienne, sans lui communiquer une fraction sensible de sa quantité de mouvement, et d'autre part, que les ondes ne soient pas bientôt éteintes par cette participation de la matière pondérable au mouvement s'il n'y a pas concordance entre les oscillations imprimées à chaque molécule de cette matière et celles de l'éther qui l'environne." It was the problem of the continuity at the interface of reflecting and refracting substances and the problem of absorption which the older simple ether theories could not explain. In Germany a similar impulse was given to the study of the interaction of elastic systems—as indeed to many problems of mathematical physics—by Franz Neumann, who was the centre of a numerous and influential school. He taught at Königsberg together with Richelot and Bessel. His lectures have been edited by his pupils. Prof. Karl Pearson, in his continuation of Todhunter's 'History of the Theory of Elasticity,' does ample justice to the labours of Neumann, who, "in his investigations on photo-elasticity and the elasticity of crystals, breaks almost untrodden ground, which both physicists and mathematicians have hardly yet exhausted" (*loc. cit.*, vol. ii. 2, p. 183). "Neumann was among the first (1841, 'Abh. der Berliner Akademie') to attribute dispersion to the influence of the ponderable particles on the particles of the ether" (ibid., p. 31). The most important original contributions of Neumann's pupils are the researches of Sellmeier, who had been led by theoretical considerations in 1866 to expect certain anomalies in the phenomena of dispersion, such as were in 1870 actually discovered by Christiansen, and fully investigated by Kundt. Surface coloration was shown to be intimately connected with the absorptive powers in substances showing these anomalous phenomena. A full report on these and other theories, based upon what has been termed abroad the "Bessel-Sellmeier hypothesis" (see Ketteler, 'Theoretische Optik,' 1885), will be found in Prof. Glazebrook's "Report on Optical Theories," Brit. Assoc. Reports, 1885.

found in Lord Kelvin's celebrated Baltimore Lectures,[1] where with unlimited resourcefulness the methods of analogy, analysis, and experiment are employed to solve or to define the intricate problems of physical optics. Nor is it a merely fortuitous coincidence for the history of thought that, whilst his mind must have been filled with the many illustrations and mechanical devices, and all the wealth of suggestions contained in the Baltimore Lectures, Lord Kelvin should have delivered the opening address to the mathematical section of the British Association, entitled, "Steps towards a Kinetic Theory of Matter." Following—as did also Clerk Maxwell—on the lines indicated by Stokes's earlier papers, he has done much to change our fundamental conceptions as to the properties of matter, and this in two distinct ways. The first consisted in breaking down the rigid barriers which popular definitions had set up between the different forms of aggregation—the solid, liquid, and gaseous states of matter; whilst the second tended to show how

37. Lord Kelvin's researches.

[1] The Baltimore Lectures were delivered by Lord Kelvin (then Sir W. Thomson) after the meeting of the British Association at Montreal in the month of October 1884, at the Johns Hopkins University, before a company of physicists. The final edition of these important and highly suggestive conferences is in the press as the fourth volume of the collected mathematical and physical papers. The completion of this publication is eagerly expected, as containing the most mature exposition of the elastic-solid theory of light, towards which the author has in the course of the last fifteen years made various valuable additions. Notably in a paper dated 1888, published in the 'Philosophical Magazine,' he has, as it has been said, "extricated the elastic theory from the position of deadlock, according to which the ether must be both compressible and incompressible," by showing that the difficulty can be met, "provided we either suppose the medium to extend all through boundless space, or give it a fixed containing vessel as its boundary." Prof. Glazebrook has further worked out the consequences of this suggestion. See vols. 26 and 27 of the 5th series of the 'Phil. Mag.,' also 'Nature,' vol. 40, 1889, p. 32, and Fletcher, the 'Optical Indicatrix,' p. 6, &c.

the supposed static properties of matter could be explained by different modes of motion, translational, periodic, or rotational. The mathematical and experimental investigations connected with the theory of radiations and vibrations had thus an influence[1] on our general views of the nature of physical processes which far exceeded the aims for which they were originally undertaken. That a substance so attenuated as the ether should have the properties of a solid; that brittle substances like pitch should flow like liquids, if only sufficient time were given; that towards very rapid impulses gases and liquids might behave as solids—all these observations resulted in a complete revolution of our scientific notions as well as of our vocabulary. The great turning-point, indeed, lay in the kinetic theory of gases, which about the middle of the century had introduced quite novel considerations by showing how the dead pressure of gases and vapours could be explained on the hypothesis of a very rapid but disorderly translational movement of the smallest particles in every possible direction. Pressure of gases having been explained by a very rapid motion of the minute particles of matter, heat was immediately conceived to be merely a "mode of motion." As no event did more to spread modern views in the theory of light, and to popularise modern scientific methods, than Kirchhoff's

[1] It has been asserted that the theory of elasticity received a great impulse when Fresnel was forced to make assumptions as to the mode of vibrations of the ether which were quite incompatible with the then accepted laws of the vibrations of an elastic medium. To this view of the origin of the modern theory of elasticity Prof. Karl Pearson takes exception, as Navier's memoir of 1827 was not suggested by optical investigations (Todhunter-Pearson, vol. ii. 2, p. 5).

and Bunsen's spectrum analysis, so in the closely related doctrine of heat, probably no publication did more to establish a general kinetic view of matter and of natural phenomena than Tyndall's celebrated treatise, 'Heat as a Mode of Motion.' In spite of the criticisms which have been levelled against this expression,[1] the book, which appeared in 1863, was to the popular mind a revelation; it was translated into many foreign languages, ran through many editions, was recommended by thinkers of the first order, and the title coveted as "manifesting far and wide through the world one of the greatest discoveries of modern philosophy."[2] It is the popular herald of the kinetic or mechanical view of nature.

38. Tyndall's 'Heat.'

39. Lord Kelvin's vortex theory of matter.

The same great authority who has so generously referred to Tyndall's treatise—Lord Kelvin—had been inspired from quite a different quarter to suggest the most advanced conception, in this line of thought, of which the human mind has so far been capable: the

[1] Notably by Prof. P. G. Tait; see his volume on 'Heat,' p. 350, also his 'Recent Advances of Physical Science,' which contains as an appendix his lecture on "Force," delivered in Glasgow on the occasion of the meeting of the British Association. He says there: "Heat and kinetic energy in general are no more *modes of motion* than potential energy of every kind is a *mode of rest.*' "Heat is not the mere motions, but the energy of these motions." There is no doubt that the terms force and motion can be used in very different meanings, and that the early expounders of the mechanical theory of heat have not been always consistent in the use of words; though their ideas, wherever they appeared in mathematical expressions, were definite enough. A good deal of vagueness has accordingly crept into popular text-books and into philosophical treatises, and criticisms such as those of Prof. Tait have been useful in helping us towards clearer conceptions. We shall come across more of these instances in the next chapter when dealing with the gradual evolution of the conception of energy.

[2] See Lord Kelvin's abstract of lecture, "Elasticity viewed as possibly a Mode of Motion," 1881; 'Popular Lectures,' &c., vol. i. p. 142. "I have always admired it" (viz., Tyndall's title); "I have long coveted it for elasticity, and now, by kind permission of its inventor, I have borrowed it for this discourse."

vortex theory of matter. As this is one of the most remarkable instances of the fruitful reaction of abstract mathematical reasoning on the progress of physical research, it will be useful to consider for a moment by what gradual steps this novel idea was evolved or suggested. The immediate occasion which led to it was

40. **Helmholtz's investigations.**

the publication, in 1858, by Helmholtz of a purely mathematical investigation of some peculiar forms of fluid motion.[1] About a hundred years before Helmholtz published his memoir, Euler had laid the foundation of theoretical hydrodynamics—*i.e.*, of the theory of the motion of fluids. In doing so, it was necessary to define

[1] Helmholtz's memoir, "Ueber Integrale der hydrodynamischen Gleichungen welche den Wirbelbewegungen entsprechen," appeared in the 55th volume of Crelle's 'Journal für die reine und angewandte Mathematik.' It was translated into English by Prof. Tait in the 'Philosophical Magazine' for 1867. Helmholtz's occupation with the subject had originated in the acoustical researches which he was carrying on at the time. These necessitated an analysis of the more complicated conditions which the motion of incompressible and elastic fluids presents in actual experience. The hydrodynamical equations had been solved under certain simplifying assumptions. Discontinuity of motion and internal friction had been left out of consideration. Helmholtz's researches led him to the study of these more complicated phenomena; and he successfully applied the mathematical methods which had proved useful in other branches of physical science for the solution of these problems. Notably in the paper on whirling motion, he came upon very remarkable and unexpected results, which ten years later led in this country to the novel speculations of Lord Kelvin. It is interesting to note how at that time researches in England or Germany could for many years remain unnoticed in the other country. The result was that the same problems were frequently taken up in ignorance of the fact that they had been treated before. See Hicks's "Report on Hydrodynamics," 'Brit. Assoc. Reports,' 1881-82. Especially the labours of Stokes seem to have been little known to German writers, who usually started from the better-known French researches. Stokes had anticipated some of Helmholtz's results referring to whirling and discontinuous motion of fluids. About the middle of the century the periodical "Fortschritte der Physik" was started by the "Physikalische Gesellschaft" of Berlin. Helmholtz himself contributed several valuable reports on acoustical subjects. See the 'Wissenschaftliche Abhandlungen,' vol. i. *passim*.

mathematically what is meant by a fluid. The chief property of a fluid, as compared with a solid body, is the perfect mobility of its parts, the absence of rigidity. Thus there were two possible kinds of fluids—those which retained their bulk or volume, whilst offering no resistance to change of shape, and those which tried to expand, and could be compressed by means of external forces. These latter were called gases. In dealing with the former, incompressibility had to be defined mathematically, as also perfect mobility. These properties constitute what is called a perfect fluid. Such perfect fluids do not exist in nature; but the method of reasoning was to begin with an ideal, simple case, and approach the explanation of natural phenomena by a process of correction, introducing more and more complications. The phenomena of the flow of liquids, practically by far the most important, could be studied to a great extent by means of the simplest form of the hydrodynamical conception, and up to the middle of the century such problems, as well as those of the propagation of small displacements under the action of external forces,—notably the motion of waves,—formed the principal problems which were treated mathematically. The idea of the friction of fluids, also called viscosity, had been excluded in the definition of a fluid, inasmuch as friction opposed the notion of perfect mobility of the parts, which was the mathematical definition of a fluid. Now it is a matter of experience that in all liquids with which we are acquainted friction can produce rotational motion, such as whirls and eddies; it was also found that other forces, such as magnetic forces, are, under certain con-

ditions, able to produce these rotations. It was therefore of interest to study the nature of rotational or whirling motion, if such could exist in a perfect liquid, and to see what would be likely to happen to these whirls. Though it might be difficult to understand how in a perfect liquid rotation of any portion could be produced, calculation might determine what would be the nature and fate of such whirls, if they did exist. The problem was a purely mathematical one. Can a rotational motion, a whirl, exist in a perfect fluid, as defined by the mathematical conception? If it can, what are the properties of such whirls, and what becomes of them? Helmholtz solved these questions in his now celebrated treatise, showing that whirls (called by English writers vortices) can exist, but only under certain conditions, such as can be experimentally represented by smoke-rings issuing from an orifice; that, if they existed in a perfect liquid, they would be indestructible and would possess a motion of their own, giving them a special individual character as to permanence and movement. The treatise, like the problem, was a purely mathematical one,[1] and in the mind of the celebrated author was probably connected more with the problem of the formation of drops, and with that of the friction or viscosity of fluids, which he attacked subsequently, than with the nature of matter. In this country vortex motion had already been studied by natural philosophers with very different ends in view.

It was known that solid bodies which are in a rapid

[1] It revealed incidentally the analogy of hydrodynamical and electrical phenomena.

41. Earlier researches on vortex motion.

rotary motion acquire properties which they do not possess otherwise—viz., rigidity—*i.e.*, reaction against change of shape (the stiffness of a travelling rope thrown off a pulley is a familiar illustration); stability—*i.e.*, reaction against change of position and motion, as in a spinning-top or a bicycle; elasticity—*i.e.*, tendency to revert to the same position, if violently disturbed. The gyroscope[1] had been invented in 1852 by Foucault, and used by him and other physicists in France and Germany to illustrate the rotation of the earth. It was now shown that portions of a perfect fluid—*i.e.*, of a body which possesses neither rigidity, nor stability, nor elasticity—when in a state of rapid rotational motion, acquire these gyrostatic properties; that whirling portions cannot be naturally created, but that if once in existence they preserve their identity, being permanently differentiated from the surrounding fluid, which may be at rest or in the state of flow. These differentiated portions of the liquid were called by Helmholtz vortex filaments; he showed that in a liquid without a boundary they must run back into themselves, forming rings which might be knotted and linked together in many ways.

[1] A much older invention was that of Bohnenberger (1817), known by his name. The name "gyroscope" was introduced by Foucault; and that of "gyrostat," as defining an apparatus which acquires stability through rotational (whirling or gyrating) motion, was used first by Lord Kelvin. An extensive treatment of the subject is to be found in the first part of Thomson and Tait's 'Natural Philosophy' (2nd ed.), pp. 314-415. It is mainly through the influence of this work, and through the inexhaustible wealth of experimental illustrations contained in many of Lord Kelvin's addresses (see 'Popular Lectures and Addresses,' vol. i. pp. 143 *sqq.*, 218 *sqq.*; iii. 165 *sqq.* 245), that gyrostatic and vortex motion has become in this country a favourite study of mathematicians and natural philosophers, and forms an important feature in almost every recent attempt to describe the properties of matter and ether.

42. Influence of Helmholtz's speculations in England.

It does not seem that Helmholtz's speculations were much taken up abroad; in this country, however, they fell on more fruitful soil:[1] they led first of all to

[1] It is a remarkable fact that the country which produced the great theory that finally destroyed the older vortex theory of Descartes, was the one in which, a century after Newton, the modern views on vortex-motion were first and almost exclusively developed. Notably the scientific atmosphere in which Thomson and Tait moved was, *inter alia*, charged with the bold ideas and the suggestive nomenclature of Macquorn Rankine. He owes his permanent place in the history of science to being side by side with Lord Kelvin and Clausius, one of the three founders of theoretical thermodynamics. But he was in addition to this perhaps the earliest and purest representative of the kinetic or mechanical view of natural phenomena, and of the scientific tendency or habit—derived from his profession as an engineer—of constructing for every phenomenon to be explained a mechanical model. In a succession of memoirs beginning in 1850, Rankine put forward his theory of "molecular vortices," "which assumes that each atom of matter consists of a nucleus or central point enveloped by an elastic atmosphere" ('Scientific Papers of Macquorn Rankine,' ed. Miller, London, 1881, p. 17). Clerk Maxwell in 1878 wrote of Rankine's theory: "Whatever he imagined about molecular vortices was so clearly imaged in his mind's eye that he, as a practical engineer, could see how it would work. However intricate, therefore, the machinery might be which he imagined to exist in the minute parts of bodies, there was no danger of his going on to explain natural phenomena by any mode of action of this machinery which was not consistent with the general laws of mechanism. Hence, though the construction and distribution of his vortices may seem to us as complicated and arbitrary as the Cartesian system, his final deductions are simple, necessary, and consistent with facts. Certain phenomena were to be explained. Rankine set himself to imagine the mechanism by which they might be produced. Being an accomplished engineer, he succeeded in specifying a particulur arrangement of mechanism competent to do the work." Maxwell goes on to say: "As long as the training of the naturalist enables him to trace the action only of particular material systems, without giving him the power of dealing with the general properties of all such systems, he must proceed by the method so often described in histories of science — he must imagine model after model of hypothetical apparatus, till he finds one which will do the required work. . . . The theory of molecular vortices was distinguished from other theories which attribute motion to bodies apparently at rest, by the further assumption that this motion is like that of very small vortices, each whirling about its own axis" (Clerk Maxwell in 'Nature,' 1878; 'Scientific Papers,' vol. ii. p. 662, &c.; and Prof. Tait's memoir of Rankine in the 'Collected Papers,' p. xxix). In the most recent attempt to reconcile the two fundamental ideas without which we do not seem to be able to proceed in a description of natural phenomena — viz., that space is a *plenum*, filled by a continuous something, and that matter

many experimental contrivances, by which the remarkable phenomena known as "gyrostatic" — *i.e.*, the stable properties of bodies in rapid rotary motion[1] —could be studied, as also to the development of the theory of knots and linkage.[2] In the resourceful brain

(and electricity) is atomic (discrete, grained), Dr Larmor has traced the modern vortex theory further back beyond Rankine to James MacCullagh, who in his 'Essay towards a Dynamical Theory of Crystalline Reflexion and Refraction' (Trans. Irish Academy, 1839), "arrived at a type of elasticity (of the ether) which was wholly rotational, . . . somewhat after the manner that a spinning flywheel resists any angular deflection of its axis" (p. 26 of his Adams prize essay 'Æther and Matter,' 1900). "Rankine, never timid in his speculations, expounded MacCullagh's analytical scheme soundly and clearly, in full contrast with the elastic properties of matter, as representing a uniform medium or *plenum* endowed with ordinary inertia, but with elasticity of purely rotational type" (ibid., p. 77; cf. p. 73); but he also remarks that "up to the period of Lord Kelvin's vortex atoms . . . the earlier theories . . . could only have been hypothetical speculations" (p. 25 note).

[1] Helmholtz himself did not give many practical illustrations of his remarkable theories. Such were first given by W. B. Rogers ('Amer. Journ. of Science' (2), vol. 26, p. 246) in 1858, without knowledge of Helmholtz's theoretical investigations. In this country such illustrations have become quite favourite popular lecture experiments (see Sir Rob. S. Ball's memoir). Smoke-rings, solid and liquid gyrostats, and a host of similar contrivances, have impressed on us the hidden resources of whirling motion. Prof. Tait, in his 'Recent Advances of Physical Science' (3rd ed., 1885, p. 296), states that experiments on smoke-rings which he performed, suggested to Lord Kelvin the vortex theory of matter. The various papers of the latter have, so far, not been collected in a convenient form. The earliest is contained in the 'Proceedings of the Royal Society of Edinburgh,' February 1867. Then followed a memoir in the 'Transactions' (April 1867) on vortex statics (Proc. R. S. E., December 1875); "Vibrations of a Columnar Vortex" (Proc., March 1880). Prof. Hicks, and especially Prof. J. J. Thomson (Trans. R. Soc., 1884; 1881), have contributed to the theory, and the latter, in his Adams prize essay for 1882, has further tested the conception in its application to chemical statics. See Hicks, 'Recent Progress in Hydrodynamics' (Brit. Assoc. Rep., 1881, p. 63, &c.), and J. J. Thomson 'On the Motion of Vortex Rings' (1883, p. 114, &c.)

[2] The creator of this branch of purely positional geometry is doubtless Johann Benedict Listing, who was led to his researches by some suggestions of Gauss. Gauss refers to the subject in connection with his unpublished researches into electrodynamics (1833, posthumously published in 'Werke,' vol. v. p. 605). Listing called this branch of geometry "Topologie" (cf. Listing, 'Vorstudien zur Topologie,' Göttingen, 1847). In the meantime Riemann had been (1851) led in his mathematical representation of functions on the surface called

of Lord Kelvin this theory led to the conception that in an all-pervading, boundless fluid, such as physicists imagined for the purposes of the theory of light, differentiated portions might exist in the form of whirling rings (vortex rings), which would possess most of the properties of ponderable matter—identity and permanence of quantity of substance, stability, rigidity, elasticity.

43. Difficulties of the vortex ring theory.

It was indeed soon found that although eminently suggestive in this way, and pointing in the direction of a general kinetic theory of natural phenomena, the vortex ring theory presented two fundamental difficulties. How does whirling matter acquire weight, and how does it acquire immensely increased inertia? In the explanation of these two properties the progress has been small, —if indeed any glimpse at all has as yet been got.[1] But by suggesting numberless experiments through which our knowledge of things natural has been enormously increased, by placing before the minds of mathematicians a great number of problems of practical importance and physical interest, and generally by familiarising the minds of philosophers with an ultimate kinetic explanation of nature,[2] the vortex-atom theory has marked an epoch in

after him, to distinguish between singly, doubly, triply, &c., connected surfaces ('Werke,' 1876, pp. 18, 88, 448). These studies, which for a long time were looked upon merely as *curiosa* or of purely abstract interest, were independently taken up in the practical interest of the vortex-atom theory by Prof. Tait in 1876 ("On Knots," Trans. Roy. Soc. Edinb., 1877, vol. 28, p. 145. &c.), and continued in 1884-85. To him we owe a convenient notation and vocabulary. For the history of the subject and further developments, see Dingeldey, 'Topologische Studien.' Leipzig, 1890.

[1] See Clerk Maxwell's article "Atom" in the 9th ed. of the 'Ency. Brit.,' reprinted in 'Scientific Papers,' vol. ii., and the account given there of Le Sage's theory.

[2] See Dr Larmor's Address to Section A of the Brit. Assoc. at Bradford in 1890 (Report, p. 625): "The vortex-atom theory has been a main source of physical suggestion, because it presents, on a simple basis, a dynamical picture of an ideal material system, atomically constituted, which could go on automatically without extraneous support. The value of such a picture may be held to lie, not in any supposition that this is the mechanism of the actual world laid bare, but in the vivid illustration it affords of the fundamental postulate of physical science, that mechanical phenomena are not parts of a scheme too involved for us to explore, but rather present themselves in definite and consistent correlations, which we are able to disentangle and apprehend with continually increasing precision."

the history of thought. As the study of stable motion or dynamical equilibrium, it has joined hands with the kinetic theory of gases—*i.e.*, the study of the motion of a swarm of bodies in rectilinear motion, and with the mechanical theory of heat—*i.e.*, of irregular infinitesimal motion of any kind; and it has certainly, through the remarkable results gained by Professor J. J. Thomson, afforded a clue to the explanation of chemical linkage, showing how it comes about that stability of chemical compounds is dependent on, and limited to, a small number of combinations or linkages.[1] The mathematical difficulties in the way of progress are enormous, sufficient to tax the brains of many generations to come, but as it

[1] See his essay on the "Motion of Vortex Rings": "Let us suppose that the atoms of the different chemical elements are made up of vortex-rings all of the same strength, but that some of these elements consist of only one of these rings, others of two of the rings linked together, or else of a continuous curve with two loops, others of three, and so on. Our investigation shows that no element can consist of more than six of these rings if they are arranged in the symmetrical way there described" (p. 119). "Each vortex ring in the atom would correspond to a unit of affinity in the chemical theory of quantivalence. If we regard the vortex rings in those atoms consisting of more vortex rings than one as linked together in the most symmetrical way, then no element could have an atom consisting of more than six vortex rings at the most, so that no single atom would be capable of uniting with more than six atoms of another element so as to form a stable compound. This agrees with chemical facts, as Lothar Meyer in his 'Moderne Theorien der Chemie,' 4th ed., p. 196, states that no compound consisting of more than six atoms of one element combined with only one of another is known to exist in the gaseous state, and that a gaseous compound of tungsten, consisting of six atoms of chlorine united to one of tungsten, does exist" (p. 120).

has been remarked, "the glory of surmounting them would be unique." [1]

The vortex-atom theory is the most advanced chapter in the kinetic theory of matter, the most exalted glimpse into the mechanical view of nature. Though suggested by Helmholtz, it has, as already stated, been limited almost exclusively to this country. If science still shows international differences and patriotic predilections, this affords one of the few remaining examples. Another step first taken in this country, the last and most important contribution to the science of physical motion, the greatest support of the kinetic or mechanical view of nature, has, in union with the undulatory theory of light, been now all but universally accepted in the scientific world: I refer to the modern view of electric phenomena, which for a long time was supported by the solitary labours and genius of Faraday.

44. Modern view of electrical phenomena: Faraday.

His great discoveries of magneto-electricity, of induction, of the electrification of light, to which I have had repeated occasion to refer, made his name familiar to the whole scientific world; but the processes of reasoning by which he arrived at them, or to which in his mind they gave rise, were ignored or not understood.[2] Whilst

[1] Tait, in 'Recent Advances of Physical Science,' p. 302, and Clerk Maxwell, in article "Atom" ('Ency. Brit.,' 9th ed., or 'Collected Scientific Papers,' vol. ii. p. 472).

[2] See Helmholtz's 'Faraday Lecture,' delivered before the Chemical Society on April 5, 1881, reprinted in his 'Vorträge und Reden,' vol. ii. p. 275, &c. "Since the mathematical interpretation of Faraday's theorems by Clerk Maxwell has been given, we see indeed how sharply defined the conceptions are and how consistent the reasoning which lay concealed in Faraday's words, which to his contemporaries appeared so indefinite and obscure; and it is in the highest degree remarkable to see how a large number of comprehensive theorems, the proof of which taxes the highest powers of mathematical analysis, were found by him without the use of a single mathematical formula, by a kind of intuition with instinctive certainty. I would not depreciate Faraday's contemporaries because they did not see this. I know myself too well how often I sat hopeless, gazing at one of his descriptions of lines of force with their numbers and tension, or looking for the meaning of statements where the galvanic current is regarded as an axis of force and much the like" (p. 277). Rosenberger tells us that it may be in part attributed to the displeasure and annoyance with which foreign philosophers received Faraday's theoretical views, that Poggendorff, who printed Faraday's earlier memoirs *in extenso* in his 'Annalen,' only give a short abstract of the later series. See Rosenberger, 'Die moderne Entwickelung der elektrischen Principien,' Leipzig, 1898, p. 105.

Continental philosophers, following Coulomb, tried to put into mathematical language the action at measurable distances of magnetic masses and elements of electrical circuits,[1] Faraday fastened upon the peculiar lines in which iron filings arranged themselves in the neighbour-

[1] These researches, of which the fourth chapter of this work gave some account, and which culminated in Weber's well-known law of electrodynamic action of electrical particles at a distance, absorbed almost exclusively the attention of natural philosophers abroad. Mathematicians of the highest rank, such as Laplace, Gauss, and Riemann, worked at the subject. It is, however, interesting to note that Gauss, with that remarkable instinct for physical adaptation of mathematical ideas which characterised also the magnetic researches which he carried on between 1830 and 1840, refrained from the development of a mathematical theory of electrodynamic action for reasons which he later explained to Weber. When the latter prepared for publication that elaborate series of exact measurements which, irrespective of the theory attached to them, formed the foundation of modern electrical science and of the correlation of the phenomena of magnetism, of electricity at rest and in motion, of induction and of diamagnetism, Gauss wrote as follows under date 19th March 1845: "The subject belongs to those investigations which occupied me very extensively about ten years ago (especially 1834-36). . . . Perhaps I may be able to think myself again into these matters, which have now become so foreign to me. . . . I should no doubt have long ago published my researches; but at the time when I broke them off, that was wanting which I then considered to be the very keystone—nil actum reputans si quid superesset agendum—namely, the deduction of the additional forces (which have to be added on to the mutual action of particles of electricity at rest, if they are in relative motion) from action, not instantaneous, but (like that of light) propagated in time. With this I could not succeed at the moment, but so far as I can remember I left the subject not entirely without hope that this might later be possible; yet, if I remember aright, with the subjective conviction that it would previously be necessary to form for oneself a workable representation (*eine construirbare Vorstellung*) of the manner in which the propagation takes place" (Gauss, 'Werke,' vol. v. p. 627, &c.)

hood of the poles of magnets;[1] inquired into the nature and condition of the region—afterwards termed the "field"—which surrounded magnetised and electrified bodies; invented the term "electrotonic state" and "dielectric"[2] to describe the part which the surrounding medium played in the so-called actions at a distance; and conceived it to

45. "Lines of force."

be in a state of tension, which he further described by filling it with so-called "lines of force." The region or "field"[3] of magnetic and electric action, filled with these curved lines of force, possessing definite direction and frequency, gave him a clear mental representation of the direction and intensity of magnetic and electric forces at any point in space in the neighbourhood of magnets or of electric currents. For Faraday, the lines of force in the magnetic field, from being originally merely a convenient geometrical device,[4] ac-

[1] "By magnetic curves I mean the lines of magnetic forces, however modified by the juxtaposition of poles, which would be depicted by iron filings, or those to which a very small magnetic needle would form a tangent" (Faraday, 'Experimental Researches on Electricity,' 1st series, November 1831, No. 114 note). "When an electrical current is passed through a wire, that wire is surrounded at every part by magnetic curves, diminishing in intensity according to their distance from the wire. . . . These curves, although different in form, are perfectly analogous to those existing between two contrary magnetic poles opposed to each other" (ibid., 2nd series, January 1832, No. 232).

[2] The term "electrotonic state" was introduced in 1831 to describe the conditions of matter in the neighbourhood of electric bodies. "It is probable that what will affect a conductor will affect an insulator also, producing, perhaps, what may deserve the term of the electrotonic state" (ibid., No. 1661, 1838), "the intervening particles assuming for the time more or less of a peculiar condition, which (though with a very imperfect idea) I have several times expressed by the term electrotonic state" (ibid., No. 1729). "I use the word 'dielectric' to express that substance through or across which the electric forces are acting" (December 1838, ibid., No. 1168, note).

[3] The term "magnetic field" seems to have been used for the first time in the year 1845 (see 'Exp. Res.,' No. 2252, vol. iii. p. 30).

[4] November 1837: "I use the term *line of inductive force* merely as a temporary conventional mode of expressing the direction of the power in cases of induction. . . . The power, instead of being like

quired gradually a physical[1] significance, for he had very early convinced himself of the fact, known already

that of gravity, which causes particles to act on each other through straight lines, . . . is more analogous to that of a series of magnetic needles. . . . So that in whatever way I view it, and with great suspicion of the influence of favourite notions over myself, I cannot perceive how the ordinary theory . . . can be a correct representation of that great natural principle of electrical action" ('Exp. Res.,' No. 1231). "I have used the phrases *lines of inductive force* and *curved lines* of force in a general sense only. . . . All I am anxious about at present is, that a more particular meaning should not be attached to the expressions used than I contemplate" (ibid., No. 1304). And after having referred to the agreement of his results with those of Poisson, arrived at by starting from "a very different mode of action," and with the experimental results of Snow Harris, he concludes by saying, "I put forth my particular view with doubt and fear, lest it should not bear the test of general examination," &c. (No. 1306).

[1] It took more than ten years before the purely geometrical or conventional use of the term "lines of force" ripened into a physical conception. The latter is definitely expounded in a paper in the 'Philos. Magazine' for June 1852. We can compare this gradual development of a symbolical into a physical theory with the gradual development of the atomic theory; atoms and molecules becoming a physical necessity to chemists long after they had been used simply as a convenient representation of the laws of equivalence and of the fixed proportions of combination (see vol. i. of this work, chap. v., p. 432, &c.) Faraday, during the years 1840 to 1850, laboured at two great problems: the one he solved brilliantly and in the direction he anticipated; the other remains a problem to this day. The first refers to the action of magnets on the dielectric. The dielectric, the space which Continental philosophers considered as a vacuum so far as magnetic and electrical phenomena are concerned, had been filled by Young and Fresnel with the luminiferous ether. Faraday suspected that this luminiferous ether cannot be insensible to magnetic action, and he sought in the experimental proof of the action of magnets on rays of light in the surrounding space a support for his view of the part which the dielectric plays in the transmission of electric and magnetic action. After many ineffectual attempts to prove this, he could at last (November 1845) announce his results to the Royal Society as follows: "These ineffectual exertions . . . could not remove my strong persuasion derived from philosophical considerations; and therefore I recently resumed the inquiry by experiment in a most strict and searching manner, and have at last succeeded in *magnetising and electrifying a ray of light, and in illuminating a magnetic line of force*. . . . Employing a ray of light, we can tell, *by the eye*, the direction of the magnetic lines through a body; and by the alteration of the ray and its optical effect on the eye, can see the course of the lines just as we can see the course of a thread of glass or any other transparent substance, rendered visible by the light" ('Exp. Res.,' vol. iii., No. 2148 and note). The second problem which Faraday attacked was to prove a similar "connection be-

to Cavendish, that in the case of electric attraction and repulsion, the nature of the intervening medium was of importance: it played a part in the electric phenomena in the same way as in the propagation of light and heat the intervening medium played a definite part. This part had been entirely overlooked by Continental philosophers, who worked on the hypothesis of an immediate action at a distance, based upon the analogy of gravitation. Their researches, carried on by methods similar to those invented by Laplace and his school for the calculation of the combined effect of gravitational forces at various points in space, entirely ignored the question how such effects were brought about. As time did not seem to enter as an appreciable factor, the investigation of the mechanism by which action at a distance was communicated was set aside as unnecessary or impossible: the astronomical view of the phenomena sufficed. For Faraday, the intervening medium, which—as in the communication of light and heat—took an active part, the question of its nature and mode of action was very important; he accordingly first of all gave it a name. As in optics the term luminiferous ether had been recently revived, and had become familiar through Young and Fresnel, so through Faraday were introduced the terms "dielectric" and "magnetic field," as the carriers of electric and magnetic action; and though for a long time used only by himself, they

tween gravity and electricity." On the failure of this attempt he fully reported in his Bakerian Lecture, November 1850 ('Exp. Res.,' vol. iii. p. 161). But the former results were sufficient to ripen gradually in his mind the idea of the physical nature of the lines of force, which he expounded with increasing precision from 1851 onward. (See 'Exp. Res.,' 28th series, vol. iii. p. 328; also pp. 402, 438).

have been the means of keeping before the minds of natural philosophers the question how these actions are mechanically communicated, a problem which lay outside of the astronomical view of the phenomena. To Faraday himself the analogy between the phenomena of these actions meant also a real physical relation or even identity, a supposition which he followed up with unwearying patience and all the experimental resources of his inventive mind, till he succeeded in showing by experiment that magnets in the neighbourhood of transparent substances which have a polarising effect on rays of light possessed the property of altering the direction in which the polarised rays show their laterality. Faraday's conception of "lines of force" filling all space and explaining electric and magnetic action, radiation, and possibly also gravitation, was elaborated during the years 1830 to 1850. An opinion then prevailed that his discoveries stood in opposition to the views elaborated and experimentally verified by Continental philosophers. The first who showed the analogy and threw out a hint how the two views could be brought into harmony was William Thomson (Lord Kelvin). As early as 1842,[1] when scarcely eighteen

46. Development of the conception by Lord Kelvin.

[1] "On the uniform motion of Heat in homogeneous solid bodies, and its connexion with the mathematical theory of Electricity," 'Cambridge Mathematical Journal,' February 1842. The following note is attached to the reprint in the 'Philosophical Magazine' of 1854: "The general conclusions established show that the laws of distribution of electric or magnetic force in any case whatever must be identical with the laws of distribution of the lines of motion of heat in certain perfectly defined circumstances. With developments and applications contained in a subsequent paper (1845), they constitute a full theory of the characteristics of lines of force, which have been so admirably investigated experimentally by Faraday, and complete the analogy with the theory of the conduction of heat, of which such terms as 'conducting power of lines of force' ('Exp. Res.,' Nos. 2797-2802) involve the idea."

years old, but already acquainted with English experimental and French mathematical researches, he pointed out how phenomena of flow—*i.e.*, of motion—could be mathematically grasped by a formula quite similar to that of the distribution of masses at rest and apparently governed by attractive forces at a distance. For instance, the distribution of temperature at various distinct points in a space in which a flow of heat from an origin had brought about a stationary condition (the equilibrium being dynamical, not statical), was mathematically expressed by a formula identical with that which, according to Poisson and others, gave the distribution of electrical or attracting masses. Now we know that in the former case the equilibrium is maintained by a flow across the intervening space, which takes time. This suggests, therefore, the possibility of explaining the so-called statical effects of attracting or repelling masses kinetically by a process of flow or motion going on in the intervening medium, a notion to which Faraday clung tenaciously. In 1845 Thomson reverted to this subject, and after harmonising the two views, concluded by stating that the latter "method of establishing the mathematical theory would be even more simple if possible than that of Coulomb."[1]

[1] "On the Mathematical Theory of Electricity in Equilibrium," 1845. See 'Reprint of Papers on Electrostatics and Magnetism,' 2nd ed., p. 29. A study of these mathematical researches of Lord Kelvin, beginning early in the 'forties and extending over more than twenty years, is of special historical interest, as showing the gradual growth of a physical out of a purely mathematical theory: most of the conceptions which have since become general through Maxwell's electro-magnetic theory, as it has been developed and popularised by subsequent writers (notably Prof. Poynting, Prof. Oliver Lodge, and Mr Oliver Heaviside), being already contained in Thomson's papers as mathematical notions. Thomson is throughout careful to

This suggestion was not carried out for some time, and then not by Thomson himself, but, at his instigation, by Clerk Maxwell. In the meantime, however, Thomson added another step to the one already taken, by bringing recent discoveries of Faraday, as well as his

point out how the elementary experimental data referring to electrical charges, as well as to magnetic bodies, can be mathematically expressed equally well by the conceptions of Coulomb and Poisson and by those of conduction and flow, which are more in conformity with Faraday's physical ideas: neither of the mathematical analogies, of attraction at a distance or of conduction through an intervening medium, being sufficient for a physical theory. These papers contain further the record of the gradual growth in the author's mind of the kinetic out of the statical view of natural phenomena. Thomson was the first (1851) to introduce the terms "field" and "lines of force" into mathematical literature, adopting them from Faraday. They have since become indispensable not only to the electrician but likewise to the mathematician; forming, as it were, a unifying term for apparently distant regions of physical phenomena, and being introduced as fundamental notions at the beginning of dynamical treatises. See, for instance, the article by M. Abraham entitled "Geometrische Grundbegriffe," in the second part of the fourth volume of the 'Encyclopädie der mathematischen Wissenschaften,' Leipzig, Teubner, 1901. Independently and quite unknown to Faraday, or to each other, two eminent mathematicians, Sir W. R. Hamilton at Dublin and Herrmann Grassmann at Stettin, were elaborating, between 1835 and 1845, the geometrical conceptions and vocabulary which are required in the representation of "directed" quantities. Their expositions have since become much simplified, and now form, under the title of "vector analysis," an indispensable geometrical instrument. The gradual evolution of the kinetic view of physical phenomena (which here concerns us most) in the memoirs of Thomson is most remarkable. *Inter alia*, he made a communication in 1847 to the British Association at Oxford, in which he dealt with the phenomena of terrestrial magnetism, stating that "it becomes an interesting question whether mere electric currents could produce the actual phenomena observed. Ampère's electro-magnetic theory leads us to an affirmative answer which must be regarded as merely theoretical; for it is absolutely impossible to conceive of the currents which he describes round the molecules of matter as having a physical existence" (Reprint, 2nd ed., p. 469). On this passage he himself remarks in 1872: "From twenty to twenty-five years ago, I had no belief in the reality of this [Ampère's] theory; but I did not then know that motion is the very essence of what has hitherto been called matter. At the 1847 meeting of the British Association in Oxford I learned from Joule the dynamical theory of heat, and was forced to abandon at once many, and gradually from year to year all other, statical preconceptions regarding the ultimate causes of apparently statical phenomena" (ibid., p. 423 note).

unique conception of the communication of electric and magnetic phenomena, into connection with the mathematical theory which had been founded and worked out by Poisson and Green. Without attempting to give a physical explanation of Faraday's lines of force, he showed how they could be utilised in calculating the complicated action of magnetic push-and-pull forces; suggested that the newly discovered property called diamagnetism, in virtue of which bodies in the neighbourhood of powerful magnets appeared to be repelled, not attracted, could be explained as a differential [1] effect of

[1] It was in the year 1845 that Faraday, after having discovered the "magnetisation of light," and made visible the "magnetic lines of force" ('Exp. Res.,' Nos. 2146-2242), entered upon that remarkable series of experiments and speculations which led him to the discovery of diamagnetism and to the assertion of the "magnetic condition of all matter" (ibid., Nos. 2243, &c.) In 1847 Thomson wrote: "According to Mr Faraday's recent researches it appears that there are a great many substances susceptible of magnetic induction, of such a kind that for them the value of the coefficient i is negative. These he calls diamagnetic substances, and in describing the remarkable results to which his experiments conducted him with reference to induction in diamagnetic matter, he says, 'All the phenomena resolve themselves into this, that a portion of such matter, when under magnetic action, tends to move from stronger to weaker places or points of force.' This is entirely in accordance with the result obtained above; and it appears that the law of all the phenomena of induction discovered by Faraday with reference to diamagnetics may be expressed in the same terms as in the case of ordinary magnetic induction, by merely supposing the coefficient i to have a negative value" (Reprint, p. 502). In the Reprint (1854) of his early papers (1842) on the corresponding problems of magnetism and heat (Reprint, p. 18) he added a note to the effect that the "same demonstration is applicable to the influence of a piece of soft iron, or other paramagnetic, or to the reverse influence of a diamagnetic on the magnetic force in any locality near a magnet in which it can be placed, and shows that the lines of magnetic force will be altered by it precisely as the lines of motion of heat in corresponding thermal circumstances would be altered by introducing a body of greater or less conducting power of heat. Hence we see how strict is the foundation for an analogy on which the conducting power of a magnetic medium for lines of force may be spoken of, and we have a perfect explanation of the condensing action of a paramagnetic, and the repulsive effect of a diamagnetic upon the lines of force of a magnetic field, which have been described by Faraday" (Reprint, p. 33 note; cf. Faraday, 'Exp. Res.,' Nos. 2807, 2808).

the magnetic actions which belong to all substances; introduced the term magnetic "permeability" [1] as descriptive of the degree in which various substances acquire magnetic properties and conduct the lines of magnetic force in the neighbourhood of powerful magnets; and finally demonstrated how, if these properties were considered as having different degrees in the different axes of crystals, in analogy with the different elasticities which they exhibited, the consequence would be a turning effect which would explain the changed optical properties of crystals under the influence of magnetic action.[2] In these investigations the ideas of

[1] This property was afterwards termed "permeability" by Thomson (Reprint, p. 489, 1872). The general rule of magnetic action can then be expressed by saying that "by virtue of differential action a body may behave paramagnetically or diamagnetically according as it is placed in a less or a more permeable medium than itself" (Chrystal in article "Magnetism," 'Ency. Brit.,' 9th ed., vol. xv. p. 248).

[2] On the Theory of Magnetic Induction in Crystalline and Non-crystalline Substances" ('Philos. Mag.,' March 1857; also Reprint, 2nd ed., p. 471, &c.) Poisson had already foreseen the mathematical possibility of what Faraday termed magne- (correctly magneto-) crystallic action, but "ce cas singulier ne s'étant pas encore présenté à l'observation, nous l'exclurons de nos recherches" ("Mémoire sur la Théorie du Magnétisme," 'Mém. de l'Institut, Paris, 1826,' quoted by Thomson, Reprint, p. 484). Stimulated by the discoveries of Faraday, Plücker at Bonn, during the extraordinary interval which separated the second from the first period of his original geometrical speculations (see vol. i. p. 242 of this work), devoted himself to the study of the electric and magnetic properties of gases and crystals, and in 1847 commenced that remarkable series of physical memoirs through which he became the fellow-worker, if not the rival, of Faraday. One of his first discoveries was the action of magnets on crystals, published in 1847 (Pogg. Ann., or Plücker's 'Physicalische Abhandlungen,' ed. Pockels, Leipzig, 1896, p. 6, &c.), which supplied to Thomson "the very circumstance the observation of which was wanting to induce Poisson to enter upon a full treatment of the subject, and made the working out of a mathematical theory of magnetic induction . . . independently of any hypothesis . . . upon a purely experimental foundation . . . important" (Thomson, *loc. cit.*, p. 471). Plücker was an original thinker, and mainly a self-taught genius, imperfectly acquainted with the labours of his contemporaries or predecessors. This has been noted by his biographers as much in his geometrical as in his physical researches (see the memoirs of Clebsch and of Prof. Riecke, prefixed to the two volumes of the 'Gesammelte Abhandlungen').

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Faraday are used merely for the sake of describing and calculating in the simplest manner phenomena which had been experimentally discovered: no attempt was made to explain physically how these actions come about. In fact, under the hands of Thomson the conceptions of Faraday were formulated as Dalton's atomic theory had been elaborated by chemists in the first half of the century, for the purpose of symbolically representing and calculating observed phenomena.

But the "lines of force" of Faraday were not to remain a mere symbolical representation, any more than Dalton's atoms were to remain merely counters of a chemical arithmetic. Both theories were to be raised to the rank of physical theories. What the kinetic theory of gases did for the atomic theory was done for Faraday's symbolism

47. Clerk Maxwell. by the researches of Clerk Maxwell. And as the fact that the molecules of matter could be really counted, and their distances and velocities measured, gave life and actual meaning to the atomic view of natural phenomena,

In his early geometrical researches he worked in ignorance of the remarkable 'Traité' of Poncelet, which had been published in 1822 (*loc. cit.*, vol. i. p. 594, &c.): even the writings of his countryman Möbius were unknown to him. Still more extraordinary was his comparative unacquaintance with the electrical measurements and theories which dominated German research when he commenced his physical labours, and which emanated from the school of Gauss and Weber. But he was equally ignorant of the purely mathematical theories of Poisson and Thomson, which, as he himself candidly confessed, might have saved him from important errors (*loc. cit.*, vol. ii. p. 460), and which were later made more widely known in Germany by the excellent treatise of his pupil Beer ('Einleitung in die Elektrostatik,' &c., Braunschweig, 1869), posthumously edited by Plücker himself. The fact that Plücker was not influenced by the spirit of Weber's researches probably made him more appreciative of Faraday's purely physical methods. In such names as Beer, Clebsch, Klein, Fessel, Geissler, and Hittorf, Plücker counts an illustrious array of pupils and fellow-workers. See Clebsch's characteristic of Plücker, *loc. cit.*, vol. i. p. xii, &c.

so the rays of electric and magnetic force seen by Faraday in the abstraction of his intuitive mind became a reality for every experimentalist when Hertz in 1888 actually showed the wonderful action of electric waves at a distance. Atoms and lines of force have become a practical —shall I say a popular?—reality, whereas they were once only the convenient method of a single original mind for gathering together and unifying in thought a bewildering mass of observed phenomena, or at most capable of being utilised for a mathematical description and calculation of actual effects.

For a quarter of a century after Faraday had conceived the notion of looking upon electric and magnetic phenomena as depending on a property belonging to all matter, and pervading all space, like radiation and gravity, the only natural philosopher who to any extent entered into his ideas was Thomson. Even Tyndall, who came more than any other prominent physicist under Faraday's immediate and personal influence, and contributed largely to our knowledge of the new phenomena discovered by his great master, does not seem to have assimilated his scientific language and reasoning. It required a mathematical mind really to grasp and put into form Faraday's notions. Encouraged by Thomson, and soon after the publication of Thomson's mathematical theory of magnetism, Clerk Maxwell devoted himself to a theoretical study of electricity and allied subjects, a field which Thomson had then almost monopolised in this country.[1] The first of Maxwell's revolu-

[1] See Professor Glazebrook's little book on 'James Clerk Maxwell and Modern Physics,' published in the "Century Science Series," 1901. On page 42 a letter of Maxwell is quoted, in which he speaks of

48. His series of works on the theory of electricity.

tionary series of works, 'On Faraday's Lines of Force,' was published in December 1855. The series was completed by the appearance in 1873 of his great work on 'Electricity and Magnetism,' which has formed the centre of a large literature to which all the scientific schools of Europe and America have contributed. Historically, Maxwell brought together two distinct and very fruitful lines of reasoning, due to Faraday and Thomson.[1] He was impressed with the desideratum of every physical theory bearing on any large class of phenomena—viz., that it must be mathematical and physical at the same time. His own theory had to embrace and unite all the purely arithmetical and geometrical regularities which had been discovered, and which at that time were known to describe correctly the facts of electric, mag-

"poaching upon Thomson's electrical preserves." In the preface to the treatise on electricity and magnetism, he refers to the apparent discrepancy between the views of Faraday and the mathematicians, and he states that he had arrived at "the conviction that this discrepancy did not arise from either party being wrong. I was first convinced of this," he proceeds, "by Sir William Thomson, to whose advice and assistance, as well as to his published papers, I owe most of what I have learned on the subject.

[1] In a different reference we may say that Maxwell's theory was prepared by three independent lines of research, starting respectively in France, Germany, and England: (1) The investigation of the actions at a distance of electrified and magnetised bodies, and of electric currents, which found mathematical expression in the formulæ of Coulomb and Ampère. The full significance and capabilities of the formulæ of electrostatic and magnetic action had been demonstrated by Thomson, who especially showed that these relations were not necessarily confined to the physical theory which had been elaborated on the Continent, but that, *mutatis mutandis*, they lent themselves equally well to the physical ideas of Faraday. (2) The exact measurements of magnetic, electro-dynamic, and galvanic action started by Ohm and Gauss in Germany, and much extended by Weber. (3) The idea of physical lines of force, filling space and representing action through contiguous particles, not at a distance, elaborated by Faraday. These three lines of research were brought together in the theory of Maxwell, which in the beginning professed to be only a mathematical but ended by being a physical theory.

netic, and galvanic phenomena, such as Coulomb's electrostatic and magnetic laws, Ampère's electro-dynamic and electro-magnetic formulæ, and Ohm's and Faraday's laws referring to galvanic currents, and many others. It had also to give an intelligible representation of the elementary actions of which these complicated phenomena are made up. In order to arrive at the latter, the method usually employed is to look for analogies in other provinces of science where the desired unification has already been brought about. The great natural philosophers of the French school who had so successfully accomplished the most extensive unification yet attempted in any large branch of knowledge—the unification of physical astronomy under Newton's gravitation formula—had tried to follow up this analogy in other realms of research, and had developed what I called in a former chapter the astronomical view of natural phenomena. Ampère, and notably Weber, had extended this analogy so as to embrace electric and magnetic phenomena. There was, however, another analogy which was more familiar to the great experimentalists in this country, notably to Faraday—namely, the analogy of those various phenomena which depend on processes of emanation, of a gradual spreading out, of a flow or conduction: those phenomena where the factor of time comes in, and where an apparently stationary condition is brought about by a mode of motion, or what has been termed a "dynamic equilibrium." Thomson, starting from Fourier's mathematical analysis of such processes, had been led to see how far-reaching this analogy is, and had latterly (1852) extended it to

embrace the processes of the flow of heat, of electricity, magnetic and diamagnetic, and of fluid motion. "He called attention to the remarkable resemblance which the diagrams of flow bore to those which Mr Faraday had recently shown at the Royal Institution to illustrate his views regarding the action of ferro-magnetics and diamagnetics in influencing the field of force in which they are placed, and justified and illustrated the expression 'conducting power for the lines of force' by referring to rigorous mathematical analogies presented by the theory of heat."[1]

This view, which Thomson had merely shadowed forth, was more fully worked out by Maxwell in 1855 and 1861. His methods[2] were "generally those suggested by the processes of reasoning which are found in the researches of Faraday, and which, though they had been interpreted mathematically by Prof. Thomson and others, are very generally supposed to be of an indefinite and unmathematical character when compared with those employed by the professed mathematicians." The first addition which he introduced, by which he made Faraday's "lines of force" mathematically more definite, was
49. His conception of "tubes of force." to change them into "tubes of force," which represented not only the direction of force at every point of space, but also—according to their sectional dimensions—the intensity of the force. These tubes were supposed to be

[1] Abstracts of two communications to the British Association at Belfast in 1852, "On certain Magnetic Curves: with Applications to Problems in the Theories of Heat, Electricity, and Fluid Motion" (Reprint of Papers, &c., p. 519, &c.)

[2] James Clerk Maxwell "On Faraday's Lines of Force," 'Transactions of the Cambridge Philosophical Society,' 1855. See 'Collected Scientific Papers,' vol. i. p. 157.

filled with a moving fluid, and the velocity of the flow—inversely proportional to the sectional area of the tubes—represented the intensity of the force at any point in space. He also showed how very much simpler the conception becomes, if the law of the acting forces is the experimentally established law of the inverse square of the distance.

This thought of "referring to the purely geometrical idea of the motion of an imaginary fluid"[1] was the beginning of the now universally adopted view of a very large class of phenomena, and it was at the same time a great step in the development of the kinetic or mechanical view of natural processes. These lines or tubes of force,[2] with which all space surrounding magnets or electrified bodies was supposed to be filled, enabled Maxwell further to give a definite representation of that peculiar state of matter of which Faraday had very 50.
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called the "electrotonic state." Thomson had already in 1847[3] shown how the ideas of Faraday, who as early

[1] How little Maxwell originally intended to give a physical theory is seen from the concluding sentences of the introduction to his first paper (*loc. cit.*, vol. i. p. 159): "By referring everything to the purely geometrical idea of the motion of an imaginary fluid, I hope to attain generality and precision, and to avoid the dangers arising from a premature theory professing to explain the cause of the phenomena. If the results of mere speculation which I have collected are found to be of any use to experimental philosophers, in arranging and interpreting their results, they will have served their purpose, and a mature theory, in which physical facts will be physically explained, will be formed by those who by interrogating Nature herself can obtain the only true solution of the questions which the mathematical theory suggests."

[2] Faraday had already in 1852 spoken of shells and tubes of force, and invented the term sphondyloid to denote the portion of space enclosed between such shells of force ('Exp. Res.,' vol. iii., No. 3271).

[3] In 1847 ('Cambr. and Dubl. Math. Journal,' reprinted in 'Math. and Phys. Papers,' vol. i. p. 76) Thomson wrote that Faraday's theory of electrostatic induction

as 1831 conceived this peculiar condition of matter to be equivalent to a state of strain, could be represented by the mechanical analogy of the strains existing in an elastic solid. He had distinguished three distinct forms of this elastic strain, and had identified these three forms severally with electrostatic, magnetic, and galvanic forces. He had not given a physical explanation of the origin of these forces, but had merely used the "mathematical analogies of the two problems (the electrical and the elastic) to assist the imagination in the study of both."[1] Maxwell now took a further step and proceeded to give a physical or mechanical description of the nature of this state of stress, of the electrotonic state of matter. With this object in view he conceives of a medium which is capable of exerting force on material bodies by being itself strained, and exhibiting the

"suggests the idea that there may be a problem in the theory of elastic solids corresponding to every problem connected with the distribution of electricity on conductors, or with the forces of attraction and repulsion exercised by electrified bodies. The clue to a similar representation of magnetic and galvanic forces is afforded by Mr Faraday's recent discovery of the affection, with reference to polarised light, of transparent solids subjected to magnetic or electro-magnetic forces."

[1] Quoted from Maxwell's paper "On Physical Lines of Force," in the 'Philos. Mag.' 1861 (see 'Coll. Papers,' vol. i. p. 453), in which Maxwell applies Rankine's conception of molecular vortices to the representation of magnetic phenomena. He refers to his earlier paper (1855) on (geometrical) "lines of force" in which he had "shown the geometrical significance of the electrotonic state," and had used "mechanical illustrations to assist the imagination, but not to account for the phenomena." "I now," he says, "propose to examine magnetic phenomena from a mechanical point of view, and to determine what tensions in, or motions of, a medium are capable of producing the mechanical phenomena observed. If by the same hypothesis we can connect the phenomena of magnetic attraction with electro-magnetic phenomena, and with those of induced currents, we shall have found a theory which, if not true, can only be proved to be erroneous by experiments which will greatly enlarge our knowledge of this part of physics" (ibid., p. 452).

phenomena of tension and pressure (magnetic action) as also of motion of its parts (electro-magnetic action). Now in a medium which is so constituted—*i.e.*, which possesses elastic mobility of its parts—we know that by a whirling or vortex motion phenomena of pressure and tension can be produced in certain parts, and the questions accordingly presented themselves to Maxwell, How by such tension and pressure in certain parts of the medium can magnetic phenomena be represented? and How can the vortices communicate motion to, or receive motion from, the interlying movable particles of the medium? He succeeded in working out a very complete model of such a medium, representing by its mechanical motions both magnetic and electro-magnetic phenomena. Especially was he successful in visualising Faraday's lines or tubes of force, and endowing them with mechanically measurable forces. Maxwell admits that "his conception . . . may appear somewhat awkward. I do not," he says, "bring it forward as a mode of connection existing in nature. . . . It is, however, a mode of connection which is mechanically conceivable and easily investigated; . . . so that I venture to say that any one who understands the provisional and temporary character of this hypothesis will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena."[1]

[1] 'Collected Papers,' vol. i. p. 486. At the end of his paper on physical lines of force, Maxwell touches on the philosophical question, "how much evidence the explanation of phenomena lends to the credibility of a theory, or how far we ought to regard a coincidence in the mathematical expression of two sets of phenomena as an indication that these phenomena are of the same kind. We know that partial coincidences of this kind have been discovered; and the fact that they

The idea of a medium of extreme rarity, pervading all space and interpenetrating all matter, capable also of the elastic reactions of a solid body, was not repugnant to physicists at the time when Maxwell wrote. Though violently opposed forty years earlier when proposed by Fresnel and Young, it had gradually, through the development of optical theories, become a well-recognised instrument of scientific thought. In such a medium a disturbance or displacement is propagated with a certain velocity dependent on its elastic nature—the so-called constants of density and rigidity. Now, looking upon a charge of electricity not as a material something—an imponderable—but as a displacement of the medium, the question arose, Does the velocity with which such a displacement travels compare at all with the known velocities of other elastic disturbances, such as light is conceived to be? It was known to electricians that an amount or charge of electricity can be either stationary (called statical electricity) or in motion (called an electric current); and Weber and Kohlrausch had in 1856 actually measured the number of units of statical electricity which must flow through an electric circuit in order to produce the known mechanical effect of a unit of electric current. The quantity which they found, and

51. Correspondence between velocities of light and of electricity.

which corresponded to a velocity, was of the same order as the velocity with which the elastic disturbance which we call light is known to travel. Maxwell was the first

are only partial is proved by the divergence of the laws of the two sets of phenomena in other respects. We may chance to find, in the higher parts of physics, instances of more complete coincidence which may require much investigation to detect their ultimate divergence" (p. 188).

to see the physical significance of this correspondence.[1] "I have deduced the relation between the statical and dynamical measures of electricity, and have shown by a comparison of the electro-magnetic experiments of MM. Kohlrausch and Weber with the velocity of light as found by M. Fizeau, that the elasticity of the magnetic medium in air is the same as that of the luminiferous medium, if these two coexistent, coextensive, and equally elastic media are not rather one medium."[2]

After having pointed out this remarkable correspondence and other analogies between electrical and optical properties which could be verified by experiment, Maxwell seems to have felt satisfied that a dynamical or kinetic explanation of electric and magnetic phenomena based upon rotary and translational motions and elastic strains in the magnetic field was quite possible. The detailed descriptions given in his earlier papers he looked upon merely as crude mechanical devices by which some of the known effects of magnets and currents could be described. The valuable result was, that the electro-magnetic field could be looked upon as a mechanical system; that the observed actions at a distance could be conceived as communicated through this mechanical system in definite measurable time; and that certain analogies had been pointed out as existing between

52. "Elastic disturbances" of the same medium.

[1] 'Philos. Mag.,' January and February, 1862; 'Coll. Papers,' vol. i. p. 492.

[2] Cf. 'Coll. Papers,' vol. i. p. 500: "The velocity of transverse undulations in our hypothetical medium, calculated from the electro-magnetic experiments of MM. Kohlrausch and Weber, agrees so exactly with the velocity of light calculated from the optical experiments of M. Fizeau, that we can scarcely avoid the inference that light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena."

optical, electrical, and magnetic phenomena, which by carefully devised experiments might be verified and extended.

Through Maxwell, following on Faraday and Thomson, the treatment of electric and magnetic phenomena had thus entered on a similar stage to that which the treatment of optical phenomena had attained half a century earlier through Young and Fresnel. A kinetic or mechanical view, more or less precise and definite, had been propounded; a considerable number of facts had been brought into connection, into line and order; the direction which experimental research must take had been indicated; and finally a correspondence had been established between two great groups of phenomena, those of electricity and magnetism on the one side, those of light on the other. It might have been expected that Maxwell would now take the same course as that taken by Fresnel about the year 1820, and perfect his views by giving his theory of molecular vortices greater precision and definiteness—*i.e.*, by perfecting the electro-magnetic model, as Fresnel and others perfected in their time the system of vibrations by which they visualised the processes of light. This is not the method which Maxwell adopted.[1] In his later and more important

[1] The progress of Maxwell's reasoning is clearly marked in the three memoirs, belonging respectively to the years 1855, 1861, and 1864, of which the last appeared in the 'Transactions' of the Royal Society, and which are reprinted in the first volume of the 'Collected Scientific Papers.' The first memoir on "Faraday's Lines of Force" adheres strictly to the mathematical formulation of Faraday's conception, much in the spirit of Thomson's many expositions. The second, on "Physical Lines of Force," follows Faraday in the attempt to take the original symbol in real earnest as a physical arrangement, and devises, or applies for that purpose, the theory of molecular vortices. The third memoir, which is by far the most important and original, drops this somewhat crude device, as well as the older theory of particles acting at a distance, with forces which, according to Weber, depend on their velocities, and starts from "the conception of a complicated mechanism capable of a vast variety of motion, but at the same time so connected that the motion of one part depends . . . on the motion of other parts, these motions being communicated by forces arising from the relative displacement of the connected parts, in virtue of their elasticity" (Papers, vol. i. p. 533). He further says: "I have on a former occasion attempted to describe a particular kind of motion and a particular kind of strain, so arranged as to account for the phenomena. In the present paper I avoid any hypothesis of this kind; and in using such words as electric momentum and electric elasticity in reference to the known phenomena of the induction of currents and the polarisation of dielectrics, I wish merely to direct the mind of the reader to mechanical phenomena which will assist him in understanding the electrical ones. All such phrases in the present paper are to be considered as illustrative, not as explanatory. In speaking of the energy of the field, however, I wish to be understood literally. All energy is the same as mechanical energy, whether it exists in the form of motion or in that of elasticity, or in any other form. The energy in electro-magnetic phenomena is mechanical energy. The only question is, Where does it reside? On the old theories it resides in the electrified bodies, conducting circuits, and magnets, in the form of an unknown quality called potential energy, or the power of producing certain effects at a distance. On our theory it resides in the electro-magnetic field, in the space surrounding the electrified and magnetic bodies, as well as in those bodies themselves, and is in two different forms, which may be described without hypothesis as magnetic polarisation and electric polarisation, or, according to a very probable hypothesis, as the motion and the strain of one and the same medium" (p. 563).

writings he adopted a different and more general process of reasoning. If electrical and magnetic as well as optical phenomena are produced by the motions of the parts of a medium possessed of certain mechanical properties, this medium represents a mechanical system, and must therefore be subject to the general laws which regulate all mechanical systems. These general laws are laid down in dynamics, where it is shown that a complete knowledge of the behaviour of such a system can be reduced to the knowledge of the distribution in it of a quantity called Energy.

53. Consequences on the lines of a theory of Energy.

I intend in the next chapter to trace historically the

growth of this conception as applied not only to the energy of visible and measurable mechanical motion, but to all other forces of nature which have in the course of the century not only been measured in terms of this one quantity, but also represented with more or less success as dependent on the energy of specific forms of motion, be this rotatory or vibratory or translational motion, regular and periodic or irregular and disorderly motion. It is clear that such a general abstract view as Maxwell (first among natural philosophers) took of a special problem was only possible after it had been shown how all physical and chemical actions and effects can be reduced to a common measure. The influence of the development of these views on the kinetic view of nature has been very great. The first and most natural effect of measuring all forces of nature in terms of the energy of motion is to strengthen the kinetic view of natural phenomena. This, however, is not the only view which is possible, or which has been taken, as I shall endeavour to show more fully hereafter.

The influence of Maxwell's ideas on scientific—nay, even on popular—thought has been very considerable. The main conception around which research, both mathematical and experimental, has moved during the last twenty years is the conception of light as an electro-magnetic phenomenon. This view has been much supported and extended by the experiments of Heinrich Hertz, who by ingenious contrivances succeeded in actually exhibiting electro-magnetic waves, and in showing how they differ from light waves merely in length and period, and agree with them so far as

reflexion and refraction and other properties are concerned. Luminous waves are now considered by many physicists to be merely electro-magnetic waves of short wave length and great frequency, such as the organ of vision is capable of perceiving in the form of light. The electric and magnetic medium is identical with the luminiferous ether, postulated by Young and Fresnel, and rays of light are merely an electric and magnetic disturbance propagated as a periodic or wave motion.

54. Destructive effect of the new theories on the astronomical view.

These discoveries and theories have gone a long way to destroy the older astronomical view of natural phenomena, which explained many effects by the action at a distance of particles of ponderable or imponderable matter. The firm conviction has taken hold of the modern scientific intellect or imagination that space is a *plenum* filled with a continuous medium, and that the undoubted atomic nature of ponderable matter may be owing merely to a specific and unmodifiable form of motion with such properties as Lord Kelvin has shown to belong to vortex filaments. The difficulty still remains how to explain the phenomenon of gravitation as well as the increased amount of inertia or mass which belongs to all ponderable matter as compared with that material substance which we call ether.

The reason why Maxwell abandoned his earlier schemes, in which he tried to construct a mechanical model of the electro-magnetic field, is not quite clear.[1] The idea has, however, been taken up by others, and elaborate descriptions have been attempted, by which the

[1] A suggestion regarding this is given by Dr J. Larmor in 'Æther and Matter,' p. 28.

processes going on in the neighbourhood of electrically charged bodies, of electric currents, of magnets and diamagnets, can be visualised.[1] For didactic purposes such elaborate models may prove to be of great value, though as a true mechanical basis of a physical theory of natural processes they have to be received with caution. None of those physicists who have expended their ingenuity in devising these contrivances seem to attach more than a symbolic or ideal value to them: they have, however, the desired effect of producing on the mind of the learner, of the practical inventor, or of a popular audience a strong conviction that all physical phenomena can be described as processes of motion, and that the ultimate solution of the problem of natural philosophy is to be found in a kinetic or mechanical view of phenomena. Physics and chemistry are, according to this

[1] Such illustrations may be found in Dr Oliver Lodge's 'Modern Views of Electricity,' a book which has had a large circulation and has helped to diffuse correct and practically useful ideas on electric and magnetic problems and phenomena. There is a danger of such mechanical illustrations becoming too rigid and of their being taken too literally; still, for the purposes of practical application and handling it is indispensable to possess some mechanical mode of representation and construction by which actual problems can be readily solved. The success of Dr Lodge's attempt both in this country and on the Continent, especially in Germany, proves sufficiently that it meets a much-felt want. See *inter alia* Prof. Rosenberger's five lectures, 'Die moderne Entwickelung der elektrischen Principien,' Leipzig, 1898, p. 133. A great authority abroad, Prof. Ludwig Boltzmann, has made use of a peculiar kind of mechanical motion, investigated by Helmholtz, to illustrate electrical phenomena. The characteristic of such motion—which is termed cyclic—is this, "that in the place of every particle which changes its position, an equal and equally moving particle enters, so that the condition of the system during the motion is nowise altered" ('Vorlesungen über Maxwell's Theorie,' Leipzig, 1891 and 1893, vol. i. p. 14). Cycles can be "coupled," &c. The general dynamical relations of such cyclic systems are investigated, and by introducing the necessary restrictions, based upon experimental facts, and suitable hypotheses—facts and hypotheses being clearly distinguished—the general equations of Maxwell are arrived at.

view, destined to become ultimately merely chapters in dynamics as the doctrine of mechanical motion.

A similar reluctance to look upon the vibrations of the luminiferous ether merely as a convenient symbolism, as a crude method of visualising molecular processes, which in reality we cannot picture to ourselves, does not seem to have troubled the minds of the great propounders of the undulatory theory of light—*i.e.*, of the elastic solid theory, as it is now termed in contradistinction to the electro-magnetic theory propounded by Maxwell. The greatest living exponent of the former view, Lord Kelvin, who in his Baltimore Lectures grappled with the difficulties which still beset that view—falling back on the principle of optical consonance and resonance, suggested by Professor Stokes to explain some of the interactions of the ether and ponderable matter; upon the theory of free and forced vibrations, suggested by Bessel and Sellmeier; and on his own fruitful suggestion of the vortex atom to explain some of the properties of ponderable atoms moving in the continuum which fills all space—expresses

55. Lord Kelvin on the vibrations of the ether.

himself very definitely on this point. "We must not listen to any suggestion that we may look upon the luminiferous ether as an ideal way of putting the thing. A real matter between us and the remoter stars I believe there is, and that light consists of real motions of that matter, motions just such as are described by Fresnel and Young, motions in the way of transverse vibrations. If I knew what the magnetic theory of light is, I might be able to think of it in relation to the fundamental principles of the wave theory of light. But it seems to me rather a backward step from an absolutely definite

mechanical notion that is put before us by Fresnel and his followers, to take up the so-called electro-magnetic theory of light in the way it has been taken up by several writers of late."

But whilst, no doubt, the train of reasoning started by Maxwell, and developed by his followers, has somewhat destroyed the simplicity and directness which the older vibratory theory of light and the kinetic theory of gases had brought into our mechanical views of natural phenomena, the subsequent experimental proof of the existence of electric waves by Hertz has done much popularly to strengthen that view. The discovery of other kinds of rays, by Lenard, Röntgen, and others, has likewise tended in the same direction, though their exact nature is still a subject of much conjecture.

Nor can it be denied that the practical usefulness also of these lately discovered forms of radiation has tended in the same direction; as has, all through the last thirty years, the enormous development of electrical industry in its many branches. Up to the beginning of the nineteenth century the principal electric and magnetic phenomena known were what we term statical: the study of these centred in the conception of electric and magnetic charges concentrated on or in conductors and acting at a distance. The practical interest was limited to mariners' compasses and lightning-conductors. The discovery of the galvanic current, and still more its applications by Davy to the decomposition of the most refractory chemical compounds, introduced an entirely new class of phenomena. Continental science, in Coulomb, Ampère, and Weber, first

developed the line of reasoning and research suggested by statical phenomena and applied this to dynamical phenomena. Faraday, following Davy, approached the subject from the point of view of the chemist. It was soon suspected, and latterly proved by actual measurements, that the quantities which come into play in statical charges, and even in a violent thunderstorm, are small compared with those of a steady electrical current. The phenomena of electricity in motion became of infinitely more practical importance than those of electrical equilibrium or of static tension. The views of Faraday, Thomson, and Maxwell, which Helmholtz, educated though he was in the Continental methods, adopted and introduced into German scientific literature, lent themselves, as he recognised, more successfully and directly to the solution of the problems which applied science forced upon theorists.

Something, indeed, has been lost by this fundamental change which has come over modern reasoning in electrical matters. This has been most clearly and pointedly expressed by M. Poincaré, the eminent French mathematician, who has done so much to illumine physical and mechanical problems from the side of pure mathematics. "Maxwell," he says, "does not give a mechanical explanation of electricity and magnetism; he confines himself to the proof that such an explanation is possible." Accordingly, those who were brought up in the traditions of the school of Laplace and Cauchy feel dismayed at the indefiniteness which adheres to the expositions of Maxwell's latest and greatest work. "A great French philosopher," M. Poincaré proceeds, "one of those

56. Indefiniteness of the electromagnetic theory.

who have most completely fathomed Maxwell's work, said to me once, 'I understand everything in the book except what is meant by an electrically charged body.'" Professor Glazebrook tells us: "We cannot find in the 'Electricity' an answer to the question, What is an electric charge? Maxwell did not pretend to know, and the attempt to give too great definiteness to his views on this point is apt to lead to a misconception of what those views were. . . . Still, in order to grasp Maxwell's theory, this knowledge is not necessary."

Nevertheless, Maxwell's followers in this country and abroad are not satisfied to leave those points which are obscure or indefinite in his theory unilluminated. I have already referred to the valuable practical illustrations of Lodge. What has been done in a more systematic manner on the Continent and at home I shall briefly refer to at the end of the next chapter. We may call it a revival of the atomic view of electricity.

CHAPTER VII.

ON THE PHYSICAL VIEW OF NATURE.

I HAVE already remarked that none of the three great generalisations which we have so far reviewed have been creations of the philosophers of the nineteenth century. Their first enunciation belongs to antiquity, though they have only within the last three hundred years been expressed in sufficiently precise terms to permit of practical measurements and mathematical deductions. The first step towards a scientifically comprehensive employment of the familiar but vague terms of attraction, of atoms, and of undulations came, as we have seen, in each case from some solitary thinker of this country: from Newton, from Dalton, from Thomas Young. The systematic elaboration belongs to the combined scientific exertions of all the civilised nations of the world. In books on astronomy, physics, and chemistry, up to the middle of the century, we can hardly find any theoretical expositions which are not based upon one or more of these three ideas. Indeed they govern the entire science of inanimate nature during the first half of the century. None of these three principles, however, appeared suf- 1. Recapitulation.

ficient to cover the whole field. The law of gravitation embraced cosmical and some molar phenomena, but led to vagueness when applied to molecular actions. The atomic theory led to a complete systematisation of chemical compounds, but afforded no clue to the mysteries of chemical affinity. And the kinetic or mechanical theories of light, of electricity, and magnetism, led rather to a new dualism, the division of science into sciences of matter and of the ether. The unification of scientific thought which was gained by any of these three views, the astronomical, the atomic, and the mechanical, was thus only partial. A more general term had to be found under which the different terms could be comprised, which would give a still higher generalisation, a more complete unification of knowledge. One of the principal performances of the second half of the nineteenth century has been to find this more general term, and to trace its all-pervading existence on a cosmical, a molar, and a molecular scale. It will be the object of this chapter to complete the survey of those sciences which deal with lifeless nature by tracing the growth and development of this greatest of all exact generalisations—the conception of energy.

2. Insufficiency of the astronomical, atomic, and kinetic views.

3. The conception of energy.

The complex of ideas and the manifold courses of reasoning which are centred in this conception form such an intricate network, the interests involved are so great, the suggestions which led up to it so numerous, the consequences which resulted for science and practice so far-reaching, that the historian has no little difficulty in laying bare the many lines of thought which apparently cross and re-cross each other. Accordingly the

history of this subject has been written from various points of view,[1] and angry controversies[2] as to priority

[1] The histories are mostly in German. I give the titles of the more important. Foremost stand the writings of Prof. Ernst Mach—viz., 'Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit' (Prag, 1872), incorporated in the author's 'Popular Scientific Lectures,' translated by Thomas J. M'Cormack, Chicago, 1894; and the same author's 'Die Mechanik in ihrer Entwickelung, historisch-kritisch dargestellt' (Leipzig, 1883, 2nd ed., 1889, also translated by M'Cormack, London and Chicago, 1893). The philosophical faculty of the University of Göttingen has twice (in 1869 and in 1884) made the principles of dynamics the subject of a prize competition, presumably both times at the instigation of the late celebrated Professor Wilhelm Weber. The first competition led to the publication of E. Dühring's 'Kritische Geschichte der allgemeinen Principien der Mechanik' (Leipzig, 1872; republished, with much controversial matter, in 1876 and 1887); the second to the publication of Prof. Max Planck's 'Das Princip der Erhaltung der Energie' (Leipzig, 1887). In the same year as the last book there appeared 'Die Lehre von der Energie,' by Dr Georg Helm (Leipzig, 1887), and lately his very complete work, 'Die Energetik, nach ihrer geschichtlichen Entwickelung' (Leipzig, 1898).

[2] The controversy turned mainly on the question of the claims of Dr Julius Robert Mayer of Heilbronn. The experimental work of Joule in England and the theoretical work of Helmholtz in Germany were published in ignorance of the writings of Mayer. Even the earlier important papers of William Thomson (Lord Kelvin) and Rudolph Clausius appeared before the name of Mayer was generally known. The question then arose to what extent the publications of Mayer really anticipated the discoveries and theories of Joule, Helmholtz, Thomson, and Clausius. It can hardly be held that they influenced them. The whole of the evidence as to the former point is contained in a very complete publication by Prof. Jacob J. Weyrauch, "Kleinere Schriften und Briefe von Robert Mayer" (Stuttgart, 1892), which forms a supplement to the edition by the same author of Robert Mayer's 'Schriften,' entitled "Die Mechanik der Wärme" (Stuttgart, 3rd ed., 1893). Both books contain very careful and exhaustive notes. Whoever desires to settle the question of Mayer's claims, which, however, will always depend much on individual opinion, will find all the documentary evidence collected in these interesting volumes. A further controversy arose later as to the discovery and enunciation of the second law of thermodynamics, the great doctrine of the "Dissipation of Energy." This controversy arose over the publication of the late Prof. P. G. Tait's 'Sketch of Thermodynamics' in 1868, which is an amplification of two articles by the same author in the 'North British Review' of 1864. The controversy, which referred mainly to R. Clausius's share in the enunciation of the second law, can be studied in Tait's little volume (1st ed., 1868; 2nd ed., 1877), in vols. 43 and 44 of the 4th series of the 'Phil. Mag.,' in his 'Recent Advances in Physical Science' (especially the preface to the 3rd edition, 1885), and in the 2nd

of discovery and as to the real points at issue have arisen. The history of thought only takes note of these in so far as they are indications of what was of real (not of personal) interest in the process, and are thus a measure of the value which was inherent in its development.

None of the different views or theories with which the earlier generations of philosophers during the century operated seemed sufficient to give an insight into the real essence, the φύσις, of natural phenomena. Neither the astronomical nor the atomic nor the kinetic view was all-embracing. On the Continent, both in France and in Germany, the sciences were rigidly marked off from one another, the connecting links were few and ill-defined, and speculations as to the general forces and agencies of nature were left to metaphysicians and treated with suspicion. In England alone the name of natural philosophy still obtained, and in the absence of separate schools of science, such as existed abroad, suggested, at least to the self-taught amateur or to the practical man, the existence of a uniting bond between all natural studies. It is significant that the term under which we now comprise, and by which we measure, all natural agençies, the term Energy, was first distinctly used in this
4. The term first used by Young. sense by Dr Thomas Young in his lectures on Natural Philosophy,[1] a course which, be it noted, also embraced

edition of the 2nd vol. of Clausius, 'Die mechanische Wärmetheorie' (Braunschweig, 1879), p. 324, &c. In the labyrinth of these controversies I have found Helm a fair and conscientious guide.

[1] Vol. i. p. 59 of the edition of Kelland. Young says: "The term Energy may be applied, with great propriety, to the product of the mass or weight of a body into the square of the number expressing its velocity. . . . This product has been denominated the living force (the *vis viva*), . . . and some have considered it as the true measure

Chemical Science, though for merely external reasons this was summarily handled. It is equally significant that the first valuable suggestions as to the connection of the various sciences, and the practical or common measure of the various agencies, came from practical or professional persons who took an outside and general view of physical and chemical processes and their application in arts and medicine. Young himself was a medical man, as were Robert Mayer and Helmholtz after him. Practical men such as Watt felt the necessity of measuring not so much forces (in the Newtonian sense) as the action of forces, and introduced the term power, and the quantity called 5. Watt introduces the term "power."
horse-power[1] to measure the capacity of an engine for doing work. Newton had already measured this action[2]

of the quantity of motion; but although this opinion has been very universally rejected, yet the force thus estimated well deserves a distinct denomination." See also p. 172.

[1] The quantity called horse-power was introduced by Boulton and Watt to measure the power of the engines they built and sold at Soho towards the end of the eighteenth century. They caused experiments to be made with the strong horses used in the breweries in London, and from the result of these trials they assigned 33,000 lb., raised one foot per minute, as the value of one horse-power. Dr Young in his 'Lectures' has the following statement: "A steam-engine of the best construction, with a 30-inch cylinder, has the force of forty horses; and since it acts without intermission, will perform the work of 120 horses or of 600 men, each square inch of the piston being nearly equivalent to a labourer" (vol. i. p. 103).

[2] See the Scholium to the "Axiomata sive Leges Motus," p. 25 of the first edition of the 'Principia,' in which the "Agentis Actio" is measured "ex ejus vi et velocitate conjunctim." Thomson and Tait ('Natural Philosophy,' 1886, part i. p. 250 *sqq.*, and Tait, 'Dynamics,' 1895, p. 181) have drawn attention to the fact that this passage of the 'Principia' contains implicitly the modern notion of energy, and the principle of the conservation of energy. The continental historians named above are inclined to give Huygens credit for having first made explicit use of the idea of the conservation of the quantity now termed energy, and they trace the further elucidation of it to the Bernoullis, especially John Bernoulli, who repeatedly speaks of the "conservatio virium vivarum," and "urges that where *vis viva* disappears, the power to do work (*facultas agendi*) is not lost, but is only changed into some other form" ('Opera,' 1742, vol. iii. pp. 239 and 243, quoted by Planck, *loc. cit.*, p. 10).

of a force by the product of the force (itself measured by the velocity of a moving mass) and the velocity or space per unit of time through which it pushes or pulls a moving body, and Leibniz[1] had suggested the term *vis viva* to distinguish it from the *vis mortua*, the force or pressure itself. But the first clear and consistent fixing of the terminology which has since been universally adopted is to be found—not in the 'Mécanique analytique' of Lagrange (that classical work on theoretical mechanics), but in the 'Mécanique industrielle' of Poncelet (1829).[2] He introduced the term "mechanical

[1] Leibniz's occupation with dynamics began with his publication of two theses in 1672, which he dedicated respectively to the Academy of Sciences in Paris and to the Royal Society. In distinction from the writings of Huygens and Newton, where precise definitions take the place of metaphysical discussions, Leibniz's tracts — except in the comparatively rare cases where he confines himself to mathematical formulæ—are vitiated, like those of Descartes, by philosophical speculations. Thus, though eminently suggestive, they contributed little to the clearing up of ideas. Influenced by Huygens and by Newton, he opposed in 1686 the ideas of Descartes on the measure of force, and has the merit of having introduced the term *vis viva* in 1695, and of having started the celebrated discussion on the measure of force which was carried on during fifty-seven years on the Continent, and only settled by D'Alembert in his 'Traité de Dynamique' (1743) by stricter definitions. An excellent account of the questions involved, and of the gradual clearing up of ideas, will be found in Prof. Mach's historical treatise on dynamics referred to above. See the English translation by M'Cormack, p. 272, &c. It is there shown that one of the great defects of Descartes' and Leibniz's dynamical writings was the want of a clear definition of mass or inertia; also that this conception follows more simply from Newton's definition of force than from Huygens' conception of work (ibid., p. 251).

[2] By the side of, and sometimes in opposition to the purely analytical school headed by Lagrange, Laplace, and later by Cauchy, there grew up in Paris the school of practical mathematicians which taught the application of theory to practice, to problems of artillery, engineering, and architecture. They created modern geometry, and to a great extent modern mechanics. Monge, Coulomb, the elder Carnot, Poncelet, Coriolis, were their leaders: Navier, Lamé, Chasles, de Saint Venant, followed, and combined their more synthetic methods with the analytical methods of the former school. Through Monge, Carnot, Navier, and Poncelet, geometry and dynamics were led into those channels which have since been so successfully followed in all applied work. To them mathematics was not merely the science of magnitude, but quite as much that of position, of design and perspective, of mechanical work and effect. They introduced a whole series of new and practical ideas, drawn from their own applications, and created a new vocabulary. They worked hand in hand with physicists and chemists, some of whom had little taste for the extremely abstract and analytical methods of the school of Laplace and Cauchy. Poncelet's original geometrical work, which will occupy us in a later chapter, led him into many controversies. It was, however, greatly appreciated in Germany and later in England. His influence on German applied mechanics has been quite as great as that on geometry; and the great text-books of mechanics by Weissbach, Redtenbacher, Rühlmann, and others, are as much indebted to Poncelet and other French models as the German text-books on mathematics, physics, and chemistry were for a long time to the well-known works of Biot, Pouillet, Cauchy, Francœur, Lamé, Regnault, and others. The influence of Poncelet on practical mechanics, and especially in the fixing of an adequate terminology, can therefore be studied equally well in French and in German historical writings. Among the former I may mention especially the 'Exposé de la Situation de la Mécanique appliquée par Combes, Phillips et Collignon,' Paris, 1867, and among the latter, notably the above-mentioned writings of Helm, who traces the growth of the conception of mechanical work in French writings, and its influence on German thought ('Energetik,' p. 12, &c.) See also Dühring, *loc. cit.*, p. 471, &c. I may also refer to Heun's Report ('Jahresbericht der deutschen Mathematiker-Vereinigung,' vol. ix. part 2, 1901), where the sciences comprised in "Mechanics" are distinguished according as they are astronomical (Laplace, Poincaré), physical (English mathematical physics, Kirchhoff, Helmholtz, Hertz), geometrical (Poinsot, Chasles, Ball), or technical (Watt, Poncelet, Rankine).

work" for the definite quantity which had before him been variously designated as power, effect, action, &c., and he distinctly states that the inertia of matter transforms work into *vis viva* and *vis viva* into work. He also measures this quantity "work" quite in the modern fashion—by the "kilogrammetre," which gives the same conception as the foot-pound, only in a different measure.

6. Poncelet introduces the term "mechanical work."

Long before the terminology thus invented and fixed by Watt, Young, and Poncelet had been accepted by scientific writers, a change in the current notions on the forces of nature had been gradually brought about from quite a different quarter. Uninfluenced by the theoretical views which were developed and firmly held

by the school of which Laplace was the most distinguished representative, natural philosophers like Black,[1] Rumford, and Davy had approached the study of those phenomena where heat and chemical change are the prominent features. The phenomena which they studied experimentally can be comprehended under the head of the disappearance and appearance of heat as measured by the thermometer, or as recognisable directly by our sensation of heat. Black accounted for the disappearance of heat by the doctrine of latent heat, and measured this by the capacity[2] for heat, or the specific heat of different substances. Rumford made exact measurements of the heat generated by friction, and showed that Black's doctrine of latent heat did not account for it. Both Black and Rumford were led to science from the side of practical interests. Black, like Young after him, was a physician. Rumford was all through his life occupied with the

7. Black, Rumford, and Davy.

[1] Joseph Black (1728-99), one of the founders of chemistry, and a prominent figure in that illustrious circle of philosophers who, during the second half of the eighteenth century, made the literature and science of Scotland renowned over the whole world, published very little, being mostly known through his teaching and his pupils. His name is, even to the present day, rarely to be found in French books; whereas in Germany, mainly owing to the historical writings of Herrmann Kopp, and quite recently of Prof. E. Mach, his great merit and originality have been fully recognised. See Kopp, 'Geschichte der Chemie,' vol. i. p. 226, &c.; 'Die Entwickelung der Chemie,' 1873, pp. 57, &c., 88, &c.; E. Mach, 'Die Principien der Wärmelehre,' 1896, p. 156, &c. Black, who as early as 1755 had shown that carbonic acid gas could disappear as a gas and become "fixed," showed later that heat could disappear as temperature and become "latent." By himself, indeed, the former important discovery was not interpreted against the then reigning phlogistic theory, nor was the latter used to upset the material theory of heat. Now, however, both discoveries are corner-stones in the history of science.

[2] According to Dr Young ('Lectures,' new ed., p. 499), the term "capacity" is due to Dr Irvine, who, as well as Dr Crawford, was much influenced by Black's lectures. These were first published in 1802 by Robison, three years after the author's death.

practical application of scientific knowledge. Black's experiments and measurements contributed largely to fix the difference between temperature and quantity of heat; he demonstrated clearly that heat may disappear in the form of temperature and exist as latent heat, that is, heat not discoverable by the thermometer. He, however, adhered to the view that heat was a material substance, which, though it might become latent, did not disappear as such. Rumford[1] was the first who definitely went a step further and suggested the convertibility of heat and mechanical work. It was not the disappearance of heat but its appearance when mechanical work was performed which attracted his attention. After eliminating all the sources from which the heat produced during the boring of cannon could have been derived, he comes to the conclusion that "it appears to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner the heat was excited and communicated in those experiments, except it be motion." Davy, who, like Black, approached science in the interests of the medical man, comes to the conclusion in his first published papers, from experiments on the generation

[1] Count Rumford's "Inquiry concerning the Source of the Heat which is excited by Friction" was published in a later edition of his 'Essays.' The experiments with the boring of cannon were carried on at Munich in 1796 and 1797; the substance of the essay was read before the Royal Society in January 1798. The 'Essays' were republished in America and translated into several foreign languages. See Rumford's 'Works,' London, 1876, vol. i. p. 482, and vol. ii. p. 471. In 1804 Count Rumford published, in his 'Mémoires sur la Chaleur' (Paris, an. 13), a "Historical Review of the Various Experiments on Heat" ('Works,' vol. iii. pp. 138-240).

of heat by friction and percussion, that heat is not matter, but "may be defined a peculiar" motion, probably a vibration,[1] of the corpuscles of bodies tending to separate them. Rumford's and Davy's memoirs referred to belong to the last years of the eighteenth century. Dr Young, in his celebrated lectures on natural philosophy, discussing the experiments of Rumford and Davy came to the conclusion "that heat is a quality, and that this quality can only be motion." He refers to Newton's view "that heat consists in a minute vibratory motion of the particles of bodies," and to his own undulatory theory of light. This analogy with light seems to have for a long time served to unify the speculations[2] of those who were inclined to

[1] See his "Essay on Heat, Light, and the Combinations of Light," which appeared in Beddoes' 'Contributions to Physical and Medical Knowledge,' 1799. This essay Davy soon after condemned as "infant chemical speculations," from which he turned away to experimental work, remarking that chemical knowledge was yet too incomplete to allow of generalisations, and that the "first step will be the decomposition of those bodies which are at present undecompounded." This was written in 1799. In 1800 (30th March) Volta's invention of the "pile" was communicated to the Royal Society, and on the 30th April of that year the first pile was constructed in this country. See the first and second volumes of Davy's 'Collected Works,' London, 1839. Davy's first publication on voltaic electricity appeared in the September number of 'Nicholson's Journal.' Though the speculations of Davy on heat and light, in which heat is conceived to be motion and light (strangely) to be material, were discarded by him, they attracted the attention of Franklin and of Count Rumford. Davy states that his experiments on the generation of heat "were made long before the publication of Count Rumford's ingenious paper on the heat produced by friction" (*loc. cit.*, vol. ii. p. 117). In spite of his own refusal to follow up the lines of thought suggested by them, they were probably the cause of Davy's appointment as lecturer on chemistry at the Royal Institution: see vol. i. p. 83; also Memoir of Count Rumford ('Works,' vol. i. 417), and Paris's 'Life of Davy,' vol. i. p. 112, &c. Tait, in 'Recent Advances,' gives a full account of Rumford's and of Davy's work.

[2] See 'Young's Lectures,' 51 and 52. In the second edition, published by Kelland forty years after the Lectures were delivered, the editor makes the following significant remark: "The theory of heat may be said to rest where it did at the time these Lectures were written. The facts which have just been mentioned clearly point out its undulatory character" (p. 506). Between the years 1835 and 1845 theoretical ideas on the nature of heat were entirely dominated by the remarkable discoveries of Melloni, Baden-Powell, Forbes, and others referring to radiant heat, which was shown to have the same properties of reflexion, refraction, and polarisation as light possessed. The analogy of this form of heat with light threw into oblivion the beginnings of a more general mechanical theory of heat, which—as we shall see further on—had been laid by Sadi Carnot in 1824.

embrace a mechanical or kinetic view of the nature of heat. Joule, as stated above,[1] was the first who emancipated himself from it.

But whilst these suggestions that heat may be regarded as somehow connected with motion remained mostly vague and undeveloped, they tended to impress upon the scientific mind the interchangeability—or, as it was called, the correlation of the different forces of nature; and the idea seems to have forced itself independently on many minds, through the study of very different groups of natural phenomena. In Germany we may look upon Liebig as the centre of a great scientific movement which tried by means of chemistry to bring the realms of organic and animated existence under the treatment of exact methods. Not only were the methods of organic analysis perfected by him and his school, and many compounds investigated which appeared to be specially the bearers of the living process; but he was also among the first to study the economy of living organisms, the circulation of matter, and the play of the varied processes by which life is maintained. Among these processes, the phenomenon of animal heat, its origin, and the part it plays in the living organism attracted special attention.

8. Correlation of forces.

9. Liebig.

[1] See vol. i. of this work, p. 434.

By his work on organic chemistry, by his many controversies, such as that on fermentation, by his popular letters on chemistry, and especially by his great influence as a teacher, Liebig himself did much to bring about an alliance of the separate sciences and a connection between practical pursuits and abstract research, and to draw attention to the interdependence of the various forces of nature. Only second in influence was Johannes Müller of Berlin. Among the many expressions which took their origin in the circle of studies suggested by these influences, we may select three as giving increasingly clear emphasis to the point now under consideration—viz., the correlation of all the physical forces of nature. These expressions are those of the convertibility of forces, of the existence of a common measure of force, and of the conservation implying the perdurability of a certain quantity—now termed Energy—of which all phenomena are merely a partial exhibition. They are connected with the names of Karl Friedrich Mohr, Julius Robert Mayer, and Hermann Helmholtz.

10. John Müller.

Were it my object merely to write the history of science, I should probably follow the example of some historians[1] and omit altogether the first of these names in the present connection. But as my object is to write the history of scientific thought, I feel bound to give a

[1] Mach, in his recent very lucid and valuable work, 'Die Principien der Wärmelehre,' Leipzig, 1896, does not mention Mohr. On the other side, Helm ('Die Energetik,' 1898, p. 9) mentions Mohr and likewise Planck ('Das Princip der Erhaltung der Energie,' 1887, p. 21). Tait's first edition of 'Recent Advances,' 1874, does not contain Mohr's name. The third edition gives a full account of Mohr's early papers (pp. 51 and 60, &c.) See also the appreciative article on K. F. Mohr in the 'Ency. Brit.'

foremost place to the short memoir of F. Mohr entitled "On the Nature of Heat," which appeared in 1837 in an obscure scientific periodical published at Vienna. The publication of it remained unknown, even to the author himself, and was certainly unappreciated by the scientific world for more than thirty years.[1]

11. F. Mohr.

[1] The story of Mohr's memoir is curious, not to say romantic. His original paper, 'Ueber die Natur der Wärme,' was offered to Poggendorf and refused, as were the later memoirs of Mayer and Helmholtz. A dread of introducing speculative matter into the 'Annalen' prevented likewise — as I related above (p. 66, note 2)—the appreciation of much of Faraday's later work. He then sent the MS. to Baumgartner, in Vienna, who—always interested in theoretical physics—printed it in a periodical ('Zeitschrift für Physik') of which he and von Holger were joint-editors. He did not inform the author of this. Mohr was a remarkably original thinker, in whose mind important ideas rose at times to extraordinary clearness, but who, like many original thinkers, did not always appreciate his own ideas at their true value, and accordingly treated them with neglect, and did not consistently develop them. In the present instance he contented himself with inserting an abstract in the 'Annalen der Pharmacie' (vol. xxiv. p. 141), of which he was then joint-editor, together with Liebig and Merck. He made no further inquiries as to the fate of his larger memoir, and, in conversation with friends up to the year 1860, as also in his 'Mechanische Theorie der chemischen Affinität' (Braunschweig, 1868, p. 45), used to deplore the loss of a document which, more fully than the short paper in the 'Annalen der Pharmacie,' would have established his priority in the clear enunciation of a remarkable principle which fifteen years later received general recognition. The matter would probably have rested there had it not been that Tyndall, in the year 1862, in a celebrated lecture before the Royal Institution, commenced that long series of historical and controversial publications in which many persons, including himself, Joule, Tait, Colding, Helmholtz, Akin, Bohn, Dühring, Zöllner, and others took part, and in which, among several claims prior to or contemporary with Mayer's, those also of Mohr received due recognition. It seems to have been especially Dr Akin who drew attention to Mohr's claims, and searched in the forgotten volumes of the Austrian periodical for the original memoir, which, unknown to the author himself, had been inscribed on p. 419 of the fifth volume. This discovery he announced to Mohr himself after having already, in November 1864 ('Phil. Mag.,' 4th series, vol. xxviii. p. 474), given several extracts, among which is the one quoted by me in the text. Mohr published, in 1869, a sequel to the above-mentioned book, entitled 'Allgemeine Theorie der Bewegung und Kraft,' in which he refers to Dr Akin's discovery, and reprints the original memoir in full. Since that time his name has figured in many historical accounts as one of the pioneers in the development of the energy-concep-

It forms, therefore, no link in the actual development of the energy-conception; but it is a significant evidence of the direction in which the ideas of natural philosophers were then moving, and of the high degree of clearness to which they rose in individual instances. When we read the following words: "Besides the known fifty-four chemical elements there exists in nature only one agent more, and this is called 'Kraft'; it can under suitable conditions appear as motion, cohesion, electricity, light, heat, and magnetism," it seems difficult, even after the lapse of two generations, to alter anything in this clear and simple enunciation of the law of the conservation of energy. It has indeed been stated that "unless some still earlier author should be discovered, there can be no doubt that Mohr is to be recognised as the first to enunciate in its generality what we now call 'conservation of energy.'"[1] At the same time, the case shows how little, at the beginning of a scientific movement, purely abstract statements are capable of really guiding research into fruitful channels. There is with Mohr no attempt to establish or apply an actual measure[2] of the amount of energy appearing in the various instances which he mentioned. This further
12. Mayer. step was taken five years later by J. R. Mayer, who can claim to be the first[3] to have ventured on a

tion; his merit being variously appraised according to the purely scientific, the philosophical, or the more practical standpoint taken up by various critics. See, *inter alia*, P. G. Tait's 'Recent Advances,' 3rd ed., p. 60, &c.; also the correspondence of Mohr and Mayer in the latter's 'Kleinere Schriften und Briefe,' ed. Weyrauch, p. 407, &c.

[1] See the article on K. F. Mohr in the 'Ency. Brit.,' 9th ed.

[2] See on this point Weyrauch, in Mayer's 'Kleinere Schriften,' p. 408.

[3] Helm ('Energetik,' p. 34) begins the list of undoubted determinations of the heat-equivalent with

numerical estimate as between mechanical energy on the one side, and the amount of one of the imponderables—*i.e.*, heat as measured by the thermometer—on the other. Although his methods were not free from objection,[1] while his arguments were mixed up with

Mayer, 1842. His determination is contained in his first paper, published, as was Mohr's, in Liebig's 'Annalen' (vol. xlii., May), with the title "Bemerkungen über die Kräfte der unbelebten Natur." The experiments performed by Rumford in 1798 were made the basis of a calculation of the heat equivalent, *i.e.*, of the weight which can be lifted one foot if the heat required to raise a pound of water 1° be converted into work against gravitation, and the figure turns out to be 1034 lb. as compared with 772 lb. given by Joule himself ('Phil. Trans.,' 1850; 'Joule's Papers,' vol. i. p. 299). The earlier computations of Séguin, based upon the work done by the expansion of steam, were referred to by Joule, Tyndall, and Tait in 1862 and 1864 ('Phil. Mag.,' 4th series, vols. xxiv. and xxviii.), and shown to lead to figures further off the mark than those of Mayer. In the course of this later controversy it became for the first time generally known that A. Colding, an engineer in Copenhagen, had a little later than Mayer (1843), and almost simultaneously with Joule, given a determination of the equivalent based upon friction of metals, which was lower than Mayer's. He accordingly now figures as second in Helm's list. One of Joule's earliest experiments with heat, "evolved by the passage of water through narrow tubes," gave the equivalent as 770, very near the figure, viz., 772, finally settled on as correct in 1850.

[1] The reasoning of Mayer is not completely contained in his first paper, which subsequently, on a suggestion of Joule's appeared in translation in the 'Phil. Mag.' (4th series, vol. xxiv. pp. 123, and 371 *sqq.*) The assumption (called by Thomson in 1851 "Mayer's hypothesis," see 'Math. and Phys. Papers,' vol. i. p. 213) that "the work spent in the compression of a gas . . . is exactly the mechanical equivalent of the . . . heat evolved," which Joule did not think it right to accept without satisfying himself by experiments (see 'Phil. Mag.,' 4th series, vol. xxiv. p. 122), was based by Mayer on an almost forgotten experiment of Gay Lussac's in the year 1807, as is evident from his subsequent paper, published in 1845 (reprint in 'Mechanik der Wärme,' ed. Weyrauch, 1893, p. 53), and still more from his correspondence with Baur previous to his first publication (ibid., p. 20, and 'Mayer's Briefe,' p. 130, September 1841). The subject was exhaustively investigated by Thomson and Joule in a joint-memoir on "the thermal effects of fluids in motion," 1852 (reprinted both in Joule's and Lord Kelvin's Scientific Papers), when it was shown that for air Mayer's hypothesis was approximately, but not absolutely, correct. So long, therefore, as the history of Mayer's reasoning was not completely known, it appeared as if he had by a kind of accident hit upon an approximately correct figure. See Tait, 'Recent Advances' (3rd ed., p. 53; but also Helm, 'Energetik,' p. 24, and Mach, 'Wärmelehre,' p. 249).

philosophical speculations which tended to prevent their ready acceptance, it cannot be denied that, as a first approximation, "his equivalent" was sufficiently near the truth to be practically useful.

But neither the happy generalisation of Mohr, which was lost or forgotten, nor the numerical estimate of Mayer, which remained unnoticed, succeeded in impressing contemporary philosophers with the importance of the subject. This was done almost at the same date, though quite independently, by the persistent and per-
13. Joule. severing experiments and measurements of James Prescott Joule, who laboured unnoticed and practically without support from 1841 to 1847, when he had the good fortune of gaining the attention and friendship of William Thomson (Lord Kelvin).[1]

[1] Joule not only defined more clearly the different data and conditions on which the correctness of the result must depend, but had also at his command a much greater wealth of novel experimental facts, brought together by his own resourceful mind. Thus from 1843 to 1850 he published no fewer than ten series of experiments, approximating from widely differing results to the true figure. See Helm's list ('Energetik,' p. 34). After he had laboured for more than five years his work was, in 1847, at the meeting of the British Association in Oxford, still almost unknown. He himself reports as follows in 1885 ('Joint Scientific Papers,' 1887, p. 215): "It was in the year 1843 that I read a paper 'On the Calorific Effects of Magneto-Electricity and the Mechanical Value of Heat' to the Chemical Section of the British Association at Cork. With the exception of some eminent men . . . the subject did not excite much general attention; so that when I brought it forward again at the meeting in 1847 the chairman suggested that, as the business of the section pressed, I should not read any paper, but confine myself to a short verbal description of my experiments. This I endeavoured to do, and discussion not being invited, the communication would have passed without comment if a young man had not risen in the section, and by his intelligent observations created a lively interest in the new theory. The young man was William Thomson, who had two years previously passed the University of Cambridge with the highest honour, and is now probably the foremost scientific authority of the age." See also Lord Kelvin's account of the meeting in 1847 in 'Popular Lectures and Addresses' (London, 1894, vol. ii. p. 556, &c.)

A pupil of Dalton, Joule was early drawn into the circle of ideas and investigations which are contained in Faraday's experimental researches. With much ampler means, and possibly also with a greater love for accurate quantitative measurements, than Faraday possessed, he grasped the great importance of the law of electrolytic equivalence as affording the means of accurately measuring chemical processes, and of giving definite expression to the vaguer ideas supported by Faraday and others that force was indestructible, and that the different forces of nature were mutually convertible. These ideas had received popular circulation and current expression in Grove's celebrated lectures on the "Correlation of Physical Forces" in 1842 and 1843. Joule, in whose mind they seem to have existed as axioms, set himself to devise accurate instruments and methods by which the convertibility of different forces, their "mechanical duty," could be measured, and their equivalence put into figures. The first numbers which Joule found differed considerably,[1] so that the conclusion arrived at that the mechanical duty or "value" of a degree of heat is a constant quantity could only have been drawn by one who had a strong *a priori*[2] con-

[1] For details see Helm, 'Energetik,' p. 34; also vol. i. p. 265, note, of the present work. Joule's equivalent varied from 742 to 890 foot-pounds, and was finally fixed at 772 in 1850, this figure being correct to $\frac{1}{2}$ per cent (Joule's 'Scientific Papers,' p. 328).

[2] Philosophical considerations are mixed up with all the early enunciations of the principle of the indestructibility of force, or energy as it was later more clearly termed. A predisposition to believe that some quantity besides matter could not be lost or created, but only preserved and transformed, existed in the minds of Mohr, Séguin, Mayer, Colding, Joule, Hirn, and has been traced variously back to the writings of earlier thinkers, such as Montgolfier, Faraday, Davy, Oersted, Leibniz, &c. Prof. Mach ('Wärmelehre,' p. 238, &c.) discusses this point fully. The principle gradually became firmly

viction in that direction. The experimental result did not satisfy Helmholtz, who, about the same time, was led to consider the origin of animal heat in living organisms, a problem with which Liebig[1] had been greatly occupied for several years. Without himself devising or instituting new experiments, or attempting any determination of the equivalent as others—notably Colding and Holtzmann—were doing, Helmholtz, in 1847, undertook a theoretical investigation which has since become classical—a corner-stone in the philosophy of the subject. He first of all gave the principle involved a correct mathematical expression, showed how it could be considered as an extension of the theorem known in abstract dynamics as the conservation of the *vis viva* of a mechanical system, attempted to define the nature of forces, in the Newtonian sense, which would be subject to the new principle, and brought it into logical connection with the axiom laid down and used by French philosophers, that perpetual motion is an impossibility. After clearing the ground so far as abstract dynamics is concerned and giving the necessary definitions, sharply distinguishing between acting (living) forces and mere tensions (dead forces), Helmholtz proceeds to draw all

14. Helmholtz.

established according as strict definitions, experimental proofs and figures, and mathematical formulæ took the place of vague speculations. Joule did the experimental, Helmholtz the mathematical, part of the work; but it is interesting to see how little the latter without the former was able to impress contemporary German writers with the value of the principle which he established. He himself even did not for a long time develop the line of reasoning which he had begun.

[1] See Helmholtz, 'Bericht über die Theorie der physiologischen Wärmeerscheinungen,' 1845, reprinted in 'Wissenschaftliche Abhandlungen,' vol. i. No. 1, also on Joule's early experiments in 'Ueber die Erhaltung der Kraft,' ibid., vol. i. p. 33.

other forces of nature into his consideration, showing, in the case of the phenomena of heat, electricity, galvanism, and magnetic induction, how the different agencies can be brought into comparison with mechanical ones by measuring the work they perform; refers to the attempts to fix the mechanical value of heat; concludes in each case that no observed phenomena—not even the processes in living organisms—stand in contradiction with the principle announced, and ends with the words: "I think in the foregoing I have proved that the above-mentioned law does not go against any hitherto known facts of natural science, but is supported by a large number of them in a striking manner. I have tried to enumerate as completely as possible what consequences result from the combination of other known laws of nature, and how they require to be confirmed by further experiments. The aim of this investigation, and what must excuse me likewise for its hypothetical sections, was to explain to natural philosophers the theoretical, practical, and heuristic importance of the law, the complete verification of which may well be looked upon as one of the main problems of physical science in the near future."[1] The reasons why this valuable document attracted little attention at the time and was set aside, as were the earlier contributions of Mohr and Mayer, by the central organ of experimental physics abroad, are interesting from a historical point of view. The first and main reason seems to have been that none of the three original and independent expressions contained any new experimental

[1] 'Gesammelte Abhandlungen,' vol. i. p. 67.

facts,[1] and that the then reigning school of natural philosophers in Germany discouraged theoretical deductions, as possibly leading back to the fatal "philosophy of nature," out of which they had only just escaped. Men of the intellectual eminence of Liebig, through whose labours an enormous mass of new facts had been accumulated, and who desired to see the more hidden processes of organic life subjected likewise to rigorous measurements, showed indeed a certain appreciation of the attempted definitions of Mohr and Mayer, struggling as he and they alike were under the still existing confusion in the fundamental conceptions.[2] And these were not

[1] See Mohr, 'Allgemeine Theorie der Bewegung und der Kraft,' p. 82, &c. Poggendorf did not reply to Mayer's repeated communications and did not return the MS.; the fact that he received it was first established by Zöllner, who in 1877 recovered the MS. from Poggendorf's heirs (Mayer's 'Schriften und Briefe,' ed. Weyrauch, p. 100), and gave a facsimile of it in his 'Wissenschaftliche Abhandlungen' (Leipzig, vol. iv., 1881, p. 672). Helmholtz, who in 1847 had no knowledge of Mayer's writings, did full justice to his claims in his address, 'Ueber die Wechselwirkung der Naturkräfte' (1854), and vindicated them against Tait's criticisms in a letter published by the latter in his 'Sketch of Thermodynamics' (Edinburgh, 1868); see Helmholtz, 'Wissenschaftliche Abhandlungen,' vol. i. p. 71, &c. Helmholtz closes his later comments on the subject ('Vorträge und Reden,' vol. i., 3rd ed., 1884, p. 74) with the following significant remark: "The best ideas run the risk of remaining barren, if not accompanied by that energy which lasts till the convincing proof of their correctness has been given." This explains the neglect of Mohr and Mayer, and why in England the interest in the energy ideas only became general after Joule's, Thomson's, and Rankine's labours, as Helmholtz himself remarks in 1854 ('Vorträge,' &c., p. 39).

[2] Helmholtz ("Ueber Mayers Priorität," 'Vorträge,' vol. i. p. 69) says: "That the [*i.e.* Mayer's] dissertation contained really important ideas, that it did not belong to the wide-ranging literature of vague suggestions, such as are annually served up by badly informed amateurs, could at best only be noticed by a reader who had already turned over in his mind similar reflections, and who could recognise them under the somewhat strange vocabulary of the author. Liebig, who, in the same year in which Mayer's dissertation appeared, published his book on animal chemistry, in which he fully discussed the question as to the origin of animal heat, was perhaps such a reader, and was therefore willing to insert the article in his annals." The same remark would refer equally to Mohr's earlier essay. It is now known from Mayer's published correspondence that some remarks of Liebig himself, which appeared early in 1842, induced him to send him his first paper in order "not to lose the right of priority" (letter to Griesinger, 5th-6th December 1842, in 'Schriften und Briefe,' ed. Weyrauch, p. 190). Mayer there says: "Liebig wrote to me, *inter alia*: 'As to what force, cause, and effect are, there exist in general such confused notions that an easily understood explanation must be considered to be of real value.' One would accordingly think that he himself considers himself quite above this general confusion; that this is not so, I could see sufficiently from his 'phenomena of motion in the animal organism' (Liebig, 'Die organische Chemie, &c.,' 1842, p. 183, &c.)"

sufficiently cleared up in Mohr's short *aperçu*, which does not attempt to distinguish between the two different meanings of the word force, nor in the earlier papers of Mayer, who, however, in later writings shows a clear appreciation of the difficulty. In Helmholtz's memoirs the desired clearness was only attained by mathematical reasoning, which in his age and country was accessible to but few naturalists. The second and probably the fundamental obstacle in the way of a just recognition of the new truth lay in the fatal use of the term "force" in two distinct meanings. Popularly the difficulty has only been removed by the creation of a new

15. "Work" and "energy" introduced by Clausius and Thomson.

vocabulary, and dates from the introduction of the term "work" by Clausius in 1850, and of the term "energy" by William Thomson, who adopted it from Young in the year 1852. The confusion which had been kept up by employing the word "force" to mean not only pressure or dead force (in the Newtonian sense) but also acting force (*vis viva* in the Leibnizian sense), and with this confusion the whole meaning of the great controversies which raged for many years between the Cartesians and Leibnizians on the correct measure of force, was then removed, and a grammatical and logical founda-

tion secured on which a new generation could enter at once into the possession of correcter dynamical and physical views. It is now being recognised more and more that the word "force" applies only to a mathematical abstraction, whereas the word "energy" or "power to perform work" applies to a real quantity; and there are not wanting suggestions that the former should be altogether banished from scientific text-books, and that the latter denotes not merely a property of matter, but that it is after matter the only real thing or substance in the material world.[1]

This radical change in the fundamental notions which underlie all physical reasoning was not brought about, however, till the vaguer views expounded by Mayer in Germany, and the exact measurements of Joule in England, had been united by the independent labours of Thomson and Clausius, whose earliest researches (also carried on independently of each other) had been suggested by the

[1] The late Prof. P. G. Tait has on various occasions expressed himself in this sense. See his lecture on "Force," delivered before the British Association, Glasgow, in 1876, and reprinted in 'Recent Advances,' 3rd ed., also the closing paragraphs of his article "Mechanics," in the 9th ed. of the 'Ency. Brit.,' reprinted as 'Dynamics,' 1895, where he says (p. 356): "The only other known thing in the physical universe, which is conserved in the same sense as matter is conserved, is energy. Hence we naturally consider energy as the other objective reality in the physical universe, and look to it for information as to the true nature of what we call force;" and (p. 361): "In all methods and systems which involve the idea of force, there is the leaven of artificiality. The true foundations of the subject, based entirely on experiments of the most extensive kind, are to be found in the inertia of matter, and the conservation and transformation of energy. With the help of kinematical ideas, it is easy to base the whole science of dynamics on these principles; and there is no necessity for the introduction of the word 'force,' nor of the sense-suggested ideas on which it was originally based." We must, however, in that case extend the conception of matter to embrace also the ether (see Tait, 'Properties of Matter,' p. 5, 2nd ed.)

still earlier writings of Sadi Carnot and Clapeyron in France. Thomson's interest in the subject dates from the middle of the 'forties. He was then occupied with finding a method for measuring heat on the absolute scale. Mohr, Mayer, and Helmholtz all approached the thermo-dynamical problem in the medical or physiological interest. Trained in the school of Liebig and Johannes Müller, they were led to study the economics of organic processes and the mechanism of the physiological phenomena of animal heat, of motion, and of nutrition. Sadi Carnot, as after him Clapeyron in France and Joule in Manchester, approached the thermo-dynamical problem from the side of practical interests, created by the introduction and universal application of steam in the useful arts. The great change worked by the steam-engine, especially in England, the utilisation of coal and ironstone, the foundation of England's growing industrial wealth, seemed to Sadi Carnot to be concentrated in the problem of the motive power of heat; as to Liebig, the key which would unlock the mysteries of vegetable growth, of animal nutrition, and of human labour, with their economic, industrial, and political aspects, lay in the problem of combustion. As in the domain of electrical science, so in that of thermotics, the first thing to do was to arrive at a correct method of measuring heat as distinguished from temperature. It was a problem of applied mathematics. About the same time Gauss had established the system of absolute measurement from a universal point of view, and he and Weber had applied it to magnetic and electrical phenomena. Thomson set himself to do the same thing in thermotics, and

16. Sadi Carnot.

he found in the ideas expounded by Poncelet, Sadi Carnot, and Clapeyron, the means of accomplishing the object. We now see how there lay, in the fundamental problem of thermo-dynamics, the unifying idea of sciences hitherto far apart and working on independent lines and with independent standards of measurement, speaking, as it were, separate languages. And what was the new idea which lay concealed in Sadi Carnot's forgotten pamphlet?[1] In Carnot's original memoir it appears as an axiom at the beginning of his reflections. "The production of motion," he says, "in steam-engines is always accompanied by a circumstance on which we must fix our attention. This circumstance is the re-establishment of equilibrium, or level, in the caloric—that is to say, its passage from one body where the temperature is more or less elevated,

[1] The story of Sadi Carnot's memoir is not less curious than that of Mohr's first paper. It was first given by Lord Kelvin in his earliest article, "On an Absolute Thermometric Scale" (1848), reprinted in 'Math. and Phys. Papers,' vol. i. p. 100), and "An Account of Carnot's Theory" (1849, ibid., p. 113). He had in 1845 searched in vain for the 'Puissance motrice du Feu' in all the bookshops of Paris. In 1848 he obtained a copy from Lewis Gordon in Glasgow. It was known to him before through Clapeyron's memoir in the 14th vol. of the 'Journal de l'École polytechnique' (1834). Sadi Carnot published his memoir as a pamphlet in 1824. It has since been republished by his brother, Hippolyte Carnot ('Réflexions sur la Puissance motrice du Feu et sur les Machines propres à développer cette Puissance,' Paris, Gauthier-Villars, 1878), with important posthumous papers, from which, *inter alia*, it is evident that Carnot, before he died, had abandoned the material theory of heat, and actually, by an unknown process, calculated the mechanical equivalent of heat as 360 kilogrammètres. As in several other cases, so also in that of Sadi Carnot, the line of reasoning initiated by Laplace, and brilliantly developed by his school, militated against the acceptance of the dynamical as opposed to the material conception of the phenomena of heat; and M. Bertin, in his "Rapport sur le Progrès de la Thermodynamique en France" ('Recueil de Rapports,' &c., p. 5) could write in 1867: "Il faut bien l'avouer, parceque c'est la vérité: nous sommes restés longtemps, je ne dis pas rebelles, mais étrangers aux nouvelles idées: elles nous sont restées trop longtemps inconnues, et encore aujourd'hui, on peut regretter qu'elles n'occupent pas une place plus considérable dans notre enseignement scientifique."

to another where it is lower. . . . The production of moving force is therefore due in steam-engines, not to a real consumption of caloric, but to a transference from a hot body to a cold body."[1]

17. Carnot introduces the idea of "availability."

18. Thomson introduces the idea of "dissipation."

If it is the object of physical science to describe the processes of nature completely and in the simplest language, we have here an instance of a description of a very general property in very simple language, and in terms which reduce it to a measurable quantity. Without this, progress is impossible. It is not likely, however, that Carnot saw the full significance of his simple statement, how in it he had introduced into physical and mathematical science the great question of the availability of the forces of nature, as Mohr and Mayer in Germany, and Faraday and Grove in England, somewhat later, dwelt on the correlation or interchangeability of those forces. The two ideas were separately developed. When they came together in one mind, when Thomson fully realised the importance and meaning of both—as he undoubtedly did earlier than any other natural philosopher—he at once established the great doctrine of the dissipation, also called degradation or depreciation, of energy. But it required some modification of Carnot's enunciation of this general property before it could be put into its modern form. This modification was preparing itself in Carnot's own mind, as his papers, posthumously published, have revealed to us.[2] What required to be modified was the word

[1] Carnot, 'Puissance motrice,' ed. 1878, pp. 5 and 6.

[2] His notebook contained the following entry ('Puissance motrice,' ed. 1878, p. 90): "Lorsqu'une hypothèse ne suffit plus à l'explication des phénomènes, elle doit être abandonnée. C'est le cas où se

caloric. Carnot was brought up under the influence of the school that looked upon heat as an imponderable substance which might hide itself—might become latent —but could not be created or destroyed. This was the view of Black, of Laplace, of Fourier; it was not the view of Cavendish, of Davy, of Rumford. The views of the former were embodied in great treatises, and consistently worked out with much collateral extension of physical and mathematical knowledge; the views of the latter were expressed in detached experiments and in casual reflections.

19. Fourier. Fourier[1] had just (1822) given to the world his epoch-making work, the 'Théorie analytique de la Chaleur,' in which he had stated that "the properties of heat form a special order of phenomena which are not to be explained by principles of motion and equilibrium;"[2] and again, "There exists a very

trouve l'hypothèse par laquelle on considère le calorique comme une matière, comme un fluide subtil." Again (p. 92): "La chaleur est le résultat d'un mouvement. Alors il est tout simple qu'elle puisse se produire par la consommation de puissance motrice et qu'elle puisse produire cette puissance. Tous les autres phénomènes . . . pourraient s'expliquer dans cette hypothèse : mais il serait difficile de dire pourquoi, dans le développement de la puissance motrice par la chaleur, un corps froid est nécessaire, pourquoi, en consommant la chaleur d'un corps échauffé, on ne peut pas produire du mouvement." And (pp. 93 and 94): "Lorsque l'on fait naître de la puissance motrice, par le passage de la chaleur du corps A au corps B, la quantité de cette chaleur qui arrive à B, cette quantité est-elle la même, quel que soit le corps employé à réaliser la puissance motrice? Y aurait-il moyen de consommer plus de chaleur à la production de la puissance motrice et d'en faire arriver moins au corps B? Pourrait-on même la consommer tout entière sans en faire arriver au corps B? Si cela était possible, on pourrait créer de la puissance motrice sans consommation de combustible et par simple destruction de la chaleur des corps." And (p. 94): "La chaleur n'est autre chose que la puissance motrice, ou plutôt que le mouvement qui a changé de forme. C'est un movement dans les particules des corps."

[1] On the tardy reception and recognition of Fourier's work see vol. i. p. 241, note, of this work.

[2] 'Théorie analytique de la Chaleur,' 1822: 'Discours préliminaire,' p. iii.

extensive class of phenomena which are not produced by mechanical forces, but which result solely from the presence and accumulation of heat. This part of natural philosophy cannot be brought under dynamical theories; it has principles peculiar to itself, and is based upon a method similar to that of the other exact sciences.[1] . . . The dilatations, indeed, caused by the repulsive force of heat, the observation of which dilatations serves as a measure of temperature, are dynamical effects; but it is not these dilatations which we calculate when we investigate the laws of the propagation of heat."[2] He proceeds to build up this new science "upon a very small number of simple facts, of which the causes are unknown, but which are gathered by observation and confirmed by experiments,"[3] and he thus arrives at certain general relations, expressed in the form of equations, which are different from, though analogous to, and not less rigorous than, the general equations of dynamics.

One of the great experimental facts upon which Fourier bases his theory of the propagation (*i.e.*, the conduction and radiation) of heat is this, that all motion of heat depends on differences of temperature. He examines how differences of temperature are equalised and deduces the law of the flow of heat.[4] Although he does

[1] Fourier, 'Théorie analytique,' p. 13.

[2] Ibid., p. 14.

[3] Ibid., pp. xi, 18, 39.

[4] I cannot here omit to point out how elegantly Prof. Mach has translated into the language of common-sense the whole process of Fourier for establishing the fundamental equation of the theory. See his 'Principien der Wärmelehre' (Leipzig, 1896), pp. 78, &c., 116 *sqq.* Every student of physics should read the chapters referring to this subject. The mathematical formulæ will thus become living to him; but he will also see how necessary the abstract mathematical expression of common-sense conceptions is in order to avoid false reasoning.

not find it necessary to enter upon any theory of the nature of heat, the analogy with the flow of water from higher to lower levels would naturally present itself. For his purpose this analogy had no importance. For the purposes of Sadi Carnot, who noticed that upon the difference of temperature depended not only the flow of heat, but also the work it might eventually

20. His influence on Carnot.

do, the same analogy seemed all-important. "We may," he says, "justly compare the motive power of heat with that of a fall of water: both have a maximum which cannot be exceeded. The motive power of a fall of water depends upon its height and the quantity of the liquid; the motive power of heat likewise depends on the quantity of caloric employed and on what we will take the liberty of calling the height of its drop—that is, the difference of temperature of the bodies between which the exchange of caloric has taken place."[1] In this analogy two further assumptions seem to be implied: First, that the work capable of being done is in direct proportion to the difference of levels of height or of temperature; secondly, that the quantities with which we operate, of water or of caloric, remain the same, before and after the fall. Neither of these inferences is necessary; neither is permissible. Carnot does not adopt the first inference,[2] but he does adopt the second,[3] though he significantly remarks that the

[1] 'Puissance motrice du feu,' ed. 1878, p. 15.

[2] "Dans la chute d'eau, la puissance motrice est rigoureusement proportionelle à la différence de niveau entre le réservoir supérieur et le réservoir inférieur. Dans la chute du calorique, la puissance motrice augmente sans doute avec la différence de température entre le corps chaud et le corps froid; mais nous ignorons si elle est proportionelle à cette différence" (ibid., p. 15; compare also pp. 38, 39).

[3] "La production de la puissance

foundations on which the theory of heat rests require careful examination.[1] Further thought evidently led him to doubt the correctness of the second assumption. It is the first point to which Thomson, more than twenty years after, directs his attention. He conceives the idea of measuring temperature by such a scale that for an equal drop in the scale—*i.e.*, by letting down heat by an equal number of degrees on the new scale—equal amounts of work shall be done.[2] The speculations of Sadi Carnot remained unnoticed for a long time. Ten years

21. Clapeyron's graphical method.

later Clapeyron[3] reverted to the subject, and put the reflections of Carnot into graphical form and into mathematical language. He introduced the conception, based on Carnot's theory, of the ratio of heat transferred from a higher to a lower level of temperature to the maximum of work obtainable,—a quantity independent of the substance employed,—and he called this fixed ratio Carnot's function. It was through his paper that

motrice est . . . due . . . non à une consommation réelle du calorique, mais à son transport d'un corps chaud à un corps froid, c'est-à-dire à son rétablissement d'équilibre" (ibid., p. 6).

[1] "Au reste, pour le dire en passant, les principaux fondements sur lesquelles repose la théorie de la chaleur auraient besoin de l'examen le plus attentif. Plusieurs faits d'expérience paraissent à peu près inexplicables dans l'état actuel de cette théorie" (ibid., p. 20, note). "La loi fondamentale que nous avions en vue . . . est assise sur la théorie de la chaleur telle qu'on la conçoit aujourd'hui, et il faut l'avouer, cette base ne nous parait pas d'une solidité inébranlable" (p. 50). As stated above (p. 118, note), Carnot emancipated himself from the conventional or material view of the nature of heat. See the appendix to the edition of 1878.

[2] See 'Cambridge Philosophical Society Proceedings,' June 1848; reprinted in Thomson's (Lord Kelvin's) 'Math. and Phys. Papers,' vol. i. p. 100.

[3] Benoit Pierre Émile Clapeyron was an engineer. In 1834 he published, in the fourteenth cahier of the 'Journal de l'École Polytechnique,' his "Mémoire sur la Puissance motrice de la Chaleur." It was through a translation of this paper in 'Taylor's Scientific Memoirs' that Thomson heard about Carnot's earlier work, and through a translation in Poggendorf's 'Annalen' (1843) that Helmholtz became acquainted with the subject.

Helmholtz in Germany, and Thomson in England, heard about Sadi Carnot himself. Sadi Carnot, so much earlier and so unlike Mayer, had nevertheless one point in common with him. This point seems to have given a common anchorage to all those thinkers who, in the course of a generation, gradually lifted the theory of heat and energy out of twilight into clear thought. Sadi Carnot, Mayer, Joule, Helmholtz, Thomson, all express

22. Perpetual motion impossible.

or imply the same idea — viz., the impossibility of a perpetual motion.[1] In one form or other this seems

[1] The conception of a "perpetual motion," or, as it is termed abroad, of a "perpetuum mobile," and that of its impossibility, have been changed and more clearly defined in the course of the hundred years which followed the decision of the Paris Academy of Sciences in 1775 not to receive in future any scheme of perpetual motion. Into the same class of axiomatic impossibilities were also thrown the "squaring of the circle" and the "trisection of the angle." Helmholtz (appendix to his Lecture on 'Die Wechselwirkung der Naturkräfte,' 1853, dated 1883) remarks that the proof of the impossibility did not then exist, and that the resolution was therefore based merely on the experience of past failures. The doctrine of Energy, the arithmetical discoveries of Gauss, and the elegant researches of Hermite and Lindemann, have thrown much light on these celebrated problems. In the last chapter of this volume I shall revert to the two latter; as to the first, the "perpetual motion," what follows may tend to clear the popular conceptions. Tait has correctly remarked that "perpetual motion is simply a statement of Newton's first law of Motion" ('Recent Advances,' 3rd ed., p. 74). He might have added that it took probably as much ingenuity on the part of Galileo to arrive at the principle of inertia—viz., that "all motion is perpetual until force interferes to alter and modify it"—as it took to formulate correctly the other principle that such a perpetual motion is of no use, because you cannot do any work with it, except by using it up or annihilating it. In the beginning of the nineteenth century the impossibility of a mechanical device for the so-called perpetual motion was universally admitted, though —as Rosenberger ('Geschichte der Physik,' vol. iii. p. 229, note) remarks — this was not also extended to physical processes, it being taught that the processes of nature represented a "perpetual cycle which uninterruptedly renewed itself." In fact, the truth was beginning to dawn that if motive power or energy could not be obtained out of nothing neither could it be destroyed. Carnot in 1824, and Mayer in 1842, both take it as an axiom that power cannot be created; Mohr in 1837, and Joule in 1843 and 1845, are equally convinced that power cannot be

to be an axiom with them, but even this apparently simple article of faith in natural philosophy meant something different to different thinkers according to the greater or less clearness of their physical conceptions. Helmholtz, in his celebrated memoir of 1847, conceives all natural processes to be ultimately reducible to purely mechanical processes, and in doing so he sees that a well-known law in mechanics, the conservation of the *vis viva*, must have a meaning for all natural forces. This he proceeds to develop. Others, like Faraday, Mohr, Grove, have a silent conviction that besides ponderable matter there is some other quantity in nature which is indestructible and cannot be created, but only changed and transferred; they frequently call it force, and thus entangle themselves or their readers in

destroyed. Under the influence of Oersted's philosophy Colding expresses similar ideas in 1843 (see 'Phil. Mag.'), 4th series, vol. xxvii. p. 58). In fact, during the fifth decade of the century the three conceptions of the impossibility of creating power, its indestructibility, and the convertibility of its different forms, were more and more clearly enunciated. They were at last expressed in the formula of the "conservation of energy." It was Thomson (Lord Kelvin) who then—in 1852—first clearly recognised that the old phantom of a perpetual motion was turning up again in a new form. (See his Essay on "Dissipation of Energy" in the 'Fortnightly Review,' March 1892, reprinted in 'Popular Lectures and Addresses,' vol. ii. p. 452.) Ever since Thomson's essay of 1852 naturalists and philosophers may be said to be trying to formulate in the simplest terms the great principle of nature, that though energy is never lost, it becomes — for our practical purposes — unavailable. Prof. Ostwald has expressed this by reviving the terminology of the perpetual motion. "It is not generally recognised that the principle of perpetual motion has two sides. On the one side . . . perpetual motion could be realised if one could create energy. . . . The expression of the impossibility of doing this is the first law of Energetics. . . . A perpetual motion could, however, on the other side be attained if it were possible to induce the large store of energy at rest to enter into transformations. . . . This might be termed a perpetual motion of the second kind." The impossibility of this Ostwald terms the second principle of Energetics ('Allgemeine Chemie,' vol. ii. part 1, p. 472; cf. Helm 'Energetik,' p. 304).

that confusion which the indefinite use of the word had caused, especially among Continental writers. One of the first practical applications of this idea as referred to the motive power of heat in Carnot's sense was made by William and James Thomson in 1849. They had both fully realised that lowering of temperature might be accompanied by the doing of work by heat, and that elevation of heat to a higher temperature meant expense of work. If, therefore, work could be done by heat without lowering the temperature, there was an apparent gain of motive power without corresponding expenditure. It was known that water at freezing temperature expanded in becoming ice: it was capable of doing work, frequently very destructive work, without a lowering of temperature. In order to convert water into ice of the same temperature, heat must be abstracted. Here, then, was a case of a possible transference of heat without fall of temperature, and the creation or gain of great power to do work; but, according to Carnot's principle, equality of temperature implied an absence of expenditure of work. So here was a case of gain without expenditure of power simply by a transference of heat at freezing-point. James Thomson [1] saw the solution of the paradox. If water

23. Application by William and James Thomson.

[1] The reasoning of James Thomson, based again upon the impossibility of a perpetual motion, is given in the following passage of his communication to the Royal Society of Edinburgh, dated January 2, 1849 (reprinted in his brother, Lord Kelvin's, 'Math. and Phys. Papers,' vol. i. p. 156): "Some time ago my brother, Prof. William Thomson, pointed out to me a curious conclusion to which he had been led by reasoning on principles similar to those developed by Carnot with reference to the motive power of heat. It was that water at the freezing-point may be converted into ice by a process solely mechanical, and yet without the final expenditure of any mechanical work. This at first appeared to me to involve an impossibility, because water expands while freezing; and therefore it seemed to follow that if a quantity of it were merely enclosed in a vessel with a movable piston and frozen, the motion of the piston consequent on the expansion being resisted by pressure, mechanical work would be given out without any corresponding expenditure; or, in other words, a perpetual source of mechanical work, commonly called a perpetual motion, would be possible. . . . To avoid the absurdity of supposing that mechanical work could be got out of nothing, it occurred to me that it is necessary further to conclude that the freezing-point becomes lower as the pressure to which the water is subjected is increased."

in expanding by freezing is made to do work, it overcomes pressure; it has to freeze under pressure. The temperature of water freezing under pressure must be lower than that of water freezing under ordinary conditions.[1] Knowing the mechanical duty of a degree of temperature and the work of the expansion of ice, he could calculate how much the freezing-point of water must be lowered by pressure. In 1850 his brother William Thomson verified this theoretical prediction by actual experiment.[2] It is well known how Helmholtz in 1865 made use of this theoretically predicted and practically verified phenomenon in his celebrated glacier theory.[3] Both James and William Thomson, when they drew the conclusions from Carnot's theory, still adhered to the doctrine of the entire conservation of heat.[4] But William Thomson, who was equally ac-

[1] "The mechanical pressure promotes—as is generally the case with the alternate action of different forces in nature—such a change, viz., melting of ice, as is favourable to the effect of its own action" (Helmholtz, 'Vorträge und Reden,' vol. i. p. 217).

[2] 'Proceedings of the Roy. Soc. of Edinburgh,' January 1850, reprinted in 'Math. and Phys. Papers,' vol. i. p. 165.

[3] Helmholtz, *loc. cit.*, p. 215 *sqq.*, where also the phenomenon discovered and called "regelation of ice," by Faraday, is similarly explained.

[4] It is important to notice this, as the formula with which we are now familiar, that the mechanical work gained meant consumption of heat, was not available at that time. This is significantly pointed out by Helm ('Energetik,' p. 69). The reasoning was accordingly more difficult and refined. James Thomson, however, had at the time some misgivings on the then prevalent view, and in a footnote he refers to the "possibility of the absolute

quainted with Carnot's ideas and with Joule's work, increasingly felt the necessity of reconciling both views in one consistent view. So did Clausius independently at Zürich. The result was the doctrine of the "conservation of energy,"—not of heat, as Carnot had it,—and the embodiment of the two correct ideas contained independently in Carnot's and Joule's work in the two well-known laws of thermo-dynamics[1] — viz., the conservation, equivalence, and convertibility of energy, as

24. The two laws of thermo-dynamics.

formation or destruction of heat as an equivalent for the destruction or formation of other agencies, such as mechanical work" ('Math. and Phys. Papers' vol. i. p. 161, note). The acceptance of the doctrine of the convertibility of heat and mechanical work — implying the conservation of energy in place of the conservation of heat, as Carnot had it—seems to have taken place in Lord Kelvin's mind immediately after his paper referred to above in consequence of a paper by Rankine "On the Mechanical Action of Heat" (Roy. Soc. Edinburgh, Feb. 1850), as is shown by his letter to Joule, dated October 1850 (*loc. cit.*, vol. i. p. 170). He there refers also to a memoir by Clausius in Poggendorf's 'Annalen' of April and May of the same year as adopting "Joule's axiom instead of Carnot's" (ibid., p. 173).

[1] The reconciliation of Joule's dynamical theory of heat with Carnot's doctrine, and the necessary modification of the latter, is contained in Lord Kelvin's classical memoir, "On the Dynamical Theory of Heat," in the 'Trans. of the Roy. Soc. of Edinburgh,' March 1851 ('Math. and Phys. Papers,' vol. i. p. 173 *sqq.*) In the introduction, Davy, Mayer, Joule, and notably Liebig, are mentioned as earlier supporters of the doctrine of the convertibility of heat into mechanical effect, Rankine and Clausius as the latest contributors (p. 176). The first and celebrated enunciation of the second law by Thomson is given at the very beginning (p. 179), and in the sequel the denial of it is shown to mean the possibility of a perpetual motion. A little farther on Thomson refers to Clausius in the words: "The merit of first establishing the proposition upon correct principles is entirely due to Clausius, who published his demonstration of it in the month of May last year" (1850). It has on the other side been admitted by Clausius ('Die mechanische Wärmetheorie,' 2te Aufl., 1876, vol. i. p. 358) that Thomson's independent development of the second law, though published later, is conducted from a more general point of view, whereas his own treatment was purely mathematical and confined to special cases. The most general and philosophical expression of the new principle was given by Thomson in his celebrated communication to the Royal Society of Edinburgh, April 19, 1852, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy" (reprinted in 'Math. and Phys. Papers,' vol. i. p. 511).

expressed in the first law, and the doctrine of the availability of energy as expressed in the second law. It was Thomson who first clearly saw that the axiom of the impossibility of a perpetual motion would be infringed if the first law of thermo-dynamics—the indestructibility of energy—was accepted without the second. For practical use, for doing work, it is not sufficient that energy be not lost; it must be available—get-at-able. Energy may be in a condition in which it is useless—hidden away—and to bring it forth again may either be for us impossible (if it be dissipated), or may require an expenditure of work—*i.e.*, of energy—to do so. The second law puts into mathematical language another very important and very striking property of the processes in nature. Let us dwell on this a moment.

The doctrine of the preservation of energy, of the equivalence of the different forms of energy, tended to put all the forms of energy on the same level. If they be convertible, they appear to be of the same value. If in doing work, energy was not consumed but only changed, it stood to reason that it might be changed back again, so that the work could be done over again. In other words, if all processes are purely mechanical processes—modes of motion—a supposition which very early forced itself with more or less clearness on the pioneers of the science of energy, they must be reversible: it must be possible to turn them round again, to undo what has been done, or to do what has been undone. Now the common-sense view of nature tells us at once that this is impossible; but it does not seem to have struck the earlier propounders of the doctrine of the

equivalence and correlation of forces, such as Faraday, Mohr, Mayer, Grove—not even Joule and Helmholtz—that if neither matter nor power is lost, the phenomena of loss and waste in nature and in human life remain unexplained. The only mind to whom this problem presented itself was Sadi Carnot, and it presented itself to him in an extreme form; for he started with the idea that even heat itself in doing work was not lost or destroyed, but handed over from the hotter body (the boiler of the steam-engine) to the colder body (the condenser of the steam-engine). We now know that this view was not correct—that the whole heat is not handed over, but always only a portion of the heat. But, with this exaggerated view in his mind, he tried to explain the phenomena of loss and waste, and he conceived that the explanation lay in the lowering of the temperature. "It would be difficult to say why"—though he had assumed it as an axiom that—"in the development of motive power by heat, a cold body should be necessary, why in consuming the heat of a heated body we cannot produce motion." [1] Heat at high temperature is of more value for doing work than the same amount of heat at

[1] The words quoted are taken from one of the fragments published in the year 1878 by H. Carnot from the posthumous MSS. of his brother, Sadi Carnot. In this fragment he approaches the modern conception that heat is the result of motion: he sees that all other phenomena can be explained by this hypothesis; but he pauses after having stated the difficulty quoted above in the text, and reverts, after some further queries, to the same difficulty in the words, "Can one consume the heat entirely without letting any arrive at the body B [viz., from a body A]? If this were possible, one could create motive power without consumption of fuel, and simply by the destruction of the heat of bodies" ('Puissance motrice, &c.,' ed. 1878, pp. 92 and 94). It is interesting to see how nearly these reflections approach to those made more than twenty years later by Thomson.

low temperature. By doing work, as also by conduction, and radiation with absorption, this inequality of temperature is spent, *i.e.*, lost. Clausius and Thomson alone seem to have grasped the value of this conception. The difficulty was to put it into mathematical language—into calculable terms. Each did this independently. Thomson, more than any other thinker, put the problem into common-sense language, brought the subject home to the practical reason; at the same time he put it into mathematical language, allowing the conceptions of waste [1] and of value and of availability (or usefulness) of energy to be scientifically—that is, measurably—defined. In 1851 he put the axiom upon which Carnot's reasoning is based (without knowing the words of Carnot quoted above) into the following words: [2] "It is impossible by means of inanimate material agency to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects." He saw at once, when adopting Joule's doctrine of the convertibility of heat and mechanical work, that, if all processes in the world be reduced to those of a perfect

[1] The term "wasted," as distinguished from "annihilated," is first introduced in Part 1 of the "Dynamical Theory of Heat," 1851, p. 189 of 'Math. and Phys. Papers,' vol. i.; and in the following year, in a paper read before the Royal Society of Edinburgh on the 19th of April, entitled, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy," the subject is brought home to the general understanding by a succession of short theses referring to the dissipation and possible limited restoration of energy ('Papers,' vol. i. p. 511, &c.)

[2] 'Math. and Phys. Papers,' vol. i. pp. 179, 511. Helmholtz ('Vorträge und Reden,' vol. i. p. 43) said in 1854: "In any case we must admire the acumen of Thomson, who could read between the letters of a mathematical equation, for some time known, which spoke only of heat, volume, and pressure of bodies, conclusions which threaten the universe, though indeed only in infinite time, with eternal death."

mechanism, they will have this property of a perfect machine, namely, that it can work backward as well as forward. It is against all reason and common-sense to carry out this idea in its integrity and completeness. "The essence of Joule's discovery is the subjection of physical phenomena to dynamical law. If, then, the motion of every particle of matter in the universe were precisely reversed at any instant, the course of nature would be simply reversed for ever after. The bursting bubble of foam at the foot of a waterfall would reunite and descend into the water; the thermal motions would reconcentrate their energy and throw the mass up the fall in drops, re-forming into a close column of ascending water. Heat which had been generated by the friction of solids and dissipated by conduction and radiation with absorption, would come again to the place of contact and throw the moving body back against the force to which it had previously yielded. Boulders would recover from the mud the materials required to rebuild them into their previous jagged forms, and would become re-united to the mountain-peak from which they had formerly broken away. And also, if the materialistic hypothesis of life were true, living creatures would grow backwards with conscious knowledge of the future, but with no memory of the past, and would become again unborn. But the real phenomena of life infinitely transcend human science; and speculation regarding consequences of their imagined reversal is utterly unprofitable. Far otherwise, however, is it in respect to the reversal of the motions of matter uninfluenced by life, a very elementary consideration of which leads to the full explanation of

25. Summary statement of Thomson (Lord Kelvin).

the theory of dissipation of energy."[1] Whilst Clausius in Germany and Thomson in England were busy reconciling the truths contained in Carnot's older researches with the new conceptions firmly established by Joule's classical measurements, putting both into mathematical and into popular language, correcting our mathematical formulæ as well as our vocabulary, other applications of the new ideas assisted in procuring for them general recognition and acceptance. Rankine[2] in England, Zeuner[3] in Ger-

26. Rankine, Zeuner, and Hirn.

[1] Lord Kelvin, in a paper read before the Royal Society of Edinburgh, 2nd February 1874, on "The Kinetic Theory of the Dissipation of Energy" ('Proceedings,' vol. viii. p. 325 *sqq.*) See also his article in the 'Fortnightly Review' for March 1892, reprinted in 'Popular Lectures and Addresses,' vol. ii. p. 449 *sqq.*

[2] The earliest formal treatise on thermo-dynamics was Macquorn Rankine's article on "The Mechanical Action of Heat" in Nichol's 'Cyclopædia' for the year 1855. The part he took in the development of the new science was practical and at the same time highly speculative. His papers on temperature and elasticity of steam and other vapours, on the expansion of liquids by heat, and on the mechanical action of heat, of dates 1849 and 1850 (see 'Miscellaneous Scientific Papers,' ed. Millar, 1881, pp. 1, 16, 234), entitle him to be considered as one of the first—if not the first (see his claim to priority in a letter in Poggendorf's 'Annalen,' p. 81, 1850)—to reconcile Carnot's discovery with the mechanical view. His investigations were peculiar, combining practical applications of great value and important predictions (see Tait's memoir prefaced to Rankine's 'Papers,' p. xxix) with daring speculation; his deductions being founded on his theory of molecular vortices. Though he exerted in this country a great influence on the early workers in thermo-dynamics, his theories were scarcely relished in Germany (see Helmholtz's criticism of Rankine's methods in 1853, quoted by Helm, 'Energetik,' p. 114), where Clausius's independent and simultaneous researches on the same subject had meanwhile usurped attention. But Rankine's 'Manual of Applied Mechanics' (1857), his 'Manual of the Steam-engine and other Prime Motors' (1859), were the first books of practical application in which, through a happy nomenclature and an extensive use of graphical methods (Watt's indicator diagram and Carnot's cycle), the new ideas were introduced to a wider circle. See Helm's estimate of Rankine's work in 'Energetik,' p. 116 *sqq.*

[3] Somewhat later than Rankine in this country, Zeuner in Switzerland and Germany, following upon Clausius's theoretical memoirs, introduced the mechanical treatment of practical heat-problems. His 'Grundzüge der mechanischen Wärmetheorie' (1860) was to many a revelation. Appearing about the time when the German mechanical and chemical industries were starting upon a new development,

many, and Hirn[1] in France, studied the most important of all machines then in use, the steam-engine, in the light of the new discoveries. It became possible to define clearly what was meant by the efficiency of an engine, and to distinguish between those losses of the energy of heat or temperature which were dependent on the use of steam as the working substance, and therefore inherent and unavoidable, and those losses which depended upon the mechanism and upon the carrying out of the process employed. The older teachings contained in treatises written before a knowledge, or even an idea, of the

largely based upon the scientific training afforded in the excellent chemical laboratories and polytechnic schools of Germany, it assisted in giving to German industrial enterprise that scientific character which was at first ridiculed and has latterly been extolled in unbounded measure, and which—combined with the organising ability inherited from English ancestry—seems to be one of the distinctive features of the great industrial progress of America. First among writers on the Continent Zeuner gave such a connected exposition of the principles developed by Clausius, Thomson, and Rankine as met the requirements of practical engineers; attached to them applications referring to the steam-engine; criticised the views adopted by Watt and later writers, notably de Pambour, with reference to the behaviour of saturated vapour in the steam-cylinder during expansion and compression; and largely prepared the way for the great improvements in steam, air, and refrigerating engines which have been brought out on the Continent by those trained in his school. Through Clausius, Zeuner, and others, Dingler's 'Polytechnic Journal' became the organ by which the many discussions on the new mechanical theory, and notably the second law of thermo-dynamics, gradually forced themselves upon the attention of practical men.

[1] Equally important were the labours of Adolph Hirn (1815-90). He was a self-made man who had grown up in the midst of the important textile industry of Alsace. With a naturally inquiring disposition he combined the scientific and artistic accomplishments for the manifestation of which the chemical and mechanical products of that country have long been renowned. He approached some of the great theoretical problems connected with practical engineering, such as those of heat, steam, lubrication, and superheating, by a long series of carefully planned experiments. A very interesting account by several authors is given in a publication by Faudel and Schwoerer ('G. A. Hirn, sa Vie, sa Famille, ses Travaux,' Paris, 1893). Hirn, like Rankine, was not only an engineer, but also an artist and a philosopher.

mechanical value and the availability of heat existed, had to be largely altered, and corrected notions laid down, frequently as a result of prolonged discussion.[1] As an example, I may refer to the controversy between Hirn and Zeuner as to the cause of the great discrepancy between the theoretical and practical figures referring to the work in the steam-cylinder, the so-called "Water or Iron" controversy.[2]

But whilst it must be admitted that the corrected views regarding the nature of heat—the preservation

[1] The best account of the practical bearings of the mechanical theories of Rankine and Clausius is to be found in Prof. Unwin's "Forrest Lecture," delivered 2nd May 1895, before the Institute of Civil Engineers, and published in the 'Electrician,' vol. xxxv. p. 46 *sqq.* and p. 77 *sqq.* He there refers to the great discrepancy between the "rational" and the "experimental" theories, and to Hirn's experiments and practical results, notably with the "steam-jacket," and his introduction of "superheating" in 1855. "No doubt the rational theory altogether underrated the enormous facility of heat-exchange, which arises out of the contact between a conducting cylinder-wall and a vapour in a condition of the greatest instability, and liable to condense or evaporate on the slightest change of thermal condition" (p. 50). The several controversies through which Clausius defended and gradually elucidated the somewhat obscure statement which he gave of the so-called second law of thermo-dynamics may be studied with advantage in the 2nd edition of his collected Memoirs ('Die mechanische Wärmetheorie,' Braunschweig, vol. i., 1876), where his replies to criticisms of Holtzmann, Decher, Zeuner, Rankine, Wand, and Tait are most instructive. A good account is also given in Baynes's 'Lessons on Thermodynamics,' Oxford, 1878, p. 103 *sqq.*

[2] See Prof. Unwin, *loc. cit.*, p. 79. "On the appearance of Isherwood's researches in 1863, the discrepancy between the rational theory and the results of experiment were recognised by Rankine and others. But the conditions of the steam-cylinder condensation are so complex that for a long time the more theoretical writers practically ignored both Hirn's and Isherwood's results. Zeuner perhaps had pushed the rational theory to the furthest limit of detail, and with the greatest insight into practical conditions. But it was not till 1881 that he began to explicitly admit the largeness and importance of the condensing action of the cylinder. Zeuner then was disposed to attribute initial condensation to the presence of a permanent and not inconsiderable mass of water in the clearance space of the engine. . . . In opening a discussion with Hirn in 1881, Zeuner wrote that if the presence of water in the clearance space was conceded, the Alsatian calculations would be

and waste (degradation) of energy, have hardly resulted in those practical achievements and improvements[1] which in other departments of applied science, notably in chemistry and electricity, have followed upon new discoveries, the influence of these new conceptions on scientific thought and method themselves has been enormous. Next to the conceptions introduced by Darwin into the descriptive sciences, no scientific ideas have reacted so powerfully on general thought as the ideas of energy. A new vocabulary had to be created; the older text-books, even where they dealt with known subjects in perfectly correct ways, had to be rewritten; well-known and approved theories had to be revised and restated in correcter terms, and problems which had lain dormant for ages to be attacked by newly invented methods. I propose in the rest of this chapter

greatly shaken. . . . There thus arose a rather angry controversy which has been summed up in the question, 'Is it water or iron?' I do not know that this controversy has been as yet completely decided." See also Peabody, 'Thermo-dynamics of the Steam-Engine,' 4th ed., New York, 1900, p. 301 *sqq.*

[1] This explains how it comes about that theoretical thermo-dynamics is still regarded with suspicion, not to say aversion, by many engineers of the old school, whose knowledge is principally based upon experience derived from the steam-engine. The first theoretical treatment of the steam-engine by Rankine in England, and Zeuner in Germany, exhibited such enormous discrepancies between theory and practice; the simplifying assumptions which were introduced in order to make the behaviour of steam in the cylinder at all calculable were so far wide of the mark, — that a general consensus seems to prevail among theoretical engineers that progress depends less upon an immediate application of thermo-dynamic principles, than upon a careful analysis—guided by theory—of elaborate tests upon the various types of engines now in use. Such experiments are accordingly—following the example of Hirn—being carried out in many scientific establishments in this country, on the Continent of Europe, and notably in the United States of America, and are elaborately recorded in many modern publications. See Peabody, 'Thermo-dynamics of the Steam-Engine,' 4th ed., preface, and chaps. xiii. and xiv.; Ewing, 'The Steam-Engine,' 1894, p. 31.

to glance summarily at these revolutions in the domain of scientific thought which the physical view, by regarding nature as the playground of the transformations of energy, has brought about. What I have just indicated will suffice to bring some order into the account I propose to give. There are four distinct directions in which we have to look. *Firstly*, there is the clearer definition of the new ideas laid down in the new vocabulary of scientific and popular language during the second half of the century. *Secondly*, there is the revision and recasting of the whole body of physical and chemical knowledge in the light of the new insight which had been attained. *Thirdly*, there is the criticism of existing theories from the new points of view; and *lastly*, there are the fresh departures which these novel ideas have suggested.

27. Revolutions brought about by idea of energy.

The first definite use of the new conceptions of power and work, and of a scale of mechanical value, were contained in the writings of Poncelet and Sadi Carnot in France during the first quarter of the century. The first philosophical generalisations were given by Mohr and Mayer; the first mathematical treatment was given by Helmholtz; the first satisfactory experimental verification by Joule, during the second quarter of the century. The practical elaboration of the whole system following upon Joule's and Regnault's experiments belongs, through Thomson and Rankine in this country, and through Clausius in Germany, to the third quarter of the century. Students in our age entering on the study of mechanical, physical, chemical, and even physiological processes, reap the benefit of these labours by at once grasping the

underlying unity and correspondence of all natural phenomena, inasmuch as they all depend on the transformation of a quantity, termed energy, which is in many cases measurable in its best-known form—*i.e.*, as energy of motion—and, where this is not possible, in the form of heat.

Helmholtz had already, in 1847, summarily reviewed the whole field, beginning with a restatement of the fundamental formulæ of dynamics in the light of the new principle, and ending with a reference to the transformation of energy in living vegetable and animal organisms. The key to his explanations is to be found in the introduction of a term to denote what becomes of energy if it ceases to exist as energy of motion or as a velocity, when it is changed to energy of mere position. To this end he introduces the idea of stress or tension.

28. Helmholtz on "tension."

The conception is already contained in older books on mechanics as latent force (Carnot),[1] and the purely mathematical treatment of dynamics by Lagrange and Hamilton had prepared the ground by showing how all dynamical problems could be reduced to the knowledge of two quantities, the *vis viva* and the force function.

[1] L. N. M. Carnot (1753-1823), usually termed the great Carnot, father of Sadi Carnot, member of the Directory, War Minister, and one of the most celebrated generals of France, has a name in science through his 'Essai sur les Machines en général' (Dijon, 1784), his 'Principes fondamentaux de l'Équilibre et du Mouvement' (Paris, 1803), as well as through his 'Réflexions sur la Métaphysique du Calcul infinitésimal' (Paris, 1797) and his 'Théorie des Transversales' (Paris, 1806), by which he became, together with Monge, one of the founders of modern geometry, of which more in a subsequent chapter. He introduced the principle of the 'Corrélation des Figures de Géométrie' (Paris, 1801). His books were translated in Germany, where they had a great influence. On his connection with the history of the conception of energy, see Bohn in 'Phil. Mag.,' iv. 300, vol. xxix.; also Helm, 'Energetik,' p. 13; and the Éloge by Arago of the year 1837.

The exposition of Helmholtz, however, does not seem to have been understood or accepted. The general recognition of the relation of active and latent forces dates rather from Thomson's and Rankine's writings in 1851 and the following years. Thomson uses the term "mechanical energy" (later, from 1851, intrinsic energy, or simply energy), and considers this quantity to be a measure of the store of power to do work which a material system possesses;[1] and Rankine,[2] early in 1853, introduces and defines the terms actual (or sensible) energy and potential (or latent) energy, which are at once adopted by Thomson[3] in the place of the terms dynamical and statical energy, which he

29. "Potential" and "actual" energy.

[1] The memoir of Thomson in which he introduces the physical conception of the quantity "energy" in the place of a merely mathematical symbol used by Clausius, and inaugurates the terminology of modern physics, is contained in the 'Transactions of the Royal Society of Edinburgh,' vol. xx., Part 3 (read December 15, 1851, and reprinted in 'Math. and Phys. Papers,' vol. i. p. 222), as an appendix to the great paper "On the Dynamical Theory of Heat, with Numerical Results deduced from Mr Joule's Equivalent of a Thermal Unit, and M. Regnault's Observations on Steam" (Trans. Edinb. Soc., March 1851: reprinted in 'Phil. Mag.,' 1852, and 'Math. and Phys. Papers,' vol. i. p. 174 *sqq.*; see especially p. 186, note). The term energy had indeed been used by Thomson already in 1849 as a synonym for mechanical effect, but he had not then accepted the dynamical theory. He merely puts the question in a footnote to his exposition of Carnot's theory: "When thermal agency is . . . spent, what becomes of the mechanical effect which it might produce? Nothing can be lost in the operations of nature—no energy can be destroyed" ('Papers,' vol. i. p. 118, 1849).

[2] In a paper read before the Philosophical Society of Glasgow, January 5, 1853, reprinted in 'Miscellaneous Scientific Papers,' ed. Millar, p. 203 *sqq.* See also Rankine's note, dated 1864, in the 28th vol. of the 4th series of the 'Phil. Mag.,' p. 404.

[3] See the Proceedings of the Glasgow Philos. Soc., January 1853, reprinted with additions from Nichol's 'Cyclopædia' (1860) in 'Math. and Phys. Papers,' vol. i. p. 521. In this paper Thomson also introduces the term "electrical capacity" of a conductor. Thomson subsequently introduced the word "kinetic" in place of "actual" energy. See also Thomson's Lecture before the Royal Institution, February 29, 1856, reprinted in 'Math. and Phys. Papers,' vol. ii. p. 182, and 'Popular Lectures,' vol. ii. p. 418, especially the note to p.

had employed before. How little these ideas, which have now been introduced into elementary text-books as the very alphabet of physical knowledge, commended themselves in that age, except to a few intellects that had been occupied for many years trying to fix precise terms which should be capable of mathematical definition, and at the same time correspond to common-sense experience, is evident, *inter multa alia*, from the criticism by Sir John Herschel in 1866.[1] Here it is maintained that the use of the term "potential energy" "is unfortunate, inasmuch as it goes to substitute a

425. A very complete and careful historical account of the gradual invention and crystallisation of the vocabulary of the energy conception is given by Helm, 'Die Lehre von der Energie,' Leipzig, 1887, p. 36 *sqq*.

[1] The passage quoted appears in an article "On the Origin of Force," by Sir John Herschel, in the first volume of the 'Fortnightly Review,' 1865, p. 439. The article is well worth reading for those who wish to realise the enormous benefit which has been rendered to science by banishing the indefinite use of the word force and by introducing the term energy, restricting the use of force to the meaning attached to it by Newton. Sir John Herschel still speaks of the "conservation of force" (as did likewise Helmholtz, who, however, very early introduces the term *Arbeitskraft*, power to do work, thus removing all ambiguity). Rankine replied to Herschel's criticism in a paper read before the Glasgow Philosophical Society, 23rd January 1867 (reprinted in 'Miscell. Scient. Papers,' p. 229 *sqq*.) He there states that the quantity itself occurs as a mathematical symbol in Newton's 'Principia' (prop. 39), but till recently had received no appropriate name. He closes his remarks by the still more important reflection: "One of the chief objects of mathematical physics is to ascertain, by the help of experiment and observation, what physical quantities or functions are 'conserved.'" As such he enumerates mass, resultant momentum, resultant angular momentum, total energy, thermo-dynamic function. Whilst this physical problem was being defined by Rankine, Cayley, Sylvester, and Hermite were working at the corresponding problem in pure mathematics to decide what properties or quantities remain unaltered (*i.e.*, invariant), if an arrangement of several algebraical symbols is subjected to algebraical operations. It is the modern doctrine of "invariants." This doctrine has led to an enormous extension and simplification of the theory of mathematical forms or quantics. It is the key to all mathematical tactics, and prepares a useful instrument for the application of mathematics to physical problems. See Major MacMahon's Address to the Mathematical Section of the British Association, Glasgow, 1891.

truism for a great dynamical fact"; an admission which would mean that it brings common-sense and precise mathematical expression into close proximity and harmony, or describes a very general phenomenon completely and in the simplest way.

In order to become generally recognised as the simple alphabet of scientific language, the new ideas had to be made the foundation of the whole structure of physical and chemical knowledge, theoretical as well as experimental; the elements and axioms had to be restated so as at once to express the new view and to open out the enlarged aspect which had been prepared. The different departments of mechanics, physics, and chemistry had to be elaborated and co-ordinated according to a uniform design. Helmholtz had indeed, as early as 1847, roughly sketched the plan of the work, but occupied as he was during the twenty following years mainly with another much-neglected field, the analysis of the phenomena of sensation, he did not return to his original thesis till many years later, when he made an application of fundamental importance.

Meanwhile the important task of rebuilding the edifice of the physical sciences, and establishing on a large scale that which I term the physical view of nature, fell almost exclusively into the hands of what we may call the Scotch school of natural philosophy—James and William Thomson, Macquorn Rankine, James Clerk Maxwell, P. G. Tait, and Balfour Stewart, in this country; whilst Clausius abroad worked almost alone. Rankine and James Thomson very early (1855) conceived the idea of a general science called "Energetics" or "the

30. The Scotch school.

abstract theory of physical phenomena in general."[1] It is only in our day, after the lapse of a quarter of a century, that these ideas have been taken up by others, and that the plan begins to be realised. The reasons why at the time it was abandoned were manifold.

To begin with, it was soon found, notably by Joule, Helmholtz, and William Thomson, that the new principle of the conservation of energy, if applied to various other phenomena outside of the narrower field of thermotics, led to a co-ordination and comprehension of them which was then quite unexpected: opening out new aspects, disclosing unknown properties, and suggesting innumerable experiments. As instances I may refer to the thermo-elastic and thermo-electric phenomena of bodies, which very early occupied the attention of the founders of the theory of energy. The discharge of the Leyden-jar, the generation of electric currents in the voltaic cell, the heat of electrolysis, the actions of permanent magnets and those between

[1] In a paper read before the Philosophical Society of Glasgow, May 1855, entitled "Outlines of the Science of Energetics," and reprinted in 'Miscellaneous Papers,' ed. Millar, p. 209 *sqq.* See for the above definition p. 228. James Thomson's contribution is to be found in a paper on "Crystallization and Liquefaction," read before the Royal Society, December 5, 1861, in which he establishes and gives examples of the application of "a general physico-mechanical principle or axiom," which indicates when a "substance or system will pass into the changed state." As Helm says, it is a first attempt to find a general rule for the transformation of energy ('Lehre von der Energie,' 1887, p. 63). That such a general rule can in the present state of our knowledge be established on purely energetic principles is upheld by some (Ostwald, Helm) and disputed by others (see especially Planck, 'Thermodynamik,' 1897, p. 71 *sqq.*), who state their conviction that the "energy-principle clearly does not suffice for the definition of natural processes." The whole discussion merges into a philosophical question, of which more later on.

electric currents and magnets, the phenomena of diamagnetism, Ampère's theory and Weber's basis of electric measurement, Seebeck's production of electric currents by heating in a non-homogeneous conductor, the remarkable phenomena known by the name of Peltier, the electro-dynamic properties of metals, the thermo-elastic properties of matter, were all studied in the light of the new principle, the conservation and transformation of energy. Another very important problem presented itself, viz., the introduction of the new ideas into the higher educational literature, the re-writing of the text-books of science on the basis of the principle of energy, and especially the development of the fundamental notions in mechanics in conformity with the more modern views. Here, then, it became evident that the physical view of natural phenomena, according to which they are all instances of the transformation of energy, could be considered and expounded as a further development of the laws of motion as laid down in Newton's 'Principia.' It was especially the third law of motion, in which Newton stated the equality of action and reaction, that lent itself to such an interpretation as would at once lead to the wider grasp and deeper insight into natural processes which the principle of energy afforded. Accordingly about the year 1860, when the new ideas on energy had, in the minds of the great pioneers, acquired that importance which has enabled them to become the basis of a more and more comprehensive view—the physical view—of natural phenomena, the necessity was experienced of

bringing them into harmony and continuity with the older Newtonian ideas. These had been only imperfectly transmitted by the many commentaries and text-books of the Cambridge school. The same was the case in the system of Lagrange, in which the whole of mechanics had been reduced to a mathematical expression, the physical and experimental foundations being pushed aside. The 'Principia' of Newton was again studied, and re-edited in the unabridged form, and an interpretation and amplification of the third law of Motion—so as to embrace the principle of energy—was made the key to the science of dynamics. Dynamics was not taught after but before statics. Statics was treated as a special case of the theory of motion. To make the new position still more marked, it was proposed to make the term dynamics the general term which embraces kinetics and statics as subdivisions, and to reserve the word "mechanics" for the science of machines. The change which then took place in the didactic methods can be seen by comparing the first and second editions of the well-known treatise by Tait and Steele on 'The Dynamics of a Particle.' The real compendium of the new doctrine is the treatise on Natural Philosophy by

31. Thomson and Tait.

Thomson and Tait, which has probably done more than any other book in this country to lead the mathematical studies at the foremost universities and colleges into paths more useful for physical and experimental research. The greatest exponent of the new ideas was James Clerk Maxwell, to whom is also due the merit of having applied them for the purpose of testing and

confirming the worth of the treasure which lay hidden in the experimental researches of Faraday. Next to the handbook of Thomson and Tait, no writings probably have done more—especially outside of England, on the Continent and in America—than those of Maxwell to revolutionise the teaching of natural philosophy.

32. Clerk Maxwell.

I must now revert to what I said in the last chapter regarding Maxwell's attempt to put the ideas of Faraday on the communication of electric and magnetic phenomena through space into mathematical language—*i.e.*, into measurable terms. I there related how Maxwell's earliest treatment of the subject was an attempt to construct a mechanical model of the dielectric that would be capable of exhibiting and transmitting the properties of stress—*i.e.*, of tension and pressure—which the experimental researches of Faraday had partly demonstrated and partly suggested. In the sequel, as was said, he desisted from this attempt, which has since been taken up and further elaborated by others, and resorted to a different train of reasoning. This line had been suggested by the introduction of the doctrine of energy into all physical research. As the work of scientific chemists was for a long time exclusively governed by the application of the principle of the constancy of weight or conservation of matter, so, when once the mathematical expression of the various forms of energy had been correctly established, it became possible to arrive at a multitude of relations of physical quantities merely by applying the principle of the constancy of the quantity of energy. In this way the principle of energy is a kind of regulative

principle, one which allows us to deal with the grand total or outcome—mathematically called the integral—of physical processes and changes without necessarily possessing a detailed knowledge of the minute elements or factors—mathematically called differentials—out of which they are compounded. Inasmuch as what we actually observe are always integral effects—*i.e.*, summations or aggregates of great numbers of individual and unobservable processes — this line of reasoning is not infrequently very useful, and has been in many cases applied to arrive at important conclusions. In fact, it is the analogue in science of the method according to which practical men very often succeed in carrying on extensive business transactions, of which they possess a merely external though accurate knowledge; or of the balance-sheet of an industrial undertaking which exhibits and guarantees the correct result, though only the profit and loss account and the ledgers would show how this result has been arrived at.

33. Faraday.

Faraday had taught us how to look upon any given portion of space in which electric, magnetic, chemical, and thermal changes were going on as a connected system, which he termed the electro-magnetic field. He and others—notably Oersted, Ohm, Weber, Lenz, and Joule—had shown how the different occurrences in such a system could be reduced to a common measure, and how they were observably connected. Maxwell brought all these phenomena together under the term "energy of the electro-magnetic field," and set himself to study the possible forms and changes of this quantity under the law of the conservation of energy—*i.e.*, as the preservation of the sum-total of the energy. This energy could exist as motion (actual or kinetic energy), being either motion of electricity as in the current controlled by the law of Ohm, or motion of ponderable masses, such as magnets or electric conductors; or it might be dissipated energy—*i.e.*, energy apparently lost in the form of heat—controlled by the law of Joule, or, to complete the summation, it might be stored-up energy—potential energy. Faraday's researches had suggested where this store was: it was in the surrounding space, which must be considered as capable of being strained or put into a condition of stress, as elastic bodies are capable of being strained. Thomson and Tait had shortly before shown how to submit the properties of elastic systems to calculation in the most general manner, by studying the modes in which energy, actual and potential, was distributed in them, whether at rest or in motion. The way seemed then paved for Maxwell to consider with the greatest generality the properties of the electro-magnetic field, reducing them all to mechanical measures. This he did by introducing the generalised conception of a displacement or strain which exists in the field, and which is communicated as a periodic or vibratory motion with a velocity dependent on the properties or so-called constants of the medium. It is known how he succeeded in identifying very completely all the various experimentally ascertained electric and magnetic phenomena, fixing their nature and quantities in conformity with experience, and arriving finally at the suggestion that the velocity of the transmission of the electro-magnetic displacement in air must be the same as that of light, the latter being,

in fact, an electro-magnetic disturbance of very short wave length. I also mentioned above how this suggestion received a brilliant confirmation from Hertz when he succeeded in exhibiting electro-magnetic waves, which in travelling through space, though not luminous, showed all the properties peculiar to light waves, such as reflexion, refraction, polarisation, &c.

Whilst in this country, during the period from 1850 to 1870, the Scotch school of natural philosophy was thus occupied in rebuilding the whole edifice of physical science on the new basis afforded by the energy ideas, Clausius in Germany worked at the further elaboration of the dynamical theory of heat, and, as I stated above, at the kinetic theory of gases, without abandoning the astronomical view of natural phenomena, which, with its supposition of forces acting at a distance, still almost exclusively governed theoretical physics and chemistry abroad. No one did more to emphasise the difference between this and Faraday's views than Clerk Maxwell, who had welded the latter into a consistent scheme by means of the conception of energy. About the year 1870 Helmholtz again appeared as a leader of scientific thought in this domain, and placed himself at the head of a movement which by degrees almost completely swept away the older ideas. It was by him or at his suggestion that many of the more modern English works of science were translated[1] and intro-

[1] Notably Thomson and Tait's 'Natural Philosophy,' and several of Tyndall's well-known more popular works on 'Sound,' 'Heat,' and 'Fragments of Science.' Helmholtz was also one of the first natural philosophers of eminent rank abroad who broke with the older habit of exclusiveness which clung to academic teachers in Germany, and who followed the English example set by the "Addresses" of

duced in Germany, and that especially the ideas of Faraday and Maxwell were popularised, expounded, and submitted to elaborate tests. These culminated in the brilliant discoveries of Hertz already referred to.

34. Helmholtz on electro-dynamics.

As in his earlier researches into the connection of the phenomena of heat and mechanical work, so in these later ones concerning the electro-dynamic laws, Helmholtz seems to have approached his subject primarily in the interest of physiological[1] science. At that time

the British Association and the still older "Lectures" of the Royal Institution. Before his time there were only rare instances—notably those of Bessel, Liebig, and Humboldt—where scientific thinkers of the first rank condescended to influence general opinion and polite literature by stepping down from the university chair into the arena of a popular audience. No other German scientific thinker has left a collection equal to Helmholtz's 'Vorträge und Reden,' not even Bessel, whose 'Populäre Vorlesungen über wissenschaftliche Gegenstände' (ed. Schumacher, Hamburg, 1848) are too little known. Du Bois-Reymond's 'Reden' are a mine of information on the history of science, and von Baer's 'Reden' (Braunschweig, 1886) contain some excellent and original discourses.

[1] Emil du Bois-Reymond, in many passages of his remarkable addresses, and latterly in his appreciative Éloge of Helmholtz (Leipzig, 1897), has preserved the historical data for a genetic history of Helmholtz's electrical researches, which, beginning in 1851, and culminating in Hertz's brilliant experiments on the "rays of electric energy" in 1888, completely changed the aspect of electrical science in Germany and to a less degree in France. The older view, based upon a mathematical development of the fundamental conception of Ampère and mainly associated with the brilliant name of Wilhelm Weber, whose very extensive and accurate measurements largely supplied the material for the modern theory, is practically unknown to electricians in this country. No English text-book contains even a reference to a view which was once dominant abroad, and which for this reason forms a very interesting episode in the history of thought. In the fourth chapter I have referred to this view as, beside the theory of Boscovich, presenting one of the most remarkable applications of the astronomical view of nature, which originated in this country but was mainly cultivated by the French school. I must now briefly refer to the counter-movement, which in Germany is mainly identified with the name of Helmholtz. He may be said to have left the mark of his genius on the scientific history of his country as Lord Kelvin has done on that of England. His collected papers show us —and du Bois-Reymond tells us—how Helmholtz's interest in electrical problems was connected with the remarkable phenomena of animal electricity, to the exploration of which the former devoted his

there existed three different theories which aimed at finding a general formula or law that should embrace all known electro-dynamic phenomena. The two earlier ones were propounded independently and about the same

life. Du Bois-Reymond was a pupil of Johannes Müller. One of the merits of Müller's school was to have made the discoveries of physics useful for physiology and medicine as the school of Liebig made those of chemistry. Helmholtz was trained in the school of Müller, but he also came largely under the influence of Franz Neumann of Königsberg, the great teacher of mathematical physics, and of Gauss and Weber, the originators in Germany of the system of absolute measurements. It is known that the interest in electrical phenomena received a great impetus through Galvani's and Volta's discoveries. But as du Bois-Reymond ('Reden,' vol. ii. p. 389) tells us, the galvanic pile constructed by Volta withdrew attention from the phenomena of animal electricity to the much more powerful actions of artificial arrangements of metals and solutions. The study of animal electricity was for a time continued only by Italian professors, and beyond the seas by Alexander von Humboldt in his observations on the torpedo; and had to wait till the school of Müller, and notably du Bois-Reymond, approached the subject methodically with the methods and ideas of modern science. This was in the fifth decade of the century. Modern science in Germany had, however, studied the properties of the galvanic current exhaustively only in linear (one dimensional) and in closed circuits or conductors. The phenomena of nervous and muscular electric currents demanded the study of sudden and repeated electrical impulses, and of the behaviour of currents in two and three dimensional conductors, and in unclosed conductors or circuits. Incited by du Bois-Reymond, Helmholtz undertook to deduce from the formulæ of Ampère, Neumann, and Weber the action of electric currents in these modified conditions. It was then found that these formulæ gave indefinite results and required to be modified or amplified. After many years of thought and research Helmholtz arrived at a generalisation which comprehended all the different existing theories as special cases. He then—in addition to a masterly mathematical discussion—betook himself to devise special experiments to decide which of the three possible expressions of the general formula came nearest the truth. A perusal of the memoirs contained in the first volume of his 'Wissenschaftliche Abhandlungen' (pp. 429-820) shows how by gradual and strictly logical steps he convinced himself of the intrinsic correctness of Faraday's conception, which, in addition to the phenomena in linear conductors or wires, constantly took notice also of those of the surrounding medium or space — *i.e.*, of the electromagnetic field. Looking back from our present position on the development of the ideas concerning electricity in motion, we can say that Continental thinkers tried to gain a correcter and more complete understanding by a mathematical, English science by a physical, extension of the then existing notions. Helmholtz in his Faraday Lecture (1881) showed how both courses, consistently pursued, lead to the same result.

time by Franz Neumann and Wilhelm Weber; the later one was the theory of Maxwell based upon the totally different view which was maintained and gradually unfolded in the experimental researches of Faraday. The two former looked to the effects of the action of electricity at measurable distances, and has been called the telescopic view; the latter reduced these to the action which takes place in contiguous portions of matter or of space, and has been called the microscopic view. Helmholtz first of all, by an independent line of reasoning, brought the three mathematical formulæ in which these different views found expression under one common formula, of which each appears as a special case, and then proceeded by theory and experiment to decide which of the three possible special forms is to be adopted. As a theoretical test he applied the principle of the conservation of energy in a manner in which it had at that time hardly been used by Continental thinkers. His reasoning, which was largely discussed and criticised by eminent philosophers, gave to this principle the prominence and importance which it has ever since maintained in all Continental treatises. It meant the introduction of the physical view of natural phenomena.[1]

[1] In England the publication of Thomson and Tait's 'Natural Philosophy' formed, as stated above (p. 144), an epoch in the teaching of the physical sciences, notably through the prominence given to the principle of the conservation of energy. A similar epoch was created in Germany, not so much by Helmholtz's enunciation of the principle in 1847 as by the use he made of it, in one remarkable instance, in reviewing and criticising the existing and apparently conflicting theories. As Lavoisier introduced the chemical balance—based upon the conservation of matter—as a test for the correctness of chemical statements, so Helmholtz used the principle of the conservation of energy in two distinct forms, as a test of the validity of electrical

In the mean time this view had gained great support by the efforts of quite a different section of scientific workers, whose labours had opened out a new and promising field of research. The new field for a considerable period belonged almost as exclusively to foreign science as the energy-conception had for twenty years belonged to this country. Early and for the most part isolated labourers were Kopp and Hess in Germany, Regnault and Berthelot in France, Julius Thomsen in Copenhagen.[1] They (with many younger men) can be

statements. These two forms were the impossibility of a perpetual motion and the equality of action and reaction. See his Faraday Lecture, 1881. Both in the positions of Thomson and Tait and of Helmholtz the principle of energy is, however, like Lavoisier's principle, purely a regulative, not a constructive, principle of scientific research. It exerts a control and enables us to check the correctness of results. Both in chemistry and physics other principles or methods are required for extending — not merely correcting—our knowledge. Such principles are in the abstract sciences the formula of gravitation, the atomic theory, the ether; in the natural sciences the morphological and genetic theories. The whole domain of physics and chemistry has been reviewed for teaching purposes from this point of view by Hans Januschke, 'Das Princip der Erhaltung der Energie,' Leipzig, 1897. See p. 14 *sqq.*

[1] Although the history of thought has more to do with theories than with the mere discovery of facts, and with the latter mainly when, as in exceptional instances, they change the scientific aspect of phenomena, I think it important to mention specially the great merit of Victor Regnault's experimental researches. How much the progress of physical and chemical theory is indebted to his elaborate and extremely accurate measurements of many physical constants may be seen by the perusal of Lord Kelvin's early memoirs on the dynamical theory of heat. The several (so-called) laws of Boyle, Dulong, and others were subjected by Regnault to exhaustive tests; the behaviour of steam in the steam-engine formed a subject of elaborate investigation; the proof that chlorine could be substituted for hydrogen in hydrocarbons supplied a prominent support to the chemical theories of Laurent. In general Regnault's work is a model of accuracy supported by great ingenuity in the construction of apparatus and the surmounting of difficulties. Like Liebig, he was the master of many pupils who subsequently became eminent. Besides being professor of chemistry and physics in Paris, Regnault was actively connected with the celebrated porcelain works of Sèvres. Similar remarks might be made with reference to the labours of Hermann Kopp, who was for many years probably the only professor of physical chemistry in Germany.

considered as the founders of the modern science of physical chemistry, which has received an elaborate exposition in the great work of Professor Ostwald. This work is probably quite as epoch-making in the domain of chemistry as Thomson and Tait's 'Natural Philosophy' has been in that of physics.

35. Ostwald's physical chemistry.

I have already explained how in the development of chemistry the attention of its great representatives was almost entirely absorbed in gaining a knowledge of the different substances with which they had to deal, and how through preoccupation with the natural history of matter, its decomposition, analysis and synthesis, and appropriate classification, the other more scientific questions regarding the physical agencies which were at work in chemical processes—constituting the doctrine of chemical affinity—were almost completely neglected. This I traced largely to the influence of that powerful instrument of exact research, the atomic view, which had been introduced into chemical science through Lavoisier and Dalton.[1] The pursuit of physical chem-

[1] It is not an unusual experience to find that the change from one theory to another, though an advance from disproved to more correct views, is also accompanied by some loss either in definiteness or in actual knowledge of facts. The undulatory theory lost the definite notion of a rectilinear ray of light, which was only regained by prolonged and difficult analysis; the electro-magnetic theory of Maxwell has not as yet given a clear representation of those electrical charges which the older theory of Coulomb and Weber introduced in the form of stationary or moving electrical masses. Something similar happened when the older phlogiston theory was dispelled by the atomic theory, and all attention was concentrated upon change of weight. The older theory maintained that when a metal is calcined it loses something — viz., phlogiston; the new theory had proved that it gains something—*i.e.*, weight in the form of combined oxygen. More recent knowledge has shown that both theories are right. It gains weight and loses potential energy, or power to do work—*i.e.*, to combine, giving rise to molecular motion or heat. The phlogiston theory contained the correct idea that besides matter there is something else—

istry, the consideration of chemical as related to other physical forces, such as gravitation, heat, or electricity, though it very greatly occupied the pioneers of chemical science in the early years of the century,—notably Berthollet and Gay-Lussac in France, Dalton and Davy in England, Berzelius in Sweden,—fell gradually into popular disfavour, so much so that even Faraday's electrolytic law had hardly any influence on the development of chemistry.[1] This one-sided direction of chemical reasoning and observation was still further promoted by the great practical and technical results which followed from the atomic conception, the ease with which processes worked out in the laboratory could be imitated on a large scale in the factory and the workshop. It was the increased power over matter and its manifold transformations which followed immediately in the wake of atomic chemistry that gave it its interest, notably when through the study of the carbon compounds—incorrectly termed organic chemistry—new industries of undreamt-of magnitude and importance were created, and when through chemical knowledge the older methods of metallurgy were rapidly superseded. To the popular mind the result is always more interesting than the process of research or of reasoning which leads up to it; the possession of the product than the knowledge of the procedure. The

viz., energy. That the correct idea contained in the phlogistic conception was not at once given up, but only gradually lost sight of, is seen from the fact that Lavoisier's first table of elements contained 'caloric' as one of the simple bodies. See Kopp, 'Entwickelung der Chemie,' p. 209.

[1] On the causes of this see Helmholtz's Faraday Lecture ('Wissenschaftliche Abhandlungen,' vol. iii.) and Ostwald, 'Allgemeine Chemie,' 2nd ed., vol. ii. part 1, p. 530.

new substance with startling properties—be they useful or only curious and rare—has almost immediately a value, whereas the manifold transformations by which it was discovered, invented, or produced escape general notice, and are accordingly of secondary interest. This interest grows in proportion as another factor of equal commercial importance gradually and slowly asserts itself, namely, the factor of cost of production, the property through which not only the material itself, but also the labour bestowed upon it, and the most intricate transmutations and secret manipulations, gain a place and definite figure in the ledger of the accountant. Those of us who entered into practical life about the beginning of the last generation of the century know well by experience how then for the first time was being established the great system of statistics, of cost of production, which now governs every well-conducted industry and manufactory, though in general this department is still but little understood. Now, in proportion, as with progressing civilisation we come more and more to use artificially prepared products in the place of natural ones, the cost-figures become more complex: there is not only the raw material and the labour of getting it, not only the general economy of arrangement and administration by which we save labour and avoid waste—there is the whole aggregate of changes and processes, manual, mechanical, and chemical, through which the raw material has to pass. These must all have a common measure by which they possess a figure of value in the ledger of the book-keeper, otherwise the latter could not produce a statement of cost. Watt,

36. The factor of "cost" in industry.

when supplanting manual labour on a large scale by the introduction of his perfected steam-engine, had suggested the term "horse-power" as the common measure of both; and the French mathematicians, who treated mechanics with a view to practical application, had introduced the term "work." In the general industries, however,—outside of special branches, notably marine engineering,—these measures were very crudely applied; they became unintelligible and meaningless where other agencies—notably those of chemistry and electricity—had to be employed. It is only since the terms "power" and "work" have been enlarged and the more general conception of energy introduced that it has become possible to measure the new forces or agencies in terms applicable to all alike. Practically as well as theoretically the system of measurement remained imperfect so long as the energy of chemical combination could not be measured in the same way as Watt measured the energy of heat, and as Joule and others taught us how to measure the energy of an electric current. The term "energy" has thus become as important a conception for practical as it has been long recognised to be for purely scientific purposes. If the only power we use is manual labour or steam power, there exists a crude way of measuring both by the hands employed and the weight of coal burnt; but electrical power is not so exclusively dependent on a personal or material item, and thus it can only be measured by a system in which the several items of cost are reduced to a common term. It is through the wholesale introduction of the electric current as a practical agent that the thing called

"energy" has become a commercial commodity as it had before become a scientific measure.

That chemical reactions are connected with mechanical, gravitational, optical, caloric, and electric phenomena has been known for a long time. Each of these manifestations has therefore been studied as affording a measure of the energy of chemical reactions, and these have in turn been looked upon as results of attractions, or of mass actions, or of thermal conditions, or of electrical polarities. We have thus mechanical, thermo-chemical, electro-chemical theories of affinity. Valuable discoveries and important suggestions have also been arrived at by these special researches: we have the laws of mass-action suggested by Berthollet and revived in modern times by Guldberg and Waage; the all-important electrolytic law of Faraday and the so-called third law of Berthelot in thermo-chemistry; further, the important researches of Kopp and Hess. 37. Berthelot and Ostwald.
None of these discoveries, however, seemed really to grasp the whole subject of chemical reaction, and accordingly they remained for a long time unknown, or fell, after a short life, into oblivion and disrepute. It has been one of the greatest performances of the last twenty years of the century to have approached the all-important question, "What is chemical affinity, and how is it to be measured?" in a comprehensive spirit, and to have brought it to the verge of solution. The merit of having done this belongs the more incontestably to Prof. Wilhelm Ostwald,[1] because no one

[1] Prof. Ostwald's principal work is the 'Lehrbuch der allgemeinen Chemie,' of which the first edition appeared in two volumes (Leipzig,

has taken such pains as he to gauge the value of many single and isolated steps that had been taken before him, and to combine them all through his own researches into a comprehensive doctrine. The practical importance of these labours—so long insufficiently understood—will doubtless in the near future be realised in proportion as the increasing competition of industry shall emphasise the necessity of studying the economics of production: this economy consisting not only in the absence of waste of matter, but likewise in the saving of work—*i.e.*, in the absence of waste of energy.[1]

1885-87); the second edition, of which the first volume appeared in 1891, is in progress, and will comprise three volumes. It is divided into three parts: *Stöchiometrie*, *Chemische Energie*, and *Verwandtschaftslehre*. Nothing can give a better idea of the enormous development of chemical science in the nineteenth century than a glance at those two monuments of learning and research, Beilstein's 'Organische Chemie' (Leipzig, 1893-1900, 5 vols., 3rd ed.) and Ostwald's 'Allgemeine Chemie.' They form the basis for future development, as did Leopold Gmelin's 'Handbuch der Chemie' for the greater part of the past century. The first edition of Gmelin appeared in 1817. See Kopp's 'Geschichte der Chemie' (vol. ii. p. 100). Since the publication of his great text-book, Prof. Ostwald has done enormous service to science by the foundation jointly with Prof. van't Hoff of the 'Journal für physicalische Chemie,' in 1889, and still more by the opening of the first laboratory specially designed for physical chemistry, in Leipzig, in the year 1887. But perhaps the most original and suggestive work o Ostwald is his work on the scientific foundations of Analytical Chemistry (Leipzig, 3rd ed., 1901. Transl. by G. M'Gowan).

[1] How recent is the systematic treatment and general recognition of physical, theoretical, or general chemistry can be seen from the historical sketches which had been published prior to Ostwald's great work. Kopp, in his excellent account of the development of chemistry, published in the Munich collection, and frequently referred to in the fifth chapter of this work (vol. i. pp. 382, &c.), has hardly any occasion to refer to physical chemistry up to the year 1870. This is the more remarkable, as Kopp himself was a solitary ingenious worker in this isolated province. A good account of his labours is contained in Thorpe's 'Essays in Historical Chemistry,' 1894, p. 299. A later and brilliant writer on the historical growth of chemical knowledge, Dr A. Ladenburg, in his 'Vorträge über die Entwicklungsgeschichte der Chemie' (2nd ed., Braunschweig, 1887), condenses all he has to say regarding this subject into a few pages in his last lecture. If German science is destined to distinguish herself in the wider sphere of general or physical chemistry as much as she has done in the past by the extreme and one-sided culture of organic or structural chemistry, it will be largely owing to the influence of the school of Ostwald and that of the industrial factor mentioned in the text, which nowadays emphasises as much the economical control of chemical reactions as it did formerly the discovery and preparation of new compounds. The ultimate success in the industrial preparation of artificial indigo, which was theoretically long known, is an example well worth careful attention.

The ideas through which unity and coherence have been introduced into the many different trains of reasoning which were bent upon unravelling the mysteries of chemical affinity came from an unexpected quarter—from the country which, in the early part of our century, had become, through Berzelius, the centre of a great school of chemical research. Prof. Ostwald, in his recent historical sketch of the doctrines of chemical affinity, dates the latest period from the year 1886,[1] when Svante Arrhenius published his theory 38. Arrhenius.
of the chemical solutions decomposed by the galvanic current, the so-called electrolytes. That the reader may understand what importance belongs to this latest development of physical chemistry, I must go further

[1] Prof. Ostwald had himself about the same time made an attempt in the second volume of the first edition of his great work to unite the *disjecta membra* of physical chemistry, notably of the theory of affinity, into a systematic whole. This first attempt may have contributed quite as much as the special labours of others, among whom he mentions specially Helmholtz, Van't Hoff, Duhem, Planck, and Arrhenius, to create an era in chemistry. It may also be noted that, like every other important step in chemistry, this latest theoretical phase is characterised by violent controversies. These became more pronounced as Prof. Ostwald introduced into the second edition of his work the idea of "energetics" as a general and sufficient basis for the whole of physics and chemistry; making a very emphatic protest against the older physical theories, based upon attractions, atomism, or kinetics, which he stigmatises as mechanical. On this important controversy I shall have to report at the end of the present chapter, where I shall also give the full literature of the subject. In the meantime, see also Ostwald, 'Allgemeine Chemie,' vol. ii. part 1, preface, and part 2, p. 182 *sqq*.

back in the history of the subject and draw attention to the gradual change which the nineteenth century has brought about in our ideas regarding the different states in which matter is supposed to exist, be it in motion or in rest: the solid, the liquid, and the gaseous states.

Not very long ago the impressions of common-sense, according to which a fundamental difference separates solid from liquid and liquid from aeriform bodies, permeated scientific treatises also. Rigid demarcations were maintained between hydrostatics and pneumatics, and likewise between the doctrines of bodies at rest and such as are in a state of perceptible motion. One of the most marked changes which the century has witnessed, has been the breaking down of these older landmarks of science. The state of rest—once supposed actually to exist—has had to give way to a state of concealed yet measurable motion, as in the case of the kinetic theory of gases, which explains dead pressure by the bombardment of innumerable particles darting about. The idea of dynamical equilibrium—*i.e.*, the maintenance of a state of uniform motion—has in many cases taken the place of static equilibrium or rest, as in the doctrine of the flow of heat, the theory of exchanges of radiation, and the conception that the rigidity of solids depends upon a peculiar form of whirling motion—the vortex. Similarly the intermediate or transition states which lie between the solid and fluid, the properties of viscosity and of colloidal substances, and of vapours as marking the transition between liquids and gases, have attracted more attention in pro-

portion as experimental science has taken the place of that purely mathematical treatment which obtained at the beginning of the century, notably in the Continental schools, and which thought it could exhaust the infinite variety of natural phenomena by a few easily defined properties measured by constants. The narrowness of this view has been gradually overcome by the influence of the great experimental philosophers in this country, and the independent development of chemical research abroad. Beside Faraday must be especially named Thomas Graham[1] and Thomas Andrews, whose original experiments did so much to extend and deepen our knowledge of the less obvious properties of matter. Graham carried on, between 1825 and 1850, extensive experiments on the diffusion of liquids and gases, on absorption, and on the phenomena of osmosis or gradual filtering of substances through porous partitions, showing how in liquids motion and pressure exist similar to that which is now

39. Graham and Andrews.

[1] Thomas Graham (1804-69), for many years professor at University College, London, then Master of the Mint, cultivated the unexplored regions of physics and chemistry in an original spirit and yet with very simple apparatus, some of which is still used under his name. His ingenious labours attracted the attention of Liebig, through whose influence was brought about the translation of 'The Elements of Chemistry' into German by Otto. This work in its subsequent enlarged editions has formed for sixty years, next to Gmelin's 'Handbook,' a cornerstone of chemical literature in Germany, where Graham's name is a household word. The discoveries of Graham on the movement and "miscibility" of gases led to the well-known law, "that the diffusion rate of gases is inversely as the square root of their density." From gases he advanced to the more complicated study of liquids, divided bodies into two classes, "crystalloids" and "colloids," studied the "transpiration" of gases through fine tubes, and their "osmosis" or gradual filtering through porous (and many apparently non-porous) partitions. In many directions he anticipated later discoveries and collected invaluable materials for subsequent theories. *Inter alia*, he established the existence of "alcoholates," compounds analogous to "hydrates," and maintained the metallic nature of hydrogen.

generally attributed to gases. Andrews[1] in the 'sixties carried on his important experiments on the transition of bodies from the liquid to the gaseous state, and came to the conclusion "that the gaseous and liquid states are only remote stages of the same condition of matter, and are capable of passing into one another by a process of continuous change."[2] He also referred to the "possible continuity of the liquid and solid states of matter."

Another important step by which our conceptions of the nature of the liquid condition of matter were considerably enlarged and altered—motion being introduced where a former view had seen only rest—was taken by Clausius, who, following Joule and Krönig, had about the same time given its modern form to the kinetic theory of gases. What suggested this step was the phenomenon of electrolysis. The older view looked upon the action of the electric current, which, passing through substances in a state of fusion or solution, liberated the constituents out of which they were composed, as an exertion of a force contrary to the forces of chemical affinity, by which the chemical constituents were supposed to be held together. In this case energy would have to be spent in doing work against chemical forces. It was, however, very soon found that the decomposition, or—as Sainte Claire Deville first called it[3]—the

[1] See vol. i. p. 316, note, of this History.

[2] See 'The Scientific Papers of Thomas Andrews,' with a Memoir by Tait and Crum Brown, London, 1889, p. 316.

[3] Sainte Claire Deville (1818-81) approached chemical research from the side of medicine, and after a series of original investigations, first in organic then in metallurgical chemistry, entered upon his remarkable work in thermal chemistry at the time when Clausius in Germany was being led from an entirely different point of view to the same subject. He introduced the term dissociation to denote the breaking-up of chemical compounds not so much through the presence of other chemical agencies as through altered physical conditions, such, notably, as heat, evaporation, and condensation. "Deville's observations on dissociation . . . have a very direct bearing on the kinetic theory of gases, and it is a fact of interest in the history of science that Deville did not recognise the validity of that theory. Our estimate of the ingenuity, skill, and patience shown in his experimental work, and of the genius and sound judgment which directed his theoretical conclusions, is perhaps raised when we recollect that he was neither led in the first nor biassed in the second by ideas derived from the kinetic theory, and his hostile, or at least neutral, attitude towards it gives perhaps greater value to the evidence that his work has contributed to its soundness" (A. Crum Brown, 'Ency. Brit.,' 9th ed., article "Sainte Claire Deville").

dissociation of the electrolyte, was not the consequence, but the accompanying feature or condition, of the existence of an electric current in a solution. Clausius first expressed this distinctly in 1857, and Helmholtz repeated it in 1880. The conception was thus introduced that in certain (not in all) solutions of chemical compounds dissociation might exist independently of an electric current, and that the latter, if introduced, only directed the already dissociated and wandering molecules (ions), freeing them at the same time of their electric charges.[1] This conception, though at first violently 40. Dissociation.

[1] I have already mentioned (vol. i. p. 435, note) that Clausius, when introducing his kinetic theory and distinguishing between molecules and atoms, could refer to several eminent chemists who had independently arrived at similar ideas by quite different trains of reasoning. Again, when introducing, in 1857, his theory of dissociation by solution, he could refer to similar anticipations. Williamson had said already, in 1850 (Liebig's 'Annalen,' vol. lxxvii. p. 37), at the meeting of the British Association in Edinburgh: "We are led to the conclusion that in an aggregate of molecules of every compound there exists a continual exchange of the elements contained in it. Suppose, for instance, that a vessel with hydrochloric acid were filled with a great number of molecules of the compound ClH, then the view at which we have arrived would lead us to the supposition that every atom of hydrogen does not remain in quiet juxtaposition with an atom of chlorine, with which it is combined, but that there is a continual exchange of places with other hydrogen atoms" (Clausius, 'Mechanische Wärmetheorie,' vol. ii. p. 167, Braunschweig, 1879). For an illustration of the theory of Clausius modified to meet more recent conceptions, see O. Lodge's 'Modern Views of Electricity,' 1892, p. 83, &c.

attacked by chemists, became gradually better understood and gained ground. The merit of having finally introduced into our modern notions the idea of the free mobility of the constituents of electrolytic compounds belongs to W. Hittorf and F. Kohlrausch. The name of the latter will be connected in the history of science with the phenomenon of the "migration of the ions," which he has expressed, after ten years of research (1869-79), in his well-known law. The question was put and answered, "What becomes of the energy of the electric current?" It was found that electrolytic conduction increased with dilution and temperature—two agents which would favour dissociation. The phenomena of dissociation had, moreover, been studied independently of the galvanic current. Following in the track of Graham and Andrews, a number of physicists abroad—notably van der Waals, Raoult, and Van't Hoff—had confirmed and extended the view that bodies in solution resembled gases, that the osmotic pressure of a liquid resembled ordinary gas pressure, that the law of Avogadro regarding the number of molecules in a gas could be transferred to matter in a state of solution, and that the magnitude of the osmotic pressure in a liquid could be used as a measure of the number of dissociated—wandering—molecules which are contained in a given volume of a solution, just as the pressure of a gas would increase if the number of molecules in a given space were increased through the splitting up of compounds. Apparent anomalies in the behaviour of gases approaching condensation were explained by the aggregation, and similar ones in dilute solutions by the dissociation, of molecules.

41. Hittorf and Kohlrausch.

The decisive step was taken in 1887 by Arrhenius,[1] who has the merit of having brought together the two independent courses of research and reasoning, and made them fruitful for each other. He shows[2] "that the difference between active and inert molecules consists in this, that the former are split into their ions, the latter not. Only the free ions take part in the conduction of electricity and in chemical reactions: this is the reason for the proportionality of the two (Faraday's law). The ions behave in solution like independent molecules: this is the reason of the deviation which electrolytic solutions show from the extended gaseous laws (Van't Hoff's discovery)." "What a change has come over our conceptions," exclaims Victor Meyer,[3] "if we have to accustom ourselves to see in a dilute solution of common salt, no longer the undecomposed molecules of a salt, but separate atoms of chlorine and sodium. For these revolutionary innovations we are indebted to the labours of Van't Hoff, Arrhenius, Ostwald, Planck, Pfeffer, de Vries, but, so far as experiments go, notably to the splendid researches of Raoult, which for years have been preparing the way for this mighty theoretical advance."

42. Victor Meyer on change of chemical views.

The year 1887, which brought together these two fruitful lines of reasoning and research, can also be considered as the epoch when the new science of physical chemistry was fairly launched into existence. The year

[1] In a communication to the Academy of Stockholm of 8th June and 9th November 1887.

[2] Quoted from Ostwald's 'Allgemeine Chemie,' 2nd ed., vol. ii. part 1, p. 656.

[3] See the highly interesting Address by Victor Meyer before the German "Naturforscherversammlung" at Heidelberg in 1889, entitled "Chemische Probleme der Gegenwart" (Heidelberg, 1890), p. 32.

1826 marks the revival of mathematical studies in Germany through the appearance of Crelle's journal; so the year 1887 saw the first number of Ostwald and Van't Hoff's 'Zeitschrift für physicalische Chemie.' From that period the physical properties of chemical substances, so long neglected, or only studied by isolated students, have received systematic, mathematical, and exact treatment, guaranteeing something like continuity and completeness, and leading on to the solution of the great remaining question, What is chemical affinity?

43. Ostwald's journal.

The eminent natural philosophers to whom is mainly due the foundation of this modern science, claim also to be gradually realising the idea which was suggested by the early representatives of the theory of energy—notably by Rankine and James Thomson—that of a general doctrine of energy, termed energetics; and they hold that this suggestion is only realisable by breaking with the conventional ideas which the older physical theories—the astronomical, atomistic, and kinetic views—have imposed upon our reasoning. They further hold that the gradual development of chemistry into an exact science necessarily requires the introduction of this broader view which they embrace, and that the older views—useful in their way—only suffice to comprehend certain restricted groups of natural phenomena, whereas in chemical changes, where all imaginable natural processes seem to come together, a larger and more independent theory is indispensable. It is interesting to note how very generally they trace this larger view to the long unnoticed labours of a natural philosopher in the New World, Professor Willard Gibbs of Yale.

44. Willard Gibbs.

The train of thought methodically and comprehensively followed out in Gibbs's various memoirs had its origin in the early speculations of William Thomson (Lord Kelvin) and Clausius, to which I referred above. Thomson was the first who, in adopting (after much hesitation) the mechanical view of the phenomena of heat, the doctrine of the convertibility and equivalence of the different forms of energy, recognised that, in order to describe natural phenomena correctly, this view required a qualification. The change of the different forms of energy into each other can for the most part take place only in one direction; there is a general tendency in nature towards a degradation or dissipation of energy. Energy, though not lost, becomes less useful, less available. The least available form of energy is heat; and it is in that form that in all natural changes a portion of energy becomes lost, dissipated, or hidden away. Thus we have to recognise the difference between available and unavailable, between useful and useless, energy. In the sequel Thomson showed in definite instances[1] how to calculate the available and the un-

[1] See 'Math. and Phys. Papers,' vol. i. No. LIX., 1852, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy"; and No. LXIII., 1853, "On the Restoration of Mechanical Energy from an unequally heated Space." In Tait's 'Sketch of Thermodynamics' (1868), we read (p. 100): "It is very desirable to have a word to express the *availability* for work of the heat in a given magazine, a term for that possession the waste of which is called *Dissipation*. Unfortunately the excellent word *entropy*, which Clausius has introduced in this connection, is applied by him to the negative of the idea we most naturally wish to express. It would only confuse the student if we were to endeavour to invent another term for our purpose." He then proceeds to use the term entropy in an altered sense, in which it measures the available instead of the unavailable energy, creating for some time a great confusion and some unnecessary irritation. See on this the early editions of Clerk Maxwell's excellent 'Theory of Heat,' and the footnote to p. 189, 8th ed., and Clausius 'Die

available energy: he introduced the word "motivity," the conception of a quantity of a "possession the waste of which is called dissipation." Whilst Thomson was thus putting into scientific language and calculating an important and obvious property of nature—namely this, that her processes mainly proceed in a certain definable direction—Rankine and Clausius were labouring independently at the mathematical wording, the analytical expression, of this remarkable discovery. Wherever a change in a system of various elements, factors, or quantities takes place mainly in a definite sense or direction, it is presumable that there exists a definite quantity which is always growing or always decreasing. This quantity may not be directly observable or measurable, as in mechanical motion velocity or distance is directly measurable; it may be hidden—we may have no special sense with which we can perceive it, as we possess a pressure sense, a heat sense, a sound and light sense; nevertheless, it may be indirectly discoverable, being made up (a function) of definite observable quantities and factors (such as heat, temperature, mass, volume, pressure, &c.) Now Rankine and Clausius found that in all thermal changes

mechanische Wärmetheorie,' vol. i. p. 387, and vol. ii. p. 324 *sqq.* A great deal of this confusion would have been avoided had Tait in 1868 introduced a really new term—viz., that suggested later (1876) by Thomson in a communication to the Royal Society of Edinburgh, and more fully explained in a paper in the 'Phil. Mag.,' May 1879, the term "Thermo-dynamic Motivity." We should then have two terms, inasmuch as the "consideration of the *energy* and *motivity*, as two functions of all the independent variables specifying the condition of a body completely in respect to temperature, elasticity, capillary attraction, electricity, and magnetism, leads in the simplest and most direct way to demonstrations of the theorems regarding the thermo-dynamic properties of matter" (*loc. cit.*, 'Papers,' vol. i. p. 459).

or heat processes—and this practically means in all natural processes—there is such a quantity which is always on the increase, and which thus measures in mathematical language the growing loss of available or useful energy in the world. Rankine simply called it the "thermo-dynamic function"; Clausius thought it important to give it a name which would co-ordinate it with energy, and he called it entropy:[1] energy which is turned inside, becomes hidden or locked up. Clausius thus gave a different wording of Thomson's doctrine of 45. Entropy.

[1] Clausius had already in 1854 (Pogg. 'Ann.,' vol. xciii. p. 481) arrived at the principal consequences and the final enunciation of what he termed "the second law of thermo-dynamics," a law which refers to the transformation, as the first refers to the conservation, of energy. He there arrives at similar conclusions to those put forth by Thomson two years earlier. The word entropy, however, was not introduced by him till 1865 (Pogg. 'Ann.,' vol. cxxv. p. 390), when he introduced it with the following remarks: "I have intentionally formed the word entropy as much as possible on the model of that of energy, for the two quantities which are to be designated by these two words are in their physical meaning so intimately related that a similarity in the terms seemed to me to be justified." As stated above (p. 167, note), Lord Kelvin, who worked simultaneously and independently at the same subject, laid more stress upon the direct statement, that in all transformations of energy we have to distinguish between the available and the total intrinsic energy, and introduced the terms energy and motivity as two functions of all the variables specifying the conditions of a system. In his article on Heat, contributed to the 'Ency. Brit.,' 9th ed., he gives the mathematical relation of motivity to entropy ('Papers,' vol. iii. p. 167). The term motivity has not become current in thermo-dynamical treatises, but the need has been very generally felt of reserving the word energy in a restricted sense for available energy, such energy as can be put to mechanical use. Wald, in a very interesting dissertation, 'Die Energie und ihre Entwerthung' (Leipzig, 1889), deplores (pp. 43 and 44) the fact that the word energy has not been reserved to denote useful, available energy. "Had the word energy," he says, "been introduced before the discovery of the first law of thermo-dynamics, then certainly only mechanical energy would have been termed simply energy." In the use of the word *Kraft* in some writers, such as Mayer, there seems occasionally a confusion between available and total or intrinsic energy. See Le Chatelier in 'Journal de Physique,' 1894.

the universal tendency in nature towards a dissipation of energy, by saying, "The entropy of the world is always on the increase."

For about twenty years after these conceptions had been introduced into scientific language and reasoning, mathematicians and physicists were mainly occupied in defining more clearly this hidden quantity, and in defending what was called the second law of thermodynamics against misconceptions and attacks. In 1875 Lord Rayleigh could still say,[1] "The second law of thermodynamics and the theory of dissipation founded upon it has been for some years a favourite subject with mathematical physicists, but has not hitherto received full recognition from engineers and chemists, nor from the scientific public. And yet the question under what circumstances it is possible to obtain work from heat is of the first importance. Merely to know that when work is done by means of heat, a so-called equivalent of heat disappears, is a very small part of what it concerns us to recognise."

Whilst these words correctly describe the general attitude of the scientific public towards this important discovery, two men had already made a beginning in
46. Horstmann. the direction indicated—Horstmann[2] in Germany, and

[1] 'Proceedings of the Royal Institution,' vol. vii. p. 386.

[2] Prof. Ostwald in the historical section of his 'Verwandtschaftslehre' ('Allg. Chemie,' 2nd ed., vol. ii. part 2, p. 111, &c.), Helm in 'Energetik' (p. 141, &c.), and Duhem in his 'Traité de Mécanique chimique' (1897, vol. i. p. 84, &c.) all do full justice to the long-unrecognised labours of Horstmann, which began in the year 1869 and were continued in Liebig's 'Annalen' in various communications during the early 'seventies, not without undergoing violent attacks from representatives of the older conceptions. Ever since James Thomson's celebrated prediction (see above, p. 126), physicists had recognised the importance of thermo-dynamical considerations,

Willard Gibbs[1] in America. They seem to have been the first to approach the question of chemical equilibrium, the result of the action of various conflicting chemical forces, termed affinities, from a general comprehensive point of view; recognising that the theory then commonly adopted on the Continent—the thermo-chemical theory of affinity—was incorrect or incomplete. This theory, which had been principally elaborated by Julius Thomsen in Copenhagen and by Berthelot in France, was supported by the large amount of valuable experimental research for which we are indebted to these two eminent men and their numerous followers.

whilst chemists persisted in the exclusive use of atomistic conceptions, which, as Horstmann pointed out, are of no avail in problems of that nature (see Helm, 'Energetik,' p. 143).

[1] More fundamental than the labours of Horstmann were those of Gibbs, which began with the year 1874, and were for a long time buried in the 'Transactions of the Connecticut Academy.' They were known to Maxwell, but remained generally unknown, partly owing to their abstract nature, partly to the fact that the majority of Continental chemists were not prepared to appreciate the mathematical form in which his expositions were clothed. Previous to the study of questions of chemical equilibrium, Gibbs had successfully developed an idea of James Thomson's—viz., the graphical representation of the different thermodynamic quantities in three instead of merely in two dimensions. Thomson had represented the properties of a body or system by referring them to volume, pressure, and temperature. Gibbs refers them to volume, energy, and entropy, the former quantities being always definable by the latter, but not *vice versâ*. The advantages of this representation were demonstrated to English students in Maxwell's 'Theory of Heat.' In Germany it was Prof. Ostwald who, by collecting and translating the memoirs of Gibbs, first made them accessible to students ('Thermodynamische Studien,' von Willard Gibbs, Leipzig, 1892). Subsequently both Ostwald and Helm have done much to promote an understanding of Gibbs's methods. See Ostwald, 'Allg. Chemie,' vol. ii. part 2, p. 114, &c.; Helm, 'Grundzüge der mathematischen Chemie' (Leipzig, 1894), and 'Energetik,' *passim*. Subsequently Gibbs also introduced the very general and useful term "phase" to denote the different states in which a substance can exist. This term denotes not only such differences as were formerly called in German *Aggregatzustände*, but likewise conditions of dissociation, allotropic and isomeric modifications.

It measured chemical reactions by what is termed their heat-toning, *i.e.*, by the amount of heat developed, and culminated in the celebrated third law of thermo-chemistry—viz., that such reactions take place as are accompanied by the greatest amount of energy liberated in the form of heat. Now, although this contains an adequate description of a very large number of reactions that take place at the temperatures at which we operate in our laboratories, the rule is by no means universal, and it required a great amount of ingenuity to explain away the many exceptions which presented themselves. The rule needed to be modified or amplified. The measurement of the energy of a chemical process by the heat-toning was not the only instance in which the thermal side of a phenomenon had been considered a sufficient means of measuring. In an allied department, that of electrolysis, Helmholtz had suggested, as early as 1847, that the electro-motive force of a galvanic cell may be measured by the heat-toning of the chemical processes which produce the current, and for a long time this was considered to be a correct expression of facts. In consequence, however, of some discrepancies which had presented themselves, Helmholtz himself was induced, about 1881, to examine the subject more thoroughly. He arrived at the conclusion that the heat-toning is not always a correct measure; and at the same time he introduced a more adequate and generally applicable method of measurement. In fact, he arrived at the conception of available or useful energy for processes which take place at constant temperature. To this quantity, which decides in which direction a reaction takes place (tempera-

ture remaining constant), he gave the name of free energy. He showed that in a state of equilibrium the free or available energy must be a minimum. He also showed the connection in which the available or free energy stands to the quantity introduced by Rankine and Clausius, the entropy which measures the unavailable or hidden energy. By making chemical changes depend on the increase or decrease of a definite measurable quantity a parallel was established between chemical and mechanical processes, the latter always taking place in the direction of a decrease of potential energy. Free energy has thus been appropriately termed by M. Duhem the thermo-dynamic potential. 47 Helmholtz's "free energy."

Helmholtz did not apply this fruitful view to chemical processes on any extensive scale, but his explanations have done much to establish that correcter and more comprehensive way of treating such questions which has since become general. Horstmann had indeed led up to this view, Willard Gibbs had applied it before, and Lord Rayleigh had suggested it.[1] The conception of

[1] The general use of the conception of useful or free energy must be dated from the remarkably lucid expositions of Helmholtz, though it is now recognised by all who have studied the history of this fertile conception that the physical notion of available energy goes back to Thomson (see Tait, 'Thermodynamics,' 1868, p. 100) and Maxwell ('Heat,' p. 187, 8th ed.; Duhem, 'Mécanique chimique,' vol. i. p. 92; Le Chatelier in 'Journal de Physique,' 1894, p. 291); that the mathematical formulæ were given by Massieu (quoted by Duhem, 'Le Potential Thermodynamique,' 1886, pp. v. and 11), and more definitely explained and applied to the physical phenomena of dissociation by Gibbs ('Thermodynamische Studien,' ed. Ostwald, p. 66, &c.; 'Amer. Journ. of Sciences and Arts,' 1879); and that it is especially owing to the labours of Duhem that the subject has received the attention of chemists. M. Duhem, in the introduction to the work of 1886, gives a very valuable and lucid historical exposition, and subsequently in his large work in four volumes ('Mécanique chimique,' 1897-1900) a vast number of applications. For the history of thought the import-

48. Kelvin's available energy.

available energy as distinguished from total energy had been introduced by Lord Kelvin and by Maxwell. This free energy is measured not only by the heat liberated, but depends on all the other factors, such as volume and pressure, the number of chemical substances engaged, and their physical conditions. The doctrine of energy and the conception of free energy pointed out a method of co-ordinating all these different factors and reducing them to a common measure. As Rankine, by the introduction of the term potential energy, did much to clear the ideas and guide the reasoning in dynamical science, so Helmholtz, by introducing the term free energy, did a great deal to introduce into chemical science the fruitful conceptions which had been elaborated and applied in physical research. The term free or available energy seems to describe more naturally the characteristic property of all energy which is useful for doing work, whilst the opposite term entropy—which measures the unavailable or hidden energy—refers to a quantity for which we have no immediate means of perception.[1]

ance of these somewhat abstruse expositions lies mainly in two directions: First, in the recognition of the fact that for the correct description of natural phenomena and changes the knowledge of the total energy is as little sufficient as that of the total weight or mass, but that it is necessary to introduce the conception of useful energy, of energy which is free or available for doing work; secondly, in the recognition that the course of chemical changes or reactions cannot be measured by attending to one special property, such as weight, or temperature, or entropy, but that it requires the measurement of a quantity which comprises all the different agencies in nature, this quantity being the energy of the system or substances in question and its availability. A third point, which is of more or less importance according to the general view adopted, is this, that the mathematical formulæ involved have exhibited the analogy between chemical and mechanical processes, the latter being those which were earliest and are most easily grasped by the mind.

[1] As Prof. Ostwald has remarked, it is to a great extent a matter of taste what particular form one adopts out of the many in which the

It was about this time—after experimental research had been carried on for many years by Julius Thomsen and Berthelot, after Horstmann had made a beginning of

second law of thermo-dynamics can be expressed ('Allg. Chemie,' vol. ii. part 2, p. 150). In every case it is simply a question how most conveniently to express and apply the general principle that heat cannot of itself pass from a colder to a hotter body, the principle on which Fourier built his "Théorie de la Chaleur," and which revealed itself as the rationale of the expositions of Carnot when in the middle of the century their hidden truth emerged from the criticisms of William Thomson (Lord Kelvin) and Clausius. Thus already in the different treatment of the same subject there showed itself the twofold tendency which reasoning on physical matters so frequently exhibits—viz., towards physical directness and mathematical elegance; the former leading to practical application, the latter to analytical refinement. Maxwell, in a review of Tait's 'Thermodynamics,' written in 1877 ('Scientific Papers,' vol. ii. p. 666), contrasts the methods of Clausius and Thomson, and Prof. Mach ('Wärmelehre,' 1896, p. 300) has made similar remarks. Of Thomson the former says, "that he does not even consecrate a symbol to denote the entropy, but he was the first to clearly define the intrinsic energy of a body, and to him alone are due the ideas and the definitions of the available energy and the dissipation of energy. . . . He avoids the introduction of quantities which are not capable of experimental measurement." Since these criticisms a great deal has been written to make the second law of thermo-dynamics and the conception of entropy more intelligible. The object here again has been twofold: first, to make the conceptions useful for the practical purpose of perfecting the heat engines (Rankine, Zeuner and his school) and of investigating the conditions of chemical equilibrium (Gibbs, Helmholtz, Duhem); next, to place the second law, which deals with the transformation of energy, on an equally firm foundation with the first law, which deals with the conservation of energy. There is no doubt that the principle of the conservation of energy owes a very large part of its intelligibility to the fact that for purely mechanical systems it follows from such well-known dynamical axioms as the laws of motion. When heat was conceived to have a mechanical equivalent in mechanical work, the more general principle of the conservation of energy seemed intelligible by mechanical conceptions. The second law, however, introduced a property of natural processes which is not so easily understood mechanically—viz., that they are not reversible—and this property was shown to be connected with a special physical quantity, for which we have a special sense—viz., temperature. The problem of making the second law mechanically intelligible thus coincides with the problem of giving a mechanical definition of temperature. It is not sufficient to call heat a mode (or, more correctly, the energy) of motion; we must express temperature, on the difference of which the usefulness of heat depends, in some way by motion, we must arrive at a

introducing thermo-dynamics into chemistry, after W. Gibbs had shown how to look at chemical energy as a sum of many forms of energy, and after Helmholtz had more clearly defined the useful conception of free or available energy as the measure of chemical reaction—

49. Ostwald's 'Allgemeine Chemie.'

that Prof. Ostwald at length ventured after the lapse of eighty years to unite in a comprehensive doctrine the scattered fragments of our existing knowledge regarding chemical affinity. This he did as a restorer of the forgotten labours and fame of Berthollet.[1] By the

kinetic definition of temperature. The two principal founders of thermo-dynamics, Clausius and Lord Kelvin, did not resort to kinetic conceptions when establishing the two laws which deal with the conservation and transformation of energy: Rankine, however, connected the subject with his theory of molecular vortices; and Clausius, who was one of the founders of the kinetic theory of gases, very early attempted to interpret the laws of the transference of heat by the help of that theory. So likewise did Maxwell, Helmholtz, Boltzmann, and many others. Mr Bryan, in a very valuable report on the "Researches relating to the Connection of the Second Law with Dynamical Principles," has given a critical summary of these various attempts (see Brit. Assoc. Reports, 1891, p. 85). The three peculiar forms of motion referred to in our last chapter—periodic, rotational, and rapid translational (disorderly) motion—have been used to suggest manifold means of translating thermo-dynamical processes into kinetic models, explaining, as Mr Bryan says, "the second law, about which we know something, by means of molecules about which we know much less" (p. 121). It does not seem that much more has been gained than a general presumption that a mechanical illustration is possible. To the statistical ideas elaborated mainly by Maxwell and Boltzmann I shall revert when treating generally of the statistical view of nature.

[1] Prof. Ostwald has himself, in the Inaugural Lecture which he delivered on the occasion of his accession to the chair of physical chemistry at Leipzig, 23rd November 1887, given a very lucid statement of the principles involved. He goes back to the two theories of chemical action represented at the beginning of the century by Bergmann on the one side and Berthollet on the other. In place of the conflict of chemical forces, in which the stronger obtains a complete victory (complete reactions)—the view of Bergmann—Berthollet introduces the "manifold play of forces acting to and fro, the result being that every one gets its due. The more powerful substance gets more, the weaker less. Only in cases where one of the possible compounds in consequence of its properties entirely leaves

publication of the second volume of his 'Lehrbuch der allgemeinen Chemie' a great impetus was given to physical chemistry. The large addition to our knowledge in this branch, and the consolidation and criticism of research which it brought about, and to which the second edition, now appearing, gives ample testimony, mark this publication as an epoch in modern scientific thought. To this development is attached the growth of the special view of natural phenomena which Ostwald and some other Continental thinkers embrace, and which they are inclined to place in opposition to the older views as a more comprehensive one. The older views they somewhat contemptuously term the materialistic views of nature—the views, in fact, which I have presented under the headings astronomical, atomic, and mechanical. As this most recent outcome of what I termed the physical view of nature refers to fundamental conceptions and has furnished much matter for discussion

the field of contest, either by falling down as insoluble or escaping as gas, can that complete decomposition take place which Bergmann held to be the normal result" ('Die Energie und ihre Wandlungen,' Leipzig, 1888, p. 20). That complete reactions were for a long time studied with predilection was most natural, especially as they are the most useful for practical purposes; but the study of moving chemical equilibrium, depending on what is now termed mass action and involving the question of the velocity of reactions, has in recent times again asserted itself. Ostwald dates the revival of this long-neglected branch of research from the year 1867, when "two Norwegian chemists, Guldberg and Waage, put the ideas of Berthollet into precise mathematical form and subjected the resulting equations to the test of observation and verification" (ibid., p. 21). Ostwald then shows further how Bergmann's theory was simultaneously revived in M. Berthelot's famous third law derived from thermo-chemistry. This in turn had to yield to the correcter views which date from Gibbs's studies "on the equilibria of heterogeneous substances" (see 'Thermodynamische Studien,' p. 66, 1875; also Ostwald, 'Allg. Chemie,' vol. ii. part 2, p. 163, on the reconciliation of Bergmann's and Berthollet's views; and further, Berthelot in 'Comptes Rendus,' 1894, 118).

abroad, I will try to sum up finally the principal points in it which are of importance for the history of contemporary thought.

Ever since the conception of energy as a quantity which, like matter, is preserved in all natural processes, forced itself with more or less clearness upon natural philosophers, the question has been insistent as to the number of different forms in which this quantity can manifest itself; and some of the earliest propounders of the doctrine attempted an enumeration of the different forms, mechanical energy of motion and of attraction usually heading the list. When that form of energy which we call heat was subjected to examination, and the remarkable property formerly called latent heat defined in the new terminology, the want arose of bringing about some kind of connection between our ideas of motion and those of heat, which were shown to be mutually convertible quantities in nature. Before that time sound and light had already yielded to the kinetic view, and an enormous increase of our knowledge in acoustics and optics had followed. Thus we find some of the pioneers of the physical or energy view of nature—notably Rankine and Joule in this country, Redtenbacher and subsequently Clausius abroad—engaged in translating the properties of heat into mechanical analogies.[1] It was not thought

[1] Rosenberger, in his 'Geschichte der Physik' (vol. iii. p. 550, &c.), gives a number of references to theories mostly forgotten which were published before and after the year 1850. Clausius, who keeps his mechanical theory of heat quite separate from his kinetic theory of gases (see the three volumes on 'Die mechanische Wärmetheorie,' 2nd ed., 1876, &c.), admits, nevertheless, in a paper published in 1857 (Pogg. 'Ann.,' vol. c., and 'Mechan. Wärmetheor.,' vol. iii. p. 1, &c.), that "from the beginning of his researches referring to heat he had attempted to account to himself for the internal state of motion of a hot body, and that he had arrived at a conception which he had already before his first publication (in 1850) used for various investigations and calculations." He further states that hearing through William Siemens that Joule had expressed a similar idea (Manchester Phil. Soc., 1848 and 1857), and more especially after the publication of Krönig (1856), he resolved to publish his views. It is interesting for our present purpose to see how Clausius, like Maxwell in a different domain of research, was originally guided by definite mechanical representations. It is equally noteworthy that Lord Kelvin's original researches on the subject of heat were quite free from this element, though we owe to him in other departments some of the most suggestive kinetic illustrations; and that he has quite recently offered valuable criticisms on the attempted mechanical interpretation of the second law of thermo-dynamics (see p. 112 of Bryan's Report, quoted above, p. 176, note). Also the first English treatise on thermodynamics written for didactic purposes (Tait's Sketch, 1868) contains no reference to molecular theory, and Hirn, one of the most active workers in the region of experimental proofs, kept clear of it.

essential, but it was found to be convenient—mainly for didactic purposes—to elaborate such analogies, explaining or describing the less known by that which is more familiar. Regarding the value of such attempts there have always existed two opinions. I have had occasion to refer to them when explaining the atomic theory. There were those who looked upon that theory merely as a convenient symbolism, and there were those who looked upon atoms and molecules as really existing things. The latter view has gained force and importance through the necessity of more and more elaborating the atomic hypothesis in order to represent not merely the chemical constitution of compounds, but likewise their manifold physical differences, some of which, in fact, could only be described by geometrical conceptions. I need only refer to what I said above on the kinetic theory of gases, and on the property termed chirality manifested by some chemical substances in solution, as well as on the phenomena of isomerism. In the last

chapter, while dealing specially with the kinetic view of natural phenomena, I had again occasion to refer to the opinion which has latterly crept into mechanical explanations—namely, that they are to be looked upon merely as symbolical, an opinion which did not enter the minds of the original propounders of the vibratory theory of sound and light, and which some eminent natural philosophers to-day strongly oppose. An opposite fate seems to have befallen the mechanical hypothesis in chemistry and in physics. Whilst Dalton's atoms were accepted with hesitation, the further elaboration of the atomic view has made it almost impossible to resist it as a physical reality; whereas the necessary complications introduced into Young's undulatory theory in order to make it cover electro-magnetic phenomena have given it the appearance of unnaturalness and artificiality—so much so that Maxwell himself abandoned the line of reasoning which led him originally to his fundamental formulæ, and contented himself with more general considerations derived from the conception of energy.

50. "Kinetics" and "energetics."

The conceptions which are expressive of the view dealt with in this chapter—the energy ideas—have had a similar fate. There have been those who have interpreted this view to mean that all phenomena in nature can be translated into the language of mechanics: they have accordingly been stimulated to invent all manner of kinetic contrivances by which light, heat, electricity, and chemical action can be represented. Others have interpreted the equivalence of all forms of energy to mean that kinetic energy is only one of the forms in

which this quantity can appear: they have thus exerted themselves to find such general properties as belong to all the forms in which energy presents itself to us. They look upon energy as a much more general conception than motion, and they think it a mistake to try to narrow the conception so that it can only mean the energy of attraction and repulsion (the astronomical view), that between the ultimate particles of matter (the atomic view), or the energy of various forms of motion (the kinetic view).

On the purely scientific side the mechanical view has much to say for itself, and can point to achievements which recommend it as a fruitful method of progress and research, and as even more fruitful for the purposes of instruction. It can claim to give in many instances an apparently easy account of the common-sense or obvious properties of bodies, and it gives this account in terms which lend themselves to strict definition, to measurement, calculation, and prediction of phenomena; it destroys all vagueness, and adopts, as it also stimulates, mathematical, which is the most cogent kind of reasoning. The kinetic theory of gases and the vibratory theory of light are notable examples. The ideas of energy and the remarkable properties of the lowest form of energy—*i.e.*, of heat—became gradually clearer and lost their strangeness as potential energy came to be defined as energy of position, available (or free) energy as the kinetic energy of regular or orderly, unavailable (or bound) energy as that of irregular or disorderly motion, and when the strange quantity termed entropy, which Clausius and Rankine strove in vain to bring home to

the general scientific intelligence, revealed itself as the measure of the disorder which prevails in the motion of the ultimate material elements of a system.[1] Faraday's lines of force and the whole elaborate imagery invented and afterwards discarded by Maxwell to describe the interaction of magnets, electric currents, and charged bodies, have proved to be most valuable instruments of thought—a useful scientific shorthand—in the hands of the teacher, as in those of the practical electrician. And although the illustrious propounder of the vortex-atom theory of matter seems latterly to have discouraged the use of this kinetic contrivance as not likely to lead to any great revelations regarding the ultimate constitution of matter or the nature of the imponderable,[2] the

[1] Helmholtz, in his first memoir on the thermo-dynamics of chemical processes ('Sitzungsberichte der Akademie zu Berlin,' 2nd February 1882), after having established the formulæ for the free energy in isothermal processes without reference to kinetic hypothesis, concludes his exposition with the following remarks: "We require, finally, an expression in order to be able to distinguish clearly what in theoretical mechanics is termed *vis viva* or actual energy from the work equivalents of heat, which are indeed mostly to be regarded likewise as *vis viva* of invisible molecular motion. I would suggest that the former should be called the *vis viva* of orderly motion. I call orderly all motion in which the compounds of velocity of the moving masses are differentiable functions of the space co-ordinates. Disorderly motion would then mean all motion in which the motion of each particle has no similarity to that of its neighbours. We have every reason to believe that heat-motion is of the latter kind, and one might in this sense regard entropy as the measure of disorder. For our means, which compared with molecular structure are coarse, only orderly motion can be freely converted again into other forms of mechanical work" ('Wissenschaftl. Abhandl.,' vol. ii. p. 972).

[2] "I am afraid it is not possible to explain all the properties of matter by the vortex-atom theory alone—that is to say, merely by motion of an incompressible fluid; and I have not found it helpful in respect to crystalline configurations, or electrical, chemical, or gravitational forces. . . . We may expect that the time will come when we shall understand the nature of an atom. With great regret I abandon the idea that a mere configuration of motion suffices" (Lord Kelvin, quoted by Prof. S. W. Holman in 'Matter, Energy, Force, and Work,' New York, 1898, p. 226).

foremost intellects are still busy in working this to them promising vein of reasoning.[1]

The opponents of the kinetic, mechanical, or material views of natural phenomena have always existed: in the early years of the century they described their view by the word "dynamic." At that time it was the atomic theory they principally objected to. But their criticisms, though not without use in exposing the limited nature of all mechanical explanations, failed to yield any fruits, inasmuch as they moved in vague expressions and did not lend themselves to that powerful method by which alone the conquest of nature has been effected, viz., mathematical reasoning, combined with observation.

51. Criticism of mechanical view.

The more recent critics of the mechanical interpretation of physical phenomena, among whom I will only mention Prof. Ostwald of Leipzig, Prof. G. Helm of Dresden, and Prof. Ernst Mach of Vienna,[2] are fully

[1] "With reference to the vortex-atom theory, I do not know of any phenomenon which is manifestly incapable of being explained by it; and personally I generally endeavour (often without success) to picture to myself some kind of vortex-ring mechanism to account for the phenomenon with which I am dealing. . . . I regard the vortex-atom explanation as the gaol at which to aim," &c. (Prof. J. J. Thomson, quoted ibid.)

[2] Prof. Ernst Mach is the earliest of these writers and had worked on quite independent lines before the other two names began to figure in scientific literature. His criticisms refer both to metaphysical and mechanical theories. His position is original and unique, and his writings, which are a splendid example of critical and historical analysis, have been invaluable to me. His earliest important essays date from the year 1872 ('Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit,' and 'Die Gestalten der Flüssigkeiten,' Prag). They are now generally accessible, having been collected and translated (under the title 'Scientific Lectures,' Chicago, 1895) by Prof. T. J. M'Cormack. His 'Science of Mechanics' (translated by the same author from the second German edition, London and Chicago, 1893) has, ever since its first appearance in 1883, had a great influence in Germany; and latterly also in this country, as may be seen from such works as Prof. Karl Pearson's 'Grammar of Science' (1st ed., 1892, p. 387), and notably from Prof. Love's 'Dynamics' (p. 85).

aware of the importance of mathematical presentation of their doctrine, and the two former have in fact done more than any one else to introduce mathematics into chemistry. But they maintain that their exact treatment is not arrived at by introducing hypothetical quantities such as the atomic and other theories are founded upon, but by contenting themselves with measuring such quantities as are presented directly in observation, such as energy, mass, pressure, volume, temperature, heat, electric potential, &c., without reducing them to imaginary mechanical or kinetic quantities.[1] To what extent they

A great many aspects of physical science which have been more prominently brought forward by the modern school of "Energetics" are to be found discussed in Mach's much earlier writings. To his valuable 'Principien der Wärmelehre' (Leipzig, 1896) I have frequently had occasion to refer in this chapter.

[1] In recent discussions and treatises two distinct tendencies must be distinguished. First we have the very useful effort to bring about a correlation of the different departments of physics and chemistry, including their applications in industry and in physiology, by the introduction of the conception of energy and the principles of its conservation and transformation. This dates practically from the publication of Thomson and Tait's 'Natural Philosophy.' The theoretical foundations of this undertaking have been very fully discussed, notably in Germany. I mention only the valuable series of writings of Prof. Max Planck, a list of which is contained at the end of his 'Thermodynamik' (Leipzig, 1897). They begin with his prize essay ('Das Princip der Erhaltung der Energie,' 1887) and his earlier dissertation (Munich, 1879) "On the Second Law." Out of this another endeavour has grown. The aim is to make the conception of energy the fundamental notion, and by following its physical appearance in its different forms, to arrive at certain fundamental relations expressed in equations, which are to serve as the basis for calculation, as in conventional physics the dynamical equations formed the starting-point for the various physical theories. In this more radical scheme the quantity "energy" was to play a part similar to that which the quantity "force" played in Newtonian dynamics. This method was probably suggested by the novel mode of treatment invented originally for heat-problems by Lord Kelvin and by Clausius, and most strictly adhered to by the former. The isolated character of this classical thermo-dynamics can be got over either by introducing a kinetic hypothesis on the nature of heat or by extending the method of thermo-dynamics to other physical provinces. The former was the most plausible view; it has its origin in the writings of Rankine

may succeed in doing this consistently seems at present uncertain. It has been maintained that the very elements of all physical measurement, the independence of the three dimensions in space, necessitates us to supplement the energy-conception—which by itself includes no more reference to direction than the conception of mass—by an assumption of a purely mechanical nature such as the number of degrees of freedom, and that the much-discussed correlation of all forms of energy, as it is suggested by W. Gibbs's formulæ, cannot be usefully carried farther. This correlation[1] has been

and Clausius. The latter method grew out of the gradual application of thermo-dynamics to chemical phenomena, where the mechanical treatment had turned out to be powerless. This more ambitious scheme of remodelling the whole of physics, chemistry, and mechanics on the model of the classical thermo-dynamics dates from the year 1887, when Prof. Georg Helm published his first treatise ('Die Lehre von der Energie,' Leipzig) and revived the word "energetics" invented by Rankine. Subsequently he published his application to chemistry ('Grundzüge der mathematischen Chemie,' Leipzig, 1894), very much under the influence of Willard Gibbs's studies of chemical equilibria and Duhem's elaboration of Helmholtz's conception of free energy. His last work ('Die Energetik,' Leipzig, 1898) gives a history of the gradual purification of the energy conception from mechanical admixtures, into which all earlier writers on the subject except Lord Kelvin are shown to have lapsed, and attempts a reconstruction of mechanics on "energetic" principles, defending the author's position against various criticisms which had meantime been made.

[1] The great generalisation of the science of energetics referred to in the text was first explicitly put forth by Helm in his treatise of 1887. He himself holds that he there finally brought together suggestions made in various ways by Zeuner (1866), Mach (1871), Gibbs (1875), Maxwell (1875), Von Oettingen (1885), and Popper (1884), and expressed them in the form of a general principle. The two factors into which all energy can be separated are called by various subsequent writers intensity, potential level on the one side; extensity, capacity, weight, on the other. In spite of further expositions of Helm in 1890 the subject did not attract much attention till Prof. Ostwald introduced it in a slightly modified form in the second edition of his great work on physical chemistry (1893), making it the foundation of the doctrine of affinity. He had evidently, between the first and second editions, given up the mechanical for the "energetic" treatment of the subject (see, *inter alia*, note 2, p. 114, of the 2nd edition; vol. ii. p. 12). At the meeting of the German

placed at the summit of the modern theory of energetics by Helm and Ostwald, after earlier writers, such as Zeuner and Mach, had already used it or drawn atten-

"Naturforscherversammlung," held at Vienna in 1894, a committee was appointed to report in 1895 at Lübeck on the "actual position of energetics," and the introduction of the subject was put into the hands of Dr Helm. His address and the discussion which followed have been given in extract in the published 'Verhandlungen' (vol. ii. part 1, p. 28, &c.), and since continued in 'Wiedemann's Annalen,' vols. lvii. *et seqq.* Simultaneously, however, the subject received a much more fundamental or philosophical development through Prof. Ostwald's general address at Lübeck with the somewhat polemical title "Die Ueberwindung des wissenschaftlichen Materialismus." From that moment the mechanical view of nature bore the stigma of materialism, to which the other side replied by attaching to the new or energetic view the stigma of "metaphysical" (see Planck, 'Wied. Ann.,' vol. lvii. p. 77) as being scientifically vague and useless. It cannot be said that the whole matter has yet been fully discussed or fathomed. Prof. Boltzmann, Prof. Carl Neumann, and Dr Helm have treated the questions at stake with much patience, and have made valuable approaches to a mutual understanding. The various contributions are most fully discussed in Helm's latest work, 'Die Energetik' (Leipzig, 1898). Some of those who originally assisted in introducing the energetic treatment have since refused to go the length of Helm's and Ostwald's final generalisations, though they prefer—for the purpose of the treatment of thermo-dynamical and chemical problems—the phenomenological method, admitting at the same time the usefulness of the atomic and mechanical hypotheses, though some do not look upon them as indispensable. This phenomenological view, which deals only with observable and measurable quantities, in contradistinction to the atomic and kinetic views, is largely represented by Prof. Nernst (see his 'Theoretical Chemistry,' translated by Palmer, London, 1895, p. 22), and by Prof. Planck (see his 'Thermodynamik,' Leipzig, 1897), though the latter considers it merely provisional, a stepping-stone in the direction of a mechanical view (p. v, preface). Prof. Boltzmann has summed up the position from a general point of view in his address at Munich in 1899. He there very lucidly defines the mechanical, energetic, and phenomenological positions, admitting the usefulness of all three, but also points out the fundamental difficulties into which a one-sided and exclusive development of any of them unavoidably leads us. Having himself done so much in applying atomic theories, he concludes by saying that "the numerous conquests of the atomic doctrine cannot be won by phenomenology or energetics," and maintains "that a theory which yields something that is independent and not to be got in any other way, for which, moreover, so many physical, chemical, and crystallographic facts speak, must not be combated but further developed" ('Verhandlungen der Versammlung zu München,' 1899, p. 121).

tion to it. It can be set out in the statement that wherever energy shows itself it appears as composed of two factors — the intensity and the capacity factors. These terms, borrowed from the older theories of heat and electricity, measure the quantity of energy as well as the direction in which changes of energy take place: the general law being that energy, in whatever form it may appear, tends to go from places of higher to places of lower potential or intensity.

52. The outcome.

The characteristic feature of this most recent outcome of the physical view of natural phenomena is that it takes in real earnest the suggestion at which many natural philosophers have independently arrived, that energy is a substance quite as much as matter. This granted, it seems at least reasonable to some thinkers to see how far they can get by employing the two conceptions of matter and energy alone without adopting a third something, the ether, which was introduced at a time when the idea of the conservation of energy had not yet been formulated.[1]

[1] For an indication of the further development of this point of view I must refer the reader to the chapter on Photo-chemistry in Prof. Ostwald's great work ('Allg. Chemie,' 2nd ed., vol. ii. part 1, p. 1014, &c.) "In the interest," he says, "of a conception of nature which is free from hypotheses, we must ask whether the assumption of that medium, the ether, is unavoidable. To me it does not seem to be so. If we ask for the cause of all displacements of energy in space which we can singly observe, we find that it always consists in differences of intensity. . . . The main point is that, having conceived energy to be a real thing, indeed the only real thing in the so-called outer world, there is no need to inquire for a carrier of it when we find it anywhere. This enables us to look upon radiant energy as independently existing in space. We have found in the general law of intensity—*i.e.*, in the empirical fact that energy tends to equalise forced changes of its density in space—the principle according to which transmission of energy in space necessarily takes place when there appears anywhere an excess." From this and other passages of Prof. Ostwald's writings it seems as if mass likewise was to be given

53. Recent triumphs of atomic view. But whilst the question as to the true method of physical research is still being ventilated abroad, as it has recently begun to be in this country also,[1] the mechanical conceptions of atoms and ether have quietly gained new victories. At the end of the last chapter I related how, in the hands of Maxwell and his followers, the word "electricity" gradually lost its substantial meaning, so that there remained only the conception of a state of motion or stress in the electromagnetic field, it being difficult to assign a definite sense to the term, an electric charge. That those who were brought up under the ideas of Coulomb and Weber would naturally regard this as a defect has also been noted. Still more had the substantial nature of electricity been forced upon those who studied the electrolytic action of solutions and currents, the wandering of

up as a secondary phenomenon of energy. See Boltzmann, *loc. cit.*, last note, p. 114, &c.; also, *inter alia*, Dr R. Pauli, 'Der erste und zweite Hauptsatz,' Berlin, 1896, preface.

[1] The discussions which began in Germany in the year 1895 at the meeting at Lübeck, and have, after being continued at subsequent meetings, and in the volumes of the 'Annalen der Physik und Chemie,' come to a kind of standstill by the exhaustive treatise of Helm on the one side and by Boltzmann's summing up on the other, do not seem to have attracted much attention in this country. Interest in the subject was, however, latterly aroused by two criticisms of the principles of scientific method coming from entirely different quarters. The first, which was of a purely philosophical character, was contained in Prof. James Ward's 'Gifford Lectures' (1896-98), published in two volumes with the title 'Naturalism and Agnosticism.' The other was an Address delivered by M. Poincaré at the Congress of Physicists in Paris in 1900. In consequence, the subject of the legitimacy of the various physical principles, such as action at a distance, atomism, kinetic and ether theories, the use of mechanical models, and many kindred questions, have been discussed in the Addresses of Poynting (1899), Larmor (1900), and Rücker (1901), before the British Association, with a very emphatic attestation of the usefulness and indispensableness of the atomistic theory regarding the constitution of matter, and the view that a continuous ether is the carrier of all physical actions through space.

the ions, and how, during the process, wandering atoms gave up or lost a definite something—viz., their electrical charges. It seemed impossible in this case to do without an atomic or molecular view of electricity. Accordingly, Helmholtz, in his celebrated Faraday Lecture (1881), after having traced the gradual displacement of the Weberian theory of electrical particles acting at a distance by that of Faraday, feels himself constrained to say: "I see very well that the assumption of two imponderable fluids of opposite qualities is a rather complicated and artificial machinery, and that the mathematical language of Clerk Maxwell's theory expresses the laws of the phenomena very simply and very truly; . . . but I confess I should really be at a loss to explain . . . what he considers as a quantity of electricity, and why such a quantity is constant, like that of a substance." And further on he says: "If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also . . . is divided into definite elementary portions, which behave like atoms of electricity."

Besides the phenomena of chemical decomposition, there was another very large and important class of phenomena which gradually led up to the conception of the substantial and atomic nature of electricity. This province of independent, and for a long time isolated, research was opened out by the combined genius of Plücker and Geissler. It was in the year 1857, two years before the announcement of the discovery of spectrum analysis, that Plücker, with the 54. Modern electrical researches.

aid of the now well-known vacuum tubes of Geissler,[1] of Bonn, began that long series of experiments on the discharge of electricity in rarefied gases, on the influence of magnets upon the course of the luminous rays, and on the spectra of incandescent gases, which subsequently, in the hands of Sir William Crookes [2] in this country, of Hittorf, Goldstein, Elster and Geitel, and of Giese in Germany, and of a great number of other natural phil-

[1] See the Memoir of Plücker in the 'Annalen der Physik und Chemie' (1857); "Ueber die Einwirkung des Magneten auf die elektrischen Entladungen in verdünnten Gasen" (reprinted in 'Gesammelte wissenschaftliche Abhandlungen,' vol. ii. p. 475, &c.) Before Plücker took up the investigation with improved means of exhaustion (later perfected by the well-known Sprengel pump), several French experimentalists — notably Quet, Gassiot, and Abria—had independently marked the difference of the light near the positive and negative poles, mostly in ignorance of the observations recorded by Faraday in his early "Experimental Researches," as far back as 1838, referring to the "dark discharge." Lord Kelvin, in his Presidential Address before the Royal Society (November 1893), refers to the researches of Faraday, and to a long list of contributions to the same subject contained in the Proceedings and Transactions of the Royal Society. Except those of Faraday, they are all later than Plücker's earliest papers. Lord Kelvin himself says: "Fifty years ago it became strongly impressed on my mind that the difference of quality between vitreous and resinous electricity, . . . essentially ignored as it is in the mathematical theories . . . with which I was then much occupied (and in the whole science of magnetic waves as we have it now), must be studied if we are to learn anything of the nature of electricity and its place among the properties of matter." Cf. the words of Hittorf (Pogg. 'Ann.,' vol. cxxxvi. p. 1), quoted by Rosenberger, 'Geschichte der Physik,' vol. iii. p. 778.

[2] The experiments and discoveries of Sir W. Crookes on "Radiant Matter," beginning with his paper in the 'Transactions' in December 1878, and continued in many subsequent communications, as also in his Address before the Brit. Assoc. in 1879, especially his theoretical explanations based upon conceptions taken from the kinetic theory of gases, made a great sensation and led to much discussion in this country and abroad. The term Radiant Matter was adopted from Faraday (see Rosenberger, *loc. cit.*, vol. iii. p. 779). The corpuscular theory of light was not indeed revived; but in general, after much criticism, Crookes's views have to a large extent been adopted; and if not the corpuscular theory of light, certainly that of electricity has been greatly supported by these brilliant experiments. See J. J. Thomson in the Princeton Lectures (1898), p. 189 *sqq.*, and Prof. Kaufmann's Address, delivered at the Hamburg meeting in September 1901 (translated in the 'Electrician' of November 8, 1901).

osophers, revealed a large array of strange and startling phenomena, which have latterly been brought somewhat into line and order by the researches of Prof. J. J. Thomson,[1] of Cambridge. A great many half-forgotten facts and experiments, which did not fit into the regular programme of electrical science or practice as it had been elaborated by the older doctrine of Coulomb and Weber on the one side, or by the more modern of Faraday and Maxwell on the other, were collected and shown to throw quite a new light on the processes of radiation and electrification, and on the relations of the atoms of ponderable matter to the vacuum, now looked upon as filled with a continuous substance, viz., the ether. The older views of the two electricities, brought before the eye by the celebrated figures of Lichtenberg;[2] many isolated facts connected with the electric spark and statical electricity, such as were collected by Riess seventy years ago, or demonstrated in the hydro-electric machine of Armstrong; theories, many times abandoned

[1] Impressed with the importance which attaches to the phenomena in question for a further development of the theory of electricity founded by Faraday and Maxwell, Prof. J. J. Thomson, in his 'Researches,' published in 1893 as a sequel to Maxwell's great treatise, devoted a long chapter to "The Passage of Electricity through Gases." His own celebrated contributions to this subject, after having been published in the 'Philosophical Magazine,' and brought before the Dover meeting of the British Association in 1899, are now summarised in his lectures on "The Discharge of Electricity through Gases" (1898). A very interesting earlier summary of the researches of others as well as of their own by Elster and Geitel, will be found in the 'Annalen der Physik' (1889), vol. xxxvii. p. 315 *sqq.*

[2] Whilst the differences between the discharges from the positive and negative terminals, after having for a long time been looked upon as isolated curiosities of electrical science, were being taken up and studied in connection with the subject here referred to (see J. J. Thomson, 'Researches,' p. 172 *sqq.*), Lord Armstrong, during the past ten years of his long and eventful life, carried on a series of experiments on a large scale, and with very powerful specially designed apparatus, on 'Electrical Discharge in Air and Water' (1895).

and as often revived, like that of Prout,[1] on the constitution of matter; the fanciful speculations of Zöllner, based upon the views of Wilhelm Weber,—all these scattered fragments or glimpses of knowledge promise at the end of the century to come together into a consistent theory of the nature of electricity as an atomically-constituted substance which is associated with particles of ponderable matter, or may even be the ultimate constituent of such matter itself. When a large mass of experimental facts and many lines of special reasoning gradually converge towards a common view, two things are indispensable in order to weld them into a consistent whole, viz., a new name or vocabulary and an hypothesis as to the elementary processes which will allow of a simple construction and subsequent mathematical calculation of the more complicated phenomena of actual experience. In the case before us, both

[1] See the concluding chapter of Prof. J. J. Thomson's 'Discharge of Electricity through Gases' (especially p. 197, &c.), where, after discussing Goldstein's "ether" theory and Crookes's "corpuscular" theory of the nature of the celebrated cathode rays, he, mainly on the strength of his own and Lenard's observations and calculations, inclines towards the latter theory, concluding that the carriers of the negative charges of electricity "are small compared with ordinary atoms or molecules, . . . this assumption being consistent with all we know about the behaviour of these rays." "It may," he continues, "appear at first sight a somewhat startling assumption in a state more subdivided than the ordinary atom; but a hypothesis which would involve somewhat similar assumptions —namely, that the so-called elements are compounds of some primordial element—has been put forward from time to time by various chemists. Thus Prout believed that the elements were all made up of the atoms of hydrogen, while Sir Norman Lockyer has advanced weighty arguments founded on spectroscopic considerations in favour of the composite nature of the so-called elements. With reference to Prout's hypothesis, if we are to explain the cathode rays as due to the motion of small bodies, these bodies must be very small compared with an atom of hydrogen, so that on this view the primordial element cannot be hydrogen." See also Sir W. Crookes's protyle theory referred to, vol. i. p. 402, note 2.

requisites were supplied before the close of the century. Here and abroad, the term electron, introduced by Dr Johnstone Stoney [1] about ten years ago, has been generally accepted to denote the ultimate particle of electricity, the atom of electricity—positive or negative—of Helmholtz. Mathematical theories have been worked out independently abroad by Prof. H. A. Lorentz [2] of Leyden, and in this country by Dr Joseph Larmor [3] of Cambridge.[4]

55. The term "electron."

[1] See 'British Association Report,' 1891, p. 574, "On the Cause of Double Lines in Spectra," by G. Johnstone Stoney: "The lines of the spectrum of a gas are due to some events which occur within the molecules, and which are able to affect the ether. These events may be Hertzian discharges between molecules that are differently electrified, or they may be the moving about of those irremovable electric charges, the supposition of which offers the simplest explanation of Faraday's law of electrolysis. . . . Several considerations suggest that the source of the spectral lines is to be sought not in the Hertzian discharges, but in the carrying about of the fixed electric charges, which, for convenience, may be called the electrons."

[2] Prof. Lorentz's principal writings are the two memoirs, "La Théorie électromagnétique de Maxwell et son Application aux Corps mouvants" (Leyden, 1892), and "Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern" (Leyden, 1895). His first labours, indeed, go back to the year 1880.

[3] Dr Larmor's principal publications are, "A Dynamical Theory of the Electric and Luminiferous Medium" ('Philos. Transactions,' 1894); Part ii., "Theory of Electrons," 1895; Part iii., "Relations with Material Media," 1898; and his Adams Prize Essay, "Æther and Matter, a Development of the Dynamical Relations of the Æther to Material Systems on the Basis of the Atomic Constitution of Matter" (Cambridge, 1900). Dr Larmor's several shorter papers and addresses, to which I shall refer, are very helpful as introducing one into this novel domain of science.

[4] A little later than Lorentz and Larmor, Dr Wiechert of Königsberg began (in 1896) a series of publications on the same subject, with the aim of making the Maxwellian conceptions more definite. With him, also, the problem narrows itself down to a reconciliation of the continuity of the ether with the atomic nature of ponderable matter, and of the electrical charges attached to it. His views, together with a historical analysis of the labours of his great predecessors, Coulomb, Ampère, Biot and Savart, Neumann, Faraday, Maxwell (including the formal simplifications introduced into Maxwell's scheme by O. Heaviside, Hertz, and Poynting), Von Helmholtz, and H. A. Lorentz, are very concisely set out in a memorial essay entitled 'Grund-

56. Difficulties of Maxwell's theory.

The theory of Maxwell had not only failed to give a definite meaning to the conception of a charge of electricity; it had also, in the general term "dielectric," somewhat obliterated the clear distinction between empty space and space filled with insulating matter, such as air. Empty space, *i.e.*, space devoid of matter, was supposed to be filled with some continuous substance, the ether, which was the seat or bearer of electric and magnetic actions, the electro-magnetic field. When the only clearly known property of this ether, the fact that it was the carrier of radiation or the luminiferous medium, was identified with its electro-magnetic nature—light being conceived to be an electro-magnetic disturbance—the new theory had to attack the great question of the relation and interaction of ether and matter, in which all the remaining problems of physical optics seemed centred.[1] How was the electro-magnetic theory of light,

lagen der Elektrodynamik,' published on the occasion of the unveiling at Göttingen, in 1899, of the monument erected in honour of Gauss and Wilhelm Weber. It is interesting to see how, from apparently quite independent beginnings, and in centres far removed from each other, the ideas of the atomic nature of electricity have almost simultaneously become crystallised, and have united themselves with the great experimental labours emanating from Plücker and Crookes to give rise, at the end of the century, to the modern theory of electrons.

[1] One of the most important of these problems is the question to what extent the ether takes part in the motion of ponderable matter through it. Astronomical aberration, discovered by Bradley, and easily explained by the then current projectile theory of light (see above, chap. vi. p. 10, note), has caused great difficulty to the undulatory theory, and even Sir George Stokes, whose ideas on the subject have been very generally quoted and accepted, would, in his Burnett Lectures on Light (1883), say no more than that "according to the theory of undulations . . . it is not inexplicable" (ed. of 1887, p. 25). That the electro-dynamic view of the ether should take up the problem was most natural, and the discussion of it is accordingly placed at the opening of Lorentz's memoir of 1895; the effect of the motion of the earth on optical phenomena having already been treated by him in 1887. Dr Larmor treats very fully of this subject in the first section of his

or the wave theory of electricity, to deal with the problem of ether and matter? In this combined scheme what and where were the electric charges or units? 57. What are electric charges?

On the Continent the labours of Prof. H. A. Lorentz of Leyden, and the almost simultaneous memoir of Von Helmholtz, approached this subject from the side of certain optical problems, notably the vexed question whether the luminiferous ether is stagnant, or participates in the movements of ponderable matter through it, and the phenomena of dispersion. These writings have formed the beginning of a long series of theoretical and experimental researches, which are by no means concluded. In this country we must chiefly consult the many and highly interesting writings of Dr Larmor for a fundamental discussion of the numerous problems involved. At the same time we find there a very thorough criticism, appreciation, and embodiment of the many scattered suggestions and contributions of English and Continental thinkers. Dr Larmor starts from a beginning which is peculiar to him. He finds among the older theoretical discussions of the nature of the luminiferous ether one[1] which will permit of such an 58. Dr Larmor's position.

essay "On Æther and Matter," and W. Wien has quite recently introduced it for discussion at the "Deutsche Naturforscherversammlung" (Düsseldorf, 1898, Bericht i. p. 49). On the occasion of this discussion, Prof. Lorentz said: "Ether, ponderable matter, and, we may say, electricity, are the building stones out of which we compound the material world, and if we only knew whether matter, in its motion, carries the ether with it or not, a way would have opened by which we could penetrate a little deeper into the nature of those building stones and their mutual action" (*loc. cit.*, p. 56).

[1] The historical traditions of Dr Larmor's theory seem to lie in what may be called the Dublin school of mathematical physics, with the great names of Rowan Hamilton (vector analysis), MacCullagh, and, in recent times, the much lamented G. F. Fitzgerald. "The form under which the atomic electric theory is introduced in Dr Larmor's latest essay

elaboration as admits on the one side the Maxwellian definitions of the propagation of electro-magnetic waves, and on the other the definition of electrons as permanent but movable states of twist or strain, which form the atoms of electricity, and possibly, in their aggregate, ponderable matter itself. The history of thought is mainly interested in this latest and most comprehensive "theory of the electric and luminiferous medium," because it is almost entirely based upon that great advance in physical theory which we owe to Helmholtz and Lord Kelvin, "the discovery of the types of permanent motion, which could combine and interact with each other without losing their individuality, though each of them pervaded the whole field." This has rendered possible an entirely new mode of treatment,[1] and at least made thinkable the reconciliation of the two apparently contradictory notions of modern physics, the continuity and uniformity of the all-pervading ether and the discontinuity of the embedded particles of matter and electricity. The history of thought also takes further note that these latest and yet unfinished theories revert, after the interval of thirty

originally presented itself . . . in the course of an inquiry into the competence of the æther devised by MacCullagh to serve for electrical purposes as well as optical ones" ('Æther and Matter,' p. vi.) "No attempt was made to ascertain whether MacCullagh's *plenum* could, in addition to its vibratory functions, take up such a state of permanent strain as would represent the electrostatic actions between charged conductors, or such state of motion as would represent the electro-dynamic action between currents. The first hint on this side of the matter was Fitzgerald's passing remark in 1880 ('Phil. Trans.,' "On the Electro-magnetic Theory of Light"), that MacCullagh's optical equations 'are identical with those of the electro-dynamical theory of optics developed by Maxwell'" (p. 78).

[1] See Larmor's Address to the British Association at Bradford ('Report,' p. 624).

years, to the older and apparently abandoned views contained in the writings of Wilhelm Weber, who dealt with electric particles and their actions at a distance. The chasm has been bridged over by such theories as those of Lorentz and Larmor, and the missing link supplied which prevented Gauss[1] from accepting that theory when it was first communicated to him by its author.[2]

[1] See above, p. 67, note, where Gauss's letter is quoted; also Larmor, *loc. cit.*, and 'Æther and Matter,' pp. 22, 72; 'Philos. Transactions,' vol. clxxxvi. (1895), p. 726; H. A. Lorentz, 'La Théorie électromagnétique de Maxwell,' 1892, p. 71: "On voit donc que, dans la nouvelle forme, la théorie de Maxwell se rapproche des anciennes idées. On peut même, après avoir établi les formules assez simples . . . regarder ces formules comme exprimant une loi fondamentale comparable à celles de Weber et de Clausius. Cependant, ces équations conservent toujours l'empreinte des principes de Maxwell." Further: Lorentz, 'Versuch einer Theorie,' &c. (1895), p. 8: "In general there lies in the assumptions which I make in a certain sense a return to the older electric theory. The kernel of Maxwell's views is hereby not lost, but it cannot be denied that with the assumption of ions we are not very far removed from the electrical particles with which one operated formerly." Wiechert ('Grundlagen der Electrodynamik,' p. 108) expresses himself similarly. Lastly, I may refer to Prof. Kauffmann's very interesting Address delivered at Hamburg, September 1891, translated in the 'Electrician' (November 1901, p. 95 *sqq.*). So we may perhaps say that as Larmor attaches himself to the traditions of the Dublin school, Lorentz and other continental representatives of the atomic view attach themselves to the school of Gauss and Weber. In proof that Weber's ideas never died out in the Göttingen school, see Rieck's Eloge of Weber, Göttingen, 1897, p. 27, and a very significant remark in the verdict of the philosophical faculty on Planck's Prize Essay ('Die Erhaltung der Energie,' 1887, p. 10).

[2] It would be unjust to dismiss this subject, the overwhelming importance of which becomes evident if we glance at the many contributions which fill the third volume of the 'Rapports présentés au Congrès International de Physique' (Paris, 1900), without stating that the atomic theory of electricity not only furnishes the very keystone which Gauss was looking for seventy years ago, but that it has also stood the test of experimental verification in the observation by Zeemann of the effect of magnetism on the rays of light, an effect which Faraday sought for in vain about the time when Gauss was in search of the keystone of electrodynamics. A very concise and interesting account of Zeemann's phenomenon will be found in M. A. Cotton's monograph "Le Phénomène de Zeemann" ("Scientia," Phys. Mathem., Paris, 1899): "Comment M. Zeemann a-t-il eu l'idée d'étudier avec un appareil de polarisation la lumière émise

59. Objections raised by atomists.

The propounders of this atomic view of electricity very naturally look with little favour on those other theories which, under the name of energetics or phenomenology, would restrict the method of science to the use of only such quantities and data as can be actually seen and directly measured, and which condemn the introduction of such useful conceptions as the atom, the electron, and the ether, which cannot be directly seen and can only be measured by indirect processes; and there is no doubt that the century ends with a very emphatic assertion of the rights and the legitimacy of the atomic and mechanical views of nature, regarding the energy principle as a regulative but not, by itself, a constructive method of research and progress; for, as Dr Larmor says, "If a molecular constitution of matter is fundamental, energy cannot also be so."[1] Nevertheless though in many ways opposed, the two views of nature meet at least in one important point. Both theories have been

dans le champ magnétique? Ici encore, la théorie vint aider l'expérience; cette fois, c'est à H. A. Lorentz que l'on est redevable du résultat obtenu. Il est juste de dire que d'autres considérations, par exemple celle de Lord Kelvin" (see Tait, Proc. Royal Soc., Edinburgh, 1875-76, p. 118) "auraient pu, elles aussi, probablement conduire à cette découverte de la polarisation des raies. Mais en fait, cette découverte a été faite grâce à l'intervention de la théorie des 'ions' de H. A. Lorentz. Dans cette théorie, dit M. Zeemann, on admet qu'il existe dans tous les corps de petites masses électrisées, ou 'ions,' dont les mouvements constituent tous les phénomènes électriques; les vibrations lumineuses seraient des vibrations de ces ions. L'état de l'éther est déterminé entièrement par la charge, la position et le mouvement de ces ions. . . . M. Lorentz fit remarquer que les bords des raies élargies devaient être polarisés. L'expérience permit à Zeemann de vérifier cette conclusion de Lorentz" (p. 37).

[1] 'Æther and Matter,' p. 286: "One effect of admitting a molecular synthesis of dynamical principles . . . is to depose the conception of energy from the fundamental or absolute status that is sometimes assigned to it. . . . We can know nothing about the aggregate or total energy of the molecules of a material system, except that its numerical value is diminished in a definite manner when the system does mechanical work or loses heat. The definite amount of energy that plays so prominent a part in mechanical

forced to consider anew the ultimate principles of all physical reasoning, notably the scope and validity of the Newtonian laws of motion and of the conceptions of force and action, of absolute and relative motion, as defined or implied in the mechanical scheme which is based upon them. Also with their increasing complexity[1] modern dynamical explanations have undoubtedly, to every impartial observer, acquired a certain character of artificiality which suggests the question to what extent all such mechanical schemes are an expression of actual truths or merely useful illustrations. For the pursuit of scientific research this question is perhaps of little importance: a method is a correct one if it leads to correct results verified by observation. Philosophically, as bearing upon the processes, powers, and limits of human reasoning, the question is all-important. We are thus led beyond the province of scientific into that of philosophic thought. In future chapters we shall frequently have occasion to note this tendency of the purely scientific thought of the century to lead up to philosophical problems. Wherever this is the case a history of scientific thought may legitimately close one of its chapters.

60. Artificial character of modern dynamical explanations.

61. The philosophic problem raised.

and physical theory is really the mechanically available energy. . . . This energy is definite, but is not, like matter itself, an entity that is conserved in unchanging amount. . . . It may and usually does diminish, in the course of gradual physical changes."

[1] The three volumes of the 'Rapports,' &c., mentioned above, have been significantly prefaced by a discourse of M. Poincaré on the relations of experimental and mathematical physics, in which he insists upon the unity and simplicity of nature as the two conditions which make generalisations possible and useful. With special reference to modern electrical theories, such as those of Lorentz and Larmor, which he had already criticised in his course on 'Electricité et Optique' (2nd ed., 1901, p. 577, &c.), he discusses the possibility of ultimate mechanical explanations. Of these, according to his view, an "infinity" is always possible. He asks what is the aim we are following—"Ce n'est pas le mécanisme, le vrai, le seul but, c'est l'unité."

CHAPTER VIII.

ON THE MORPHOLOGICAL VIEW OF NATURE..

1. The abstract sciences. THE different aspects of nature which I have reviewed in the foregoing chapters, and the various sciences which have been elaborated by their aid, comprise what may appropriately be termed the abstract study of natural objects and phenomena. Though all the methods of reasoning with which we have so far become acquainted originated primarily through observation and in the reflection over things natural, they have this in common, that they—for the purpose of examination—remove their objects out of the position and surroundings which nature has assigned to them: that they *abstract* them. This process of abstraction is either literally a process of removal from one place to another, from the great work- and store-house of nature herself, to the small workroom, the laboratory of the experimenter; or—where such removal is not possible—the process is carried on merely in the realm of contemplation: one or two special properties are noted and described, whilst a number of collateral data are for the moment disregarded. In the former case, it is by a process of actual or physical, in the latter by one

of purely mental, abstraction that our study begins and is prosecuted. One very powerful instrument of research, where through size and distance—be they very great or very small—objects of nature are beyond our actual reach, is given us in the diagram and the model. There we, for the sake of study, picture or imitate on a reduced or an enlarged scale the movements of the heavenly bodies which are too large or of the atoms which are too small for our actual grip. Now and again the natural philosopher who thus uses the abstract methods of experiment, registration, and calculation, is forcibly reminded that he is in danger of dealing not with natural, but with artificial, things. Instances are plentiful where, through the elaboration of fanciful theories, the connection with the real world has been lost and scientific reasoning has been led astray, to be recalled to a more fruitful path only by the effort of some original genius living in immediate communion with the actual world.

There is, moreover, in addition to the aspect of convenience, one very powerful inducement for scientific workers to persevere in their process of abstraction, in the study of such things and phenomena as can be handled in the laboratory and the workshop, and studied by diagram and by model. This is the practical usefulness of such researches in the arts and industries. In these we do actually abstract the possessions of nature from their proper hiding-places; we drag the minerals from the bowels of the earth; we cut up the timber of exotic growth into artificial fragments; we break up that natural equilibrium in which electrical and 2. Convenience and usefulness of the process of abstraction.

chemical agencies have, for thousands of years, evaded our discovery and our regard. Having done so, we create an artificial world of our own making which ministers to our wants, comforts, pleasures, and supplies that most inestimable of all commodities of civilisation, varied and stimulating work for ready hands and active brains. The wants and creations of artificial life have thus proved the greatest incentives to that abstract and artificial treatment of natural objects and processes for which the chemical and electrical laboratories with the calculating room of the mathematician on the one side, and the workshop and factory on the other, have in the course of the century become so renowned. All this great activity is—as I have abundantly shown—more and more governed by the scientific, the exact, or the mathematical spirit.

3. Interest opposed to the spirit of abstraction.

There is, however, in the human mind an opposite interest which fortunately counteracts to a considerable extent the one-sided working of the spirit of abstraction in science and the growing tendency towards artificiality in our practical life. This is the genuine love of nature, the consciousness that we lose all power if, to any great extent, we sever or weaken that connection which ties us to the world as it is—to things real and natural: it finds its expression in the ancient legend of the mighty giant who derived all his strength from his mother earth and collapsed if severed from her. In its extreme and purest form this interest probably lies at the root of all poetry and all art, and it accordingly governs a great part of the literature and thought of the century. It will occupy us later on in our historical

survey. At present it interests us only as far as it asserts itself also in science. In the study of natural objects we meet with a class of students who are attracted by things as they are: not so much by those which we artificially prepare in our laboratories, as by the infinite variety of real forms; not so much by the geometrical types which allow us to bring them together under some abstract formula, as by the apparent disorder and divine confusion in which real things are scattered about in the heavens and on our globe. It is not the general equation which in its complete solution contains all real and many unreal instances merely as special cases that interests them, but the individual examples themselves. The general laws of motion admit of an infinity of special cases which may never occur in nature; organic chemistry adds daily to the already enormous array of compounds which do not present themselves in living organisms. Clearly, besides the abstract sciences, which profess to introduce us to the general relations or laws which govern everything that is or can be real, there must be those sciences which study the actually existing forms as distinguished from the possible ones, the "here" and "there," the "where" and "how," of things and processes; which look upon real things not as examples of the general and universal, but as alone possessed of that mysterious something which distinguishes the real and actual from the possible and artificial. These sciences are the truly descriptive sciences, in opposition to the abstract ones. They are indeed older than the abstract sciences, and they have, in the course of the period under review in this work,

4. The descriptive sciences.

made quite as much progress as the purely abstract sciences. In a manner, though perhaps hardly as powerful in their influence on practical pursuits, they are more popular; they occupy a larger number of students; and inasmuch as they also comprise the study of man himself, they have a very profound influence on our latest opinions, interests, and beliefs—*i.e.*, on our inner life. It is the object of this and some of the following chapters to trace concisely the altered ways and means by which, in the course of the last hundred years, the study of the actual things and events of nature has been prosecuted. For those who wrote the history of the descriptive sciences in the middle of our century, the arrangement of this vast subject presented little difficulty. It had been in the main accomplished by the great naturalists who, during the seventeenth and eighteenth centuries, laboured to bring the large and ever increasing number of natural objects into some supposed system and some professed order, to enumerate them in catalogues or marshal them in museums. The familiar division of natural things into animals, vegetables, and minerals had received a general sanction. Separate sciences, with separate chairs at the universities, which still survive, attended to the separate treatment of these subjects. One of the greatest changes which the present age has witnessed has been the breaking down of the old landmarks and of the stereotyped divisions which existed in the beginning and all through the first half of the century.[1]

5. The breaking down of old landmarks.

[1] This change has also very much lessened the interest with which we now regard the solution of a problem which, down to recent times, was much discussed—the classification of the sciences. It will be seen that of the many principles of division which have been adopted, the present work only retains that one principle which, in some form or other, appears in every attempt towards classification—the difference between the abstract and the concrete or actual. The two original philosophical systems which France and England in the course of the century have produced, the positivist philosophy of Comte and the philosophy of evolution of Herbert Spencer, have both dealt elaborately with the problem of the classification of the sciences. In this they betray their descent from the philosophy of Bacon and their practical tendencies. It is mainly in the interests of teaching that the division of the sciences is of importance; and so here it has proved to be indispensable, but also, not unfrequently, narrowing and harmful. German philosophers, who have generally been more influenced by the traditions of Descartes, Spinoza, and Leibniz, have attached less importance to the rigid divisions. The result has been that in Germany, more than in any other country, those modern sciences have grown up which cultivate the borderland that separates the existing well-marked provinces which are artificially kept up by the older chairs at the universities. Examples of this are the new sciences of physiological psychology and of physical chemistry, both brilliantly and for the first time represented at the university of Leipzig. The two great conceptions, however, which have probably done more than any others to break down the old conventional landmarks that kept the sciences asunder, the conception of energy and the idea of descent, were first prominently put forward in this country. The classical treatise on the division of the sciences in the widest sense is the 'De Augmentis Scientiarum' of Lord Bacon. An important and original work on the subject is André Marie Ampère's 'Essai sur la Philosophie des Sciences, ou Exposition analytique d'une Classification naturelle de toutes les Connaissances humaines' (1834). An analysis of the book is given in Whewell's 'Philosophy of the Inductive Sciences,' vol. ii., Book 12. Ampère's classification, on the model of that in botany, is symmetrical and dichotomous. Aug. Comte's classification, contained in the second "Leçon" of the 'Cours de Philosophie positive' (1830, vol. i.), is termed by its author "une échelle", or "une hiérarchie encyclopédique." Mr Herbert Spencer, in an essay 'On the Genesis of Science' (1854), republished with additions in the third volume of his 'Essays' (1874), criticised Comte's attempt to classify the sciences "serially." He more than any other thinker has assisted in breaking down the older idea, which was very prominent in many classifications of the great French naturalists, the idea of the subordination of things in nature, of the "échelle des êtres," and the corresponding conception of an hierarchy of the sciences. In the place of this serial arrangement, a genealogical arrangement, under the specific term of evolution, was introduced, and the sciences were co-ordinated according to their

If we try to specify a little more closely the agencies and interests that were at work in bringing about this very marked change, which, like every change of the kind, has been reflected by the altered vocabulary of our languages, we come upon two distinct influences—

one of which has tended enormously to broaden our view of natural objects and events; the other to narrow it down and make it more definite, scientifically accurate, and precise. The former has tended to sweep away the older landmarks and divisions as inadequate to afford us a correct view of nature; the latter has tended to create new divisions and definitions, more in harmony with the lines on which the abstract sciences of physics and chemistry have been developed, and has thus brought the actual objects and events of nature more within the grasp of those exact and mathematical methods which those sciences have perfected. The former has been carried on in the vast workshop of nature herself by those daring and far-seeing travellers who, with Alexander von Humboldt at their head, have attempted to gain a view of nature on an extensive scale. For the sake of the increase of natural knowledge alone, they visited distant countries where the elemental forces of nature, undisturbed by the inroads of civilisation, have battled and co-operated to produce the magnificent floras and faunas of the tropics, or where, as in Siberia, the eternal cold has preserved intact the remains of bygone periods. Equipped with the instruments and methods of modern science, they recognised the necessity of studying the actual formation and stratification of rocks, the geographical distribution of organic life on the surface of the

6. The spirit of exploration.

genesis, the three great divisions being the abstract, abstract-concrete, and concrete sciences. My readers will readily see the similarities and the differences which exist between this classification and the more general distinctions which I have adopted; and I remind them again that I am not writing a history of Science but of Thought, and that all divisions of this great subject are, more or less, arbitrary.

globe, or in the depths of the ocean; of visiting the real dwelling-places, the habitat of living beings: thus counteracting and enlarging the narrow and pedantic views which the older, purely systematic, and lifeless treatment of natural objects was in danger of fostering. We know how the germs of two of the greatest generalisations of science were laid in the minds of Mayer and of Darwin during their visits to distant countries, and how fertile in natural knowledge of all kinds have been the voyage of the Challenger and many other similar expeditions, and with what interest and curiosity scientific and popular audiences listen to the narrative of such daring explorers as Fridjof Nansen.

The other and much more concentrated influence, which from the opposite side co-operated with the labours of the great explorers in remodelling the descriptive sciences and infusing new life and vigour into them, has been not less marked. There has always existed one great interest, in which nearly all the descriptive branches of natural knowledge have found a common rallying ground and a uniting purpose — namely, the art of healing, the alleviation of human suffering and the curing of disease. During long ages, when the purely scientific interest was almost dead, physical and chemical research was created or kept alive by the physician, the alchemist, and the apothecary; medical works like those of Celsus and Galenus in antiquity [1] have been the ency-

7. The medical interest.

[1] It may also be pointed out that Aristotle was descended from a family of doctors, that—according to Zeller ('Philosophie der Griechen,' vol. ii., part 2) — the assumption is warranted "that the medical art of his father Nicomachus, who was the medical adviser and friend of the Macedonian king, Amyntas, had a prominent influence on the mental development of his son."

clopedias of the existing knowledge of nature, and celebrities like Boerhaave, Linnæus, and Haller in more modern times have been the living centres of all the natural sciences. The same uniting bond has not been wanting in our century, when it has again, as many times before, manifested its powerful influence, has brought together researches which were on the point of falling asunder, and infused new life and interest into the driest of studies. As I have had occasion to remark above, the modern school of medicine originated in the attempt—begun by Lavoisier in France, but carried out on the largest scale in the chemical and physiological laboratories of Germany—of making the new discoveries in physical science and chemistry fruitful for medical purposes and the treatment of pathological cases. The discovery of galvanism gave probably the earliest impetus, and was, to the discredit of an exacter treatment, largely misused in the earlier part of the century, till Du Bois Reymond, in the middle of the period, based his elaborate researches on more correct methods, and created nearly all the knowledge we now possess of the electrical currents in the nervous system. Somewhat earlier, Liebig led the study of the phenomena of animal heat and of the food relations of the animal and vegetable kingdom; the brothers Weber had introduced dynamics into the theory of the motion of the heart and the limbs; whilst Johannes Müller and his numerous school about the same time laid the foundations of physiological and pathological acoustics and optics. Quite independently of these applications of the mechanical and physical sciences, which led some over-hastily to imagine that in the doctrine of the

8. Physical science applied to medicine.

organism as a pure machine lay an answer to the great problems of life and consciousness, Theodor Schwann proclaimed about 1840, on the basis of minute microscopic observation, the essential identity of animal and vegetable—*i.e.*, of all living—structure, thus taking probably the greatest step in uniting researches which had so far been carried on in a disconnected fashion. Here is the beginning of the modern theory of the organic cell—of cellular pathology, and the actual inauguration of modern biology. Twenty years later, the appearance of Darwin's 'Origin of Species' urged still further the study of the whole of organic life from a comprehensive point of view. In addition it led to a closer union with the sciences of inorganic nature, an appeal being now made to palæontological and geological records in proof of the gradual development of all forms of living as well as of inanimate reality. The studies of the geologist, which up to then had been prosecuted on independent lines, joined hands not only with those of the zoologist and botanist, but likewise with the theory of cosmological genesis of the planetary system, as proclaimed at the end of the former century by Laplace in his 'Exposition du Système du Monde,' and fifty years earlier by Kant in his 'Natural History of the Heavens.' If in the course of our century, through the combined influence of travel on the one side and medicine on the other, the history of natural objects has been united in the larger conception of biology, this itself at the close of the century promises to be united with geology and astro-physics (a science almost entirely founded on the invention and on the revelations of the

9. Schwann.

10. Darwin.

spectroscope), into the still wider conception of a general science of evolution, as enunciated already forty years
11. Herbert Spencer. ago in the writings of Herbert Spencer, and in a more shadowy form by Herder in the eighteenth century, and by Leibniz in the seventeenth.

Seeing, then, that the treatment of the descriptive sciences of nature has been so radically changed during the course of the century, and that the change has been accompanied by a complete revolution in our modes of thinking and reasoning on these subjects, the historian of Thought cannot be content with merely chronicling the progress of the methods in use in the separate sciences, such as mineralogy, geology, botany, and zoology, even with the addition of the more recent sciences of palæontology, physiology, and comparative anatomy. He might in doing so fairly grasp the history of the descriptive sciences up to the year 1850. It is exactly in this
12. Whewell's divisions abandoned. manner that Whewell, in his 'History of the Inductive Sciences,' treated this part of his subject. Beyond that period the old landmarks designated by those names have disappeared or become of secondary importance. On the other side, whilst a history of Evolution in Science might seize on the great characteristic feature of the more modern research which belongs to the second half of the century, it would hardly suffice to sum up the leading ideas of the descriptive branches of science as they were carried on on independent lines during the earlier years of our period. Evolution had then no definite meaning, and Biology was a disregarded term. We must thus look out for some more general aspects which belong alike to the earlier

and later periods, and which will enable us to see how that great change has gradually come about.

All studies that deal with the actual things and events by which, on a large and on a minute scale, we are surrounded in nature, are comprised under the term Natural History. In opposition to Natural Philosophy, which comprises our abstract knowledge of the possible forms of motion and the possible combinations of the elements into which we have so far been able to decompose matter, Natural History deals only with such forms and combinations as actually exist around us, only with such processes of change as actually take place in nature. Some of these forms and changes we may be able to collect in our museums or imitate in our laboratories, but the forms of nature cannot in this way be exhausted, nor her processes understood. Her forms or things do not exist in isolation, but always in a certain environment, having a definite plan, a position in time and space. These surrounding features are as important as the things themselves. Besides this, the processes of nature draw on the great factor of time with a much more liberal hand than we can permit ourselves to do. Nevertheless, as in 13. Divisions of natural history.
the abstract sciences we deal with things at rest and with things in motion, so we can appropriately divide our study of the real and the actual into the attempt to give some account of the forms and things which actually exist and continually recur, and the study of the changes which things undergo. In abstract science the terms statics and dynamics, the doctrines of rest and of motion, have been generally introduced, to distinguish the two great aims of study; some cor-

responding terms may appropriately define the twofold interest which we take in natural objects. The term morphology [1] was introduced early in the century by

[1] The term morphology was introduced by Goethe to define a series of researches and studies to which he was led by his equal interest in art, nature, and human society. Returning from Italy, which he describes as "rich in forms," to Germany, which he terms in contrast "gestaltlos," he reports that three distinct problems had presented themselves. "Wie die begünstigte griechische Nation verfahren um die höchste Kunst im eigenen Nationalkreise zu entwickeln. . . . Wie die Natur gesetzlich zu Werke gehe, um lebendiges Gebild, als Muster alles künstlichen, hervorzubringen. . . . Wie aus dem Zusammentreffen von Nothwendigkeit und Willkür, von Antrieb und Wollen, von Bewegung und Widerstand ein drittes hervorgeht . . . die menschliche Gesellschaft." For the purpose of finding an answer to the second of these questions, Goethe collected and observed, read and speculated, and formed the conception of a general science of organised beings, termed morphology, which was not to treat merely of external figure, but to comprise also physiology and the study of development. It is the first great attempt to think of nature as a whole, and to break down the rigid lines which divided the several natural sciences. He thus inaugurated the modern view of nature by introducing the general science of morphology. His first literary attempt in this direction was the now celebrated pamphlet on the 'Metamorphosis of Plants,' in which he represents the leaf as the typical formation from which the other parts of the plant can be derived. Whether this derivation is a real process in the sense of modern evolution, or a merely ideal one in the sense of the earlier archetypal view, Goethe does not clearly say. This uncertainty Goethe shares with the whole school of the "Naturphilosophie," as Julius Sachs points out in his 'History of Botany' (German edition, 1875, p. 170). This is not the point to which I want to draw attention at present. More important is the remark which Goethe makes in the further historical account of the gradual development of his morphological ideas. Wolf, the philologist, pointed out to him that his own namesake, Caspar Friedrich Wolf, had anticipated Goethe in the attempt to demonstrate the fundamental identity of the different parts of a plant. In the sequel of his most appreciative analysis of Wolf's expositions, Goethe characteristically notes that Wolf does not include in his conception the "metamorphosis of animals," or introduces it only as something entirely different. That Goethe's idea of morphology as a general science of the forms and change of forms in nature is applicable likewise to inanimate forms—to geological, geographical, and many other formations, nay, even to rigid things like crystals, and to such unstable formations as the parts of speech and language—has in the course of the century been abundantly recognised. It is known how, guided by the same general interest, Goethe studied the formations and transformations of animals, rocks, and clouds, though, according to Zittel ('Gesch, der Geologie,' 1899, p. 275), C. F. Naumann first used the expression, "morphology of the surface of the earth," in 1850. Goethe's morphological writings have been for the first time completely edited and annotated in the three volumes (6 to 8) of the second division of his works now being published by the Goethe-Gesellschaft at Weimar. The authority whom I approach nearest in the use I make of the term morphology is probably Haeckel. See the first book of his 'Generelle Morphologie der Organischen Wesen' (1866, vol. i. pp. 1-108).

one who loved above all things to watch the works of nature in their proper abodes—who combined the poetical with the scientific interest,—by Goethe. The term genesis [1] has long been employed to describe the processes by which the actual world has come to be what it is. To the statical and dynamical aspects of the abstract sciences correspond accordingly to some extent the morphological and genetic aspects of the natural sciences. To some extent only, for in nature, where everything is subject to continual flow, we never come upon a realisation of absolute rest, a pure form, a rigid type. Rather would I put it in this way: In the perpetual variety of change the morphological view tries to define those recurring forms or types which present themselves again and again, towards which all changes seem to revert; thus bringing some order into

14. Morphology and genetics.

[1] Goethe's morphological studies were equally directed towards the formation and the transformation of living things: morphology was to him the science of "Bildung und Umbildung." In the course of the century the terms morphology and morphological school have come to mean more and more that complex of comparative researches which historically prepared the genetic, developmental, or evolutionist school of thought, but which were mainly dominated by the conception of fixed types and forms, and, though searching for the laws of modification, did not rise to a clear enunciation of a theory of evolution and descent. Goethe himself hovered all his life long between an artistic predilection for the perfect form or model and a deeper philosophical conviction of the continual flow of things. See a remark of his ('Werke,' II., vol. vi. p. 304) in an aphorism on "genetic treatment": "Erst bin ich geneigt mir gewisse Stufen zu denken; weil aber die Natur keinen Sprung macht, bin ich zuletzt genöthigt mir die Folge einer ununterbrochenen Thätigkeit als ein Ganzes anzuschauen, indem ich das Einzelne aufhebe, ohne den Eindruck zu zerstören." See also a remark on Goethe's undefined position in Carus, 'Geschichte der Zoologie' (1872), p. 590.

what would otherwise be disorder and confusion. On the other side, the genetic view deals with the transition from one form to another in the course of time; takes more interest in movement and in the process and function; and seeks for their probable laws and regularities. Without wishing to limit these remarks to merely organic or living things, the difference between the morphological and genetic views can be brought home to the mind by referring to the different objects of anatomy and physiology.[1] This twofold and very general aim—the desire to know what is, and how it has come to be—has existed at all times, though frequently obscured by artificial and temporary restrictions. From this point of view I propose to survey the mental attitude of the century towards the real things and events of nature, as distinguished from the artificial or mathematical forms and processes of our studies and our laboratories, our calculating and measuring rooms. The

[1] Genetic theories have everywhere been prepared and ushered in by morphological studies. So in Goethe's time; so later on, after Darwin had given a definite law of descent, and Herbert Spencer had fixed the vocabulary and ideas of evolution, this relation is manifested by two great works, the 'Generelle Morphologie der Organischen Wesen,' by Ernst Haeckel in Germany (1866), and Francis M. Balfour's 'Elements of Embryology' (1874) in England. It is characteristic that Prof. Haeckel, in the further development of his literary activity, dropped the term morphology, and published the desired new editions of his great work under two different titles, 'Natürliche Schöpfungsgeschichte' (1868, 2 vols.), and 'Systematische Phylogenie' (1896, 3 vols.) The division of the great modern biological doctrine into morphology and genetics is in conformity with Mr Herbert Spencer's treatment in the 'Principles of Biology,' vol. ii., published in 1865, and with the two divisions of Haeckel's 'Generelle Morphologie,' which treated respectively of the "science of developed forms" and the "science of developing forms"—*i.e.*, of structure and process. I have chosen such expressions in the text as will permit of a comprehension of inanimate as well as of animated nature. In 1875 there were founded simultaneously in Germany two periodicals, representing respectively the morphological and genetic sides of animal biology.

present chapter will deal with the morphological, the following with the genetic, views of nature.[1]

Were the real world only one out of many possible worlds which the mathematical mind can imagine, though through its complication and intricacy it might still far surpass its powers of analysis; were the actual forms of nature only some of the infinitely possible states of equilibrium, the events and changes surrounding us in space and time only a few of the countless combinations of motion taught in dynamics; were the actual course of things—as mathematicians since Laplace have fancifully put it—only one particular solution of the general differential equations of the world-motion,—then the two great domains of morphology and genesis would exhaust the subject and satisfy all the interests by which natural history has been created. Unfortunately for the 15. Other aspects.
pure mathematician, but fortunately for the rest of mankind, notably the poet and the artist, it is not so. An enormous gulf separates the creations of nature from the most perfect machine; and the fact that, with all the most delicate methods at her command, her most perfect machines, like the human eye, do not come up to the demands of the optician,[2] shows us that other agencies

[1] As in abstract mechanics, the study of the conditions of equilibrium, *i.e.*, statics, preceded in time the study of the phenomena of motion, *i.e.*, dynamics, so in the study of nature the apparently finished or developed forms attracted attention before their genesis was inquired into; and as the key to statics has in the course of time been discovered to lie in dynamics, so the key to an understanding of form and structure has been found to lie in the dynamical theory of descent or evolution. In animal biology a separate influence —the medical interest—led, however, very early to a study of function and of the processes in the living organism.

[2] This refers to a well-known remark of Helmholtz in his popular lectures on the 'Theory of Light' (1868), where he enlarges on the remarkable imperfections of the eye as an optical instrument. His real

and other interests are at work than we have as yet been able to grasp. So long as astronomy was content to observe the orbits and motions of the heavenly bodies from a distance, it indeed appeared possible to define that science as merely "une question d'analyse"; but in astronomy even, spectroscopy has brought distant objects near to us and opened out endless vistas into a purely descriptive branch of the science, a natural history of the heavens. Still more so is this the case when we fix our gaze on the world immediately surrounding us—on the things and events in which we ourselves take an active part. Here two phenomena attract our attention —the problem of life, and the problem of consciousness or mind. The knowledge which we possess, or imagine we possess, of the latter, which is gained from a purely introspective point of view, the psychological aspect, I leave at present quite out of the question. As external observation through our senses would never have given it; as in the map of reality which we call nature, we have not even succeeded in accurately locating consciousness,—I relegate this large department of Thought to a different place in this work. At present we have to do only with the study of nature, the first condition of

16. Life and mind.

object was to dispel the popular conception that the accuracy and variety of the performances of the human eye could be explained by the precision and complexity of its structure, as if it were an optical instrument of a degree of perfection which could not be equalled by any optician. In the sequel Helmholtz shows how this admiration of a wrongly supposed mechanical perfection must make room for an admiration of a different kind, as "every work of the organic formative power of nature is for us inimitable"; a remark which really supports the argument in the text ('Vorträge und Reden,' 3. Aufl. 1884, vol. i. p. 240, &c.) It is also important to note how Helmholtz traces the imperfections of the eye to its genesis—*i.e.*, its development in the embryo. The genetic supplements the purely structural examination (ibid., p. 255).

which is that her phenomena have, or have at some time had, a definite place and position in space. Here, then, the phenomena of lower and higher life and the new creations of human culture, art and industry, open out a great department of reality which is accessible to external observation and study. Without committing ourselves to any theory on the subject, we have in this department to deal with the phenomena of apparent or real design and purpose. How has the century dealt with these phenomena? The answer to this question, the history of nineteenth century thought as directed towards the phenomena of life and of mind as natural phenomena, will be dealt with in two further chapters, which will respectively deal with the vitalistic[1] and the psycho-

[1] It would have been in some respects preferable to use the word "biological" instead of vitalistic. In fact, in the original draft of this passage I used the former term. The reasons which made me alter it are the following: The term biology was first used in 1801 by Lamarck in his 'Hydrogéologie.' "About the same time it occurred to Treviranus that all those sciences which deal with living matter are essentially and fundamentally one, and ought to be treated as a whole; and in the year 1802 he published the first volume of what he also called 'Biologie.' Treviranus's great merit lies in this, that he worked out his idea, and wrote the very remarkable book to which I refer. It consists of six volumes, and occupied its author for twenty years —from 1802 to 1822. That is the origin of the term 'biology'; and that is how it has come about that all clear thinkers and lovers of consistent nomenclature have substituted for the old confusing name of 'natural history,' which has conveyed so many meanings, the term 'biology,' which denotes the whole of the sciences which deal with living things, whether they be animals or whether they be plants." This extract from Huxley's "Lecture on the Study of Biology" (South Kensington, Dec. 1876, reprinted in 'American Addresses,' &c., 1886, p. 129, &c.), has induced me to adopt the term "vitalistic" to denote those doctrines and chapters in biology which deal specially with the principle and phenomena of life. A very large portion of biology deals with such phenomena of living things as can be studied without any reference to a doctrine or theory of life in particular, they being either mere facts of distribution or that very large and increasing class of biological processes which admit of purely mechanical, physical, or chemical description and explanation. The very fact, however, that the question whether the principle of life is purely mechanical

physical views of nature. Thus four distinct chapters, dealing severally with the morphological, the genetic, the vitalistic, and the psycho-physical aspects of nature, will together attempt to describe the manifold and changing methods of reasoning by which our century has approached the actual things and events which surround us.

17. Vitalistic and psycho-physical aspects.

"Nature does not employ all figures, but only certain ones of those which are possible: and of these, the determination is not to be fetched from the brain, or proved *a priori*, but obtained by experiments and observations." These words, set down nearly two centuries ago by a now forgotten natural philosopher,[1] express clearly the object of a study which, towards the end of the eighteenth century had received definite expression in vari-

or not is not yet decided, makes it necessary to retain in a history of Thought a special term comprising all speculations which deal with the purely scientific solution of that problem. In fact, the question what is life is still unanswered. *A fortiori*, these remarks refer also to the question, What is mind or consciousness? But the two chapters referring to these problems will limit themselves to an historical exposition of what has been done to solve them by purely scientific, *i.e.*, exact, methods. The full name of the author of the 'Biologie' was Gottfried Reinhold Treviranus (1776-1837) of Bremen. Though introducing the larger conception of biology, his own original labours were mainly in the domain of zoology. His brother, Ludolf Christian Treviranus (1779-1864), devoted himself mainly to botanical science, and was largely influenced by the doctrines of the "Naturphilosophie." On the former, see Carus, 'Geschichte der Zoologie' (München, 1872), *passim*; on the latter, Sachs, 'Geschichte der Botanik' (ibid., 1875, p. 291).

[1] They are quoted by Whewell ('Hist. Induc. Sciences,' 3rd ed., vol. iii. p. 165), from a work entitled 'Dissertatio de Salibus' (1707), by the Italian Professor at Padua, Dominico Gulielmini (1655-1710). He was a practical physician as well as a natural philosopher. He was the forerunner of Romé de Lisle and Haüy, inasmuch as he established the principle, not then sufficiently appreciated, that the constancy of the angles is characteristic of all crystals. See Kopp's 'Geschichte der Chemie,' vol. ii. pp. 83-404.

ous branches of natural science, and which can be best characterised by the term morphology.[1] The word was first applied only to plants, then also to animals, and later still to crystals and minerals. The words quoted above refer to the forms of inanimate nature, to crystals. In all these cases we have to do with definite individual objects, which can be removed from their surroundings and examined in the laboratory. There is, however, no reason why a study of the actual forms of nature on a large scale, such as the physiognomy of landscape, the configuration of mountains and valleys, the shapes of glaciers, the actual distribution of land and water on our globe, the stratification of rocks, the formation of clouds, and many other things, should not all be comprised under the term, the morphological view of nature. And conceived in this larger sense, the study of nature as a whole and in its separate parts had at the end of the eighteenth century already made very important progress. In fact, natural history had, in the course of that century, gradually emerged from the previous epoch, that of the purely systematic and classificatory attempts, which aimed at giving inventories, collecting specimens, and classifying natural objects, naming, describing, and identifying them. The interest of the latter was a practical one, frequently

18. Morphology defined.

[1] In the 'Leçons sur les Phénomènes de la Vie communs aux Animaux et aux Végétaux,' a work which did so much to break down the older division of the sciences which deal with animals and vegetables separately, Claude Bernard says (p. 333 of vol. i., 1885): "Dans un autre équilibre cosmique, la morphologie vitale serait autre. Je pense, en un mot, qu'il existe virtuellement dans la nature un nombre infini de formes vivantes que nous ne connaissons pas. Ces formes vivantes seraient en quelque sorte dormantes ou expectantes. . . . Il en est ainsi des corps nouveaux que forment les chimistes; ils ne les créent pas, ils étaient virtuellement possibles dans les lois de la nature."

prompted by the needs of the medical profession, which studied animals as affording an insight into the analogous structure and functions of the human body;[1] and plants, because they largely furnished the materials for the preparation of medicines. To this must further be added the practical interests of agriculture, of gardening, and of the artificial culture of flowers and exotic plants, and the breeding of domestic animals. All these interests, however stimulating they may have been and still are, introduce an element of artificiality into the study of nature. They have all a greater concern for natural objects, be they beautiful or useful, than they have for nature itself. From this artificial position the true sciences of nature had to emancipate themselves by slow degrees and with many efforts. Ever since the time of Linnæus, through whose labours the systematic attempts received a kind of finality, and even in his own writings, great discussions were carried on as to

19. Artificial and natural systems.

the difference between a natural and an artificial order of plants and animals. "The natural orders,"[2] says Linnæus, "teach us the nature of plants, the artificial orders enable us to recognise plants. The natural orders, without a key, do not constitute a method; the method ought to be available without a master. . . . The habit of a plant must be secretly consulted. A practised botanist will

[1] Referring to Albrecht von Haller, Victor Carus ('Gesch. d. Zoologie,' p. 567) says, "Through the leap which physiology took, thanks to his labours, zootomical researches developed in a direction which brought them into complete subjection to physiology, with a neglect of the independent importance which belongs to them. . . . It diverted attention from the immediate object of zoology, the explanation of animal forms and their variety, to the more remote problem—the explanation of the phenomena of life."

[2] Quoted by Whewell ('Hist.,' vol. iii. p. 268) from the 'Genera Plantarum' (1764).

distinguish at the first glance the plants of different quarters of the globe, and yet will be at a loss to tell by what mark he detects them. There is, I know not what look—sinister, obscure, in African plants; superb and elevated in the Asiatic; smooth and cheerful in the American; stunted and indurated in the Alpines."[1] The inventor of the sexual system of plants, which proved to be such a good "finder" in the hands of the botanist and herbalist, speaks of the difficulty of the task of discovering the natural orders. "Yet," he says, "I, too, have laboured at this—have done something, have much still to do, and shall labour at the object as long as I live."[2]

20. Linnæus and Buffon.

Linnæus's artificial system met with little acceptance in France, where, under the opposite influence of Buffon,[3]

[1] Quoted by Whewell ('Hist.,' vol. iii. p. 268) from the 'Philosophia Botanica' (1751).

[2] Ibid., quoted from the 'Classes Plantarum' (1738). Julius Sachs, in his excellent 'History of Botany' (Munich, 1875, transl. from the German by H. E. Garnsey, 1890), says of Linnæus, that in his morphological as well as in his systematic labours, there existed two unreconciled conceptions—a superficial one, meant only for practical use, which found expression in his artificial sexual system, and a deeper, scientifically valuable one. "For practical purposes of description he elaborated his nomenclature of the parts which, however useful, appears nevertheless flat and superficial, as any deeper foundation through a comparative study of forms is wanting. But alongside of this, there appears in various passages of his writings the desire for a more profound conception of plant-forms. What he had to say on this subject he brought together under the term 'metamorphosis plantarum'" (p. 110 of the German edition).

[3] Buffon's great name has a place in the history of the genetic as well as of the morphological view of nature, inasmuch as he looked at the things of nature as much from the side of their individual speciality as from that of their connection and orderly arrangement in time and space. And inasmuch as he "does not only consider the form, but tries to maintain an interest in the general economy of the whole of nature by picturing to us the homes, the habits and customs, the instincts, &c., of living things, so he strove in general to represent the single phenomena of nature as existing in intimate connection" (Carus, 'Gesch. der Zoologie,' p. 523). "As Buffon opposed the extreme systematisers, who seemed to think it the end of science, not so much to know about an object as to be able to name it, and fit it into their system, so Daubenton (the collaborator of Buffon in France)

the great botanists, from Jussieu to De Candolle, and the great zoologists, notably Cuvier, made an attempt towards a freer and more generous and more sympathetic conception of the objects as well as the totality of nature. These attempts were continued much on the same lines till well on into the nineteenth century. Buffon's comprehensive scheme was premature, but it had a very great and beneficial influence in popularising and enlivening the frequently dry and uninteresting pursuits of the collector and systematiser. Cook's voyages during the last third of the eighteenth, and Humboldt's travels at the turn of the two centuries, did much to further a comprehensive view; but the great task of the morphologist, like every other scientific work, had to be solved by special studies in separate departments. It grew from small beginnings and detached contributions.

One of the most notable of these, and one also which has all along exerted a great influence on all morphological studies, is the theory of crystals, both natural and artificial. I have already had occasion to refer to the labours of Haüy [1] and his successors. They have led to a complete mastery of the geometrical forms which minerals occasionally present in nature, and which substances assume if allowed to solidify out of the liquid condition. The science of crystallography, now appro-

21. Morphology of crystals. priately termed the "morphology of crystals," [2] has had

insisted on the study of each animal as an individual whole. . . . He occupied himself, therefore, with the production of a series of admirable monographs appended to the descriptions of Buffon in the 'Histoire Naturelle'" (Huxley in the chapter on Owen's position, &c., in 'Life of Richard Owen,' 1894, vol. ii. p. 280).

[1] See vol. i. p. 116, &c., of this history.

[2] See 'The Morphology of Crystals,' by N. Story Maskelyne, 1895.

a peculiar fascination as forming the transition from the abstract science of geometrical forms and statical equilibrium to the study of the actual forms of real things. Here, if anywhere, it seemed as if we might discover the link that connects the theoretically calculable with the actually existing, the possible with the real. Accordingly, we find a very general and recurring tendency to carry over the notions of crystallography into other sciences — into the morphology of plants and animals. The planes and axes of geometry, and the forces of attraction between particles of matter, have formed a theme which has been endlessly repeated and varied in explaining the elements and the forms of living matter. But whilst these fanciful analogies [1] of organic crystals, of polar distribution, and the network of tissues, to which are also allied the spiral theories of leaves and branches in plants and other geometrical arrangements, have at times attracted much attention,[2] and have served to give at least the

[1] "Ces comparaisons entre les formes minérales et les formes vivantes ne constituent certainement que des analogies fort lointaines, et il serait imprudent de les exagérer. Il suffit de les signaler. Elles doivent simplement nous faire mieux concevoir la séparation théorique de ces deux temps de la création vitale: la création ou synthèse *chimique*, la création ou synthèse *morphologique*, qui en fait sont confondues par leur simultanéité, mais qui n'en sont pas moins essentiellement distinctes dans leur nature" (Claude Bernard, 'Leçons sur les Phénomènes de la Vie,' &c., vol. i. p. 296). See also on the extravagances of such search for analogies, Jul. Sachs, 'Gesch. d. Botanik,' p. 173, &c.

[2] I shall revert to this subject when speaking of the elder De Candolle. Here only a passing remark on the "spiral theory," which was mainly developed by K. F. Schimper and Alexander Braun, after the regular geometrical arrangement of leaves around their stalks had already been noticed in the eighteenth century by Charles Bonnet, following Cæsalpinus. For about thirty years, from 1830 onward, the spiral theory was very popular in Germany. In France, the somewhat related theories of symmetry of De Candolle, of metamorphosis of Goethe and of spiral

semblance [1] of an explanation of organic structures and forms, they have in reality done as little as Boscovich's centres of force and curves of attraction and repulsion in mathematical physics to establish a firm basis for actual research; for nowhere have they been capable of exact determination such as has been applied to the angles and figures of crystals.

22. Morphology on a large scale.

Simultaneously with the science of crystallography there came into being the science of minerals on a larger scale of study, through actual observation in definite localities of the formation and stratification of rocks; of the traces of the influence of the great

arrangements of Schimper, became known under the term "Morphologie végétale," through Auguste de Saint Hilaire in his 'Leçons de Botanique' (1840). To the spiral theory, although strongly opposed in course of time by Wilhelm Hofmeister, one of the founders of the genetic conception of plant life, Sachs, the historian of botany, nevertheless assigns an important historical influence, "as through Schimper's theory the morphologically so important relative position of the plant organs was for the first time placed in the foreground of morphology" (*loc. cit.*, p. 180). See, however, on this subject the paper by A. H. Church on "Phyllotaxis" in vol. i. p. 49 of 'The New Phytologist,' 1902.

[1] The early propounders of the cellular theory of organic structures adopted the view that cells were formed in a surrounding liquid in the manner of crystals in a mother-liquor. When it was established that organic structures grow by intussusception, not by juxtaposition and accretion, like crystals, and that cells multiply by division, the discoveries of Graham, who divided bodies into crystalloids and colloids, were utilised for the purpose of explaining or illustrating organic processes. On this distinction is based the celebrated "micellar theory" of Nägeli, who, in his 'Mechanisch-physiologische Theorie der Abstammungslehre' (München und Leipzig, 1884), works out a complete mechanical doctrine of the constitution and formation of organic structures. The ideas contained in this elaborate treatise have been much used in Germany by various writers, but mostly only as convenient illustrations. See O. Hertwig, 'The Cell' (transl. by Campbell, 1895), p. 58, &c. The micellar theory does not seem to have found much favour in France or in this country, where a general opinion prevails which is probably best represented in the words of Claude Bernard: "Les phénomènes physico-chimiques des êtres vivants, quoique soumis aux lois de la physique et de la chimie générales, ont leurs conditions particulières qui ne sont réalisées que là, et dont la chimie pure ne peut offrir qu'une image plus ou moins inexacte" ('Phén. de la Vie,' &c., vol. ii. p. 487).

agencies of nature,—of water, atmosphere, and of ice and heat. Last came the study of the fossil remains of organic life as the means of fixing the age and the order of succession of various geological formations. Werner [1] in Germany, Cuvier [2] in France, Hutton [3] in Scotland, William Smith [4] in England, led the way, from different points of view, towards an actual knowledge and a possible theory of the existing forms and structures in and on the crust of our globe. The study of these subjects, morphology on the largest scale, necessitated distant travels, the examination of formations *in situ* and under diametrically opposite conditions. Its greatest and unequalled representative was Alexander von Humboldt,[5] who also brought the observations of 23. Humboldt.
geographical, geological, and mineralogical facts and details into connection with the study of climate, of the weather, of the distribution of plants and animals.[6]

[1] See *supra*, vol. i. p. 283.
[2] Ibid., p. 125.
[3] Ibid., p. 283.
[4] Ibid., p. 291.
[5] A good account of the gradual development of the plan of "Cosmos" will be found in Bruhns's 'Life of A. von Humboldt' (transl. by Lassell, 1873), vol. ii., *passim*. It is clear that two great influences co-operated to ripen in Humboldt's mind the conception of unrolling a great tableau of the physical world in its purely material and in its ideal or poetical aspects: the influence of the great scientific movement then emanating from Paris, and the not less important influence of the ideal movement represented by the names of Herder, Goethe, and Schelling, which emanated from the centre of Germany. "But, however greatly Humboldt may be indebted to the inspiring influence of his contemporaries, the great merit of the work lies in what he alone has accomplished—the attempt by means of a comprehensive collation of details, and the institution of the most searching comparisons, to give a scientific foundation to the ideal cosmology of Herder, Goethe, Schelling, and their disciples . . . In him may be said to be united the two schools of philosophy, so brilliantly represented during the closing years of the former century. On this account he was at the same time exposed to the censure of the representatives of either system" (vol. ii. p. 312).
[6] The third volume of the 'Life of Humboldt,' in the original German edition, gives an account

He may be called the morphologist of nature on the largest scale: the representation of the grand aspect of things as exhibited in his 'Cosmos,' and in his earlier 'Ansichten der Natur,' was the leading idea of his life and work. Through him and his friend Karl Ritter "comparative geography received a treatment worthy of the subject, showing its connection with the history of the human race and the advancement of civilisation, inasmuch as the configuration of the earth is proved to have been an important element in the dispersion of nations."[1]

But morphology, or the study of forms and structures, has to be carried on not only on the large, the gigantic scale, as by Humboldt; it is quite as important, and has probably been even more influential, when directed towards the minute, the imperceptibly small, which ordinarily quite escapes our notice. If

by various specialists of Humboldt's labours in the sciences of astronomy, geology, geography, the distribution of animal and plant life, meteorology, and other provinces of research, some of which largely owe their existence to his initiative. The study written by Ewald on his geological work, and that of Griesbach, on what is termed in German animal and plant geography, are specially interesting. Unfortunately this most fascinating volume has not been brought out in the English edition. As illustrating the comprehensiveness of Humboldt's view it is well to note how, before beginning to put together his materials in the great tableau which the 'Kosmos' was intended to be, he drew two entirely different pictures of nature on our globe; first in the large work on the New Continent ('Voyage aux Régions équinoxiales du Nouveau Continent,' in six parts, published in Paris, 1805 to 1834), and then from an entirely opposite aspect in his works on Central Asia ('Asie Centrale: Recherches sur les Chaînes et Montagnes et la Climatologie comparée,' 3 vols., Paris, 1843). "To Humboldt the importance of the Asiatic expedition consisted in its elevating him above the one-sided effect of having contemplated nature exclusively in the New World, and leading him, so to speak, to feel experimentally that the earth, in common with every other object, is possessed of opposite sides" ('Life of Humboldt,' vol. ii. p. 212).

[1] See 'Kosmos,' vol. i. p. 60 (German edition, 1845).

the great revolution of ideas which the seventeenth century witnessed was much assisted by the invention of the telescope and founded upon its revelations, the change of thought during the nineteenth century has been connected more with the revelations of the microscope. The great movement of ideas started by Galileo, and continued through Kepler, Newton, and Laplace, was accompanied by the perfection of the telescope. The invention of the microscope enabled Nehemiah Grew and Malpighi to begin half a century later their embryological studies, and to inaugurate a line of research which, in our days, through a long series of observations[1] from Amici to Strasburger on the pro-

24. Morphology on a minute scale.

[1] These observations begin with the year 1830, when Amici, to whom great improvements in the microscope are due, "traced the pollen grain from its lighting on the carpel tip down into the recesses of the ovule" (Geddes and Thomson, 'The Evolution of Sex,' p. 140), and removed all doubts and uncertainty by his observations on orchids in 1845 and 1846. "Here he demonstrated the whole series of processes, from the pollen dust on the stigma to the formation of the embryo" (Sachs, 'Gesch. d. Botanik,' p. 469). About the same time (1843) Martin Barry "observed the presence of the sperm within the ovum in the rabbit ovum" (Geddes and Thomson, *loc. cit.*, p. 142). It took, however, a quarter of a century, from the first discovery of Amici, before the process of fertilisation described by him was accepted by embryologists as typical for both plants and animals. Bischoff, the great authority in Germany, after confirming the entrance of the sperm-cell into the ovum, maintained by Barry in 1843, and by Newport (with frogs) in 1851 and 1853, expresses his "infinite astonishment," adding that "Dr Barry is certainly the first who has seen a spermatozoon in the interior of any ovum, and notably in the ovum of a mammal, and that to him belongs the glory of this discovery" (Theod. Bischoff, 'Bestätigung des von Dr Newport bei den Batrachiern und Dr Barry bei den Kaninchen behaupteten Eindringens der Spermatozoiden in das Ei,' 1854, p. 9). For the history of scientific thought it is significant to see how little, even in the middle of the century, discoveries referring to the phenomena of plant life or structure were known or utilised by students of animal life. A mutually fructifying influence seems to date, like so many other advances from the publication, in 1859, of the 'Origin of Species.' "The distinctively modern era in the history of fertilisation dates from about 1875, when the brilliant researches of Auerbach, Van Beneden, Bütschli,

cess of fecundation in plants, and from Martin Barry to Hertwig and Fol on that in animals, has been brought to a temporary climax. The combination of telescope and microscope in the spectroscope has opened out a field of research in astronomy of which Laplace had no conception.

So much has depended, during our century, on the unravelling and disentangling of the imperceptibly small (once considered an unworthy occupation), that a short reference to the history of that optical instrument to which we are so greatly indebted may not be out of place.

25. The Microscope. The gradual perfection of the microscope is as much indebted to the problems and labours of anatomical workers during the seventeenth and the nineteenth centuries, as anatomy itself reciprocally has been indebted to the microscope. Robert Hooke, in 1660, first gave a useful form to the compound instrument. Leuwenhoek perfected the simple microscope; and during the earlier part of our century no one did more than Amici in Modena and Lister in England[1] to start that great succession of improvements by which errors due to colour and indistinctness—the chromatic and spherical aberrations—were removed. In the middle of the century the influence of some eminent botanists, notably of Hugo von Mohl and Nägeli, in perfecting micrometric processes was considerable; whilst the last twenty years have witnessed quite a new departure in the theory of optical images, in that of microscopic vision, in the improvement of optical glass, and in the investigation of the possible limit of the magnifying powers. The most eminent physical authorities—such as Stokes and Lord Rayleigh in England, Helmholtz in Germany — have taken up one or more of these points; but the whole subject is associated with the name of Prof. Ernst Abbe[1] of Jena, who, through his connection with the well-known firm of Carl Zeiss, has been able to put into actual practice many of the suggestions which resulted from his theoretical investigations. As the historians of zoology

Fol, O. Hertwig, and others, showed that one of the essential phenomena in fertilisation is the intimate and orderly association of the sperm-nucleus, of paternal origin, with the ovum-nucleus, of maternal origin, the result being the cleavage or segmentation-nucleus. The researches of Strasburger, De Bary, and others, established the same result in regard to plants" (J. A. Thomson, 'The Science of Life,' p. 127, 1899).

[1] The improvements of Amici seem to go back to the year 1812, those of Lister to 1826. The former is usually considered the inventor of the "immersion" system, —that of placing a drop of water between the object or its covering glass and the objective lens. This system has lately been improved by Abbe, who discovered a liquid with the same refractive index as the glass of the objective possesses. According to Hogg ('The Microscope,' 15th ed., 1898, p. 10), the immersion system was suggested by Pritchard in London before Amici hit upon it. The necessary modifications required where the immersion system is used, seem, however, to have been first worked out by the celebrated Paris opticians, MM. Hartnack and Nachet.

[1] The labours of Abbe go back to the year 1873. Simultaneously and independently, Helmholtz attacked the theory of microscopical vision and the question of "resolution"—*i.e.*, of the possible limit to the resolving power of any optical arrangement. Airy had attacked the same subject on purely dioptrical lines. Helmholtz and Abbe went a step farther, taking into account the physical nature of light as a wave-motion, subject to interference phenomena, notably those caused by inflection, where objects with very fine markings are concerned. Abbe's methods were for a long time only imperfectly known. The publication, however, of his theories by Czapski ('Theorie der optischen Instrumente nach Abbe,' Breslau, 1893) made the whole subject better known, and has been followed by two masterly papers by Lord Rayleigh and Prof. Johnstone Stoney in the 42nd vol. of the 'Philos. Mag.' (1896). The latter paper especially gives several interesting examples of the use of recent microscopic appliances and the means of avoiding errors in handling very delicate and minute objects. It seems that the instrument cannot any longer be used without a theoretical knowledge of its optical construction, which enables the observer not only to see, but also to criticise and to interpret.

and botany tell us, the use of the microscope had made little or no progress[1] during the eighteenth century: the study of structures and tissues had lost interest in comparison with the study of the physiological functions of the parts of plants and the organs of animals, which had been respectively furthered by Hales in England and by Haller in Germany.[2] Our century thus found the morphological studies of the imperceptibly small in a very backward state: it had to improve the instrument for its research *pari passu* with this research itself.[3] But it has been truly remarked that the increased use of the microscope necessitated likewise a mental training in the interpretations and delineations of what was observed through it. "By fortifying the eye with the micro-

26. Its improvement.

[1] "So long as, in consequence of the imperfections of optical instruments, deceptive images existed, and, for instance, all microscopical structures appeared as composed of rows of beads, the explanation of what was seen stood under the influence of deceptions, which were only gradually recognised as such" (Carus, 'Gesch. d. Zool.,' p. 629). Compare also what Sachs says (Gesch. d. Bot., p. 241).

[2] "The characteristic feature of that period lay in this, that the examination of the finer structure is always mixed up with reflections on the functions of elementary organs, so that anatomy and physiology always support each other, but also, in consequence of their imperfect state, do each other injury" (Sachs, *loc. cit.*, p. 240). Similarly Carus (*loc. cit.*, p. 567), "Through the progress which physiology made, thanks to Haller's activity, zootomical investigations took a direction which brought them into complete dependence on physiology, . . . and retarded the progress of zoology by diverting attention from its primary object—the exposition of animal forms and their differences."

[3] As late as 1827 Aug. Pyrame de Candolle could still write ('Organographie végétale,' vol. i. p. 7), "De nos jours, MM. Mirbel, Link, Treviranus, Sprengel, Rudolphi, Kieser, Dutrochet, et Amici ont publié des recherches très délicates sur le tissu végétal, et les ont accompagnées de figures nombreuses et soignées ; mais la nécessité d'employer continuellement dans ces recherches un instrument aussi difficile à bien manier que l'est le microscope composé, fait que malgré l'habileté de ces observateurs, l'anatomie délicate des végétaux est encore . . . d'une incertitude désespérante pour les amis de la vérité."

scope, it became itself a scientific instrument which no longer hurried over its objects in flighty motion, but is disciplined by the intellect of the observer and forced into methodical work."[1] Similarly, no doubt, the increasing devotion to the pastime of sketching from life and nature in our days must have the effect of obliging the eyes of many persons to look stedfastly and carefully at the forms and outlines of things, and of thus training the artistic faculty.

It is, however, a remarkable fact that one of the greatest leaders in the morphological study of natural objects, Bichat, the great observer of membranes and tissues, despised the microscope, the instrument by which the sciences he founded were to benefit so enormously.

The object of morphology, as distinct from that of classification, can be defined as the attempt to describe, and if possible to comprehend and explain, the relative similarity as well as the graduated differences of form and structure which natural objects present to our gaze. Although the study can be conducted on a large as well as on a small scale, these similarities and differences sooner made themselves felt in the comparatively smaller objects of living nature. These can, without apparent loss of their characteristic appearance and individuality, be collected and brought together, whereas a collection of minerals, with the exception of crystals and gems, always presents only fragments, and forces upon us the conviction that they can really be studied only in their habitation, *in situ*. The same conviction has indeed gradually

27. Morphology and classification.

[1] Sachs, *loc. cit.*, p. 237.

made its way into botany; and last of all into zoology. The herbarium or collection of dead plants was much sooner superseded by the "jardin des plantes" than the zoological museum with its skeletons, stuffed animals, and specimens in alcohol has been supplanted by any scientific collection of living animals. Marine stations, which study plant and animal life *in situ*, are quite a recent invention.[1] The study of the forms of nature or morphology in the earlier or more limited sense, referred thus more exclu-

[1] M. Yves Delage distinguishes four great periods in the study of living things. The first, culminating in Linnæus and Buffon, studies living objects in the great outlines of their external forms, of the habits of plants and the customs of animals. Detailed examination by dissection is resorted to, but only as a secondary method and in order to supplement the intuitive discovery of natural affinities. Then comes the second period, that of Cuvier and his followers, relying mainly on anatomical dissection. The third period begins with the marine stations. "Je ne crains pas de dire que la fondation des laboratoires maritimes a marqué une troisième période et constitué une nouvelle méthode aussi importante que les précédentes. Si l'on songe que plus des trois quarts des types d'invertébrés appartiennent au monde de la mer, que le plus grand nombre ne pouvaient parvenir dans les centres scientifiques dans un état convenable pour l'examen microscopique, si l'on songe que tout ce qui concerne leurs mœurs et leur embryogénie ne peut s' étudier loin de la mer, on comprend l'importance de ces créations. Faut-il rappeler que l'introduction de cette méthode est due à H. de Lacaze-Duthiers? . . . Aussi la fondation du laboratoire de Roscoff a-t-elle été le signal de la création d'une multitude d'établissements plus ou moins similaires sur les côtes de tous les pays" ('L'Hérédité et les grands problèmes de la Biologie,' p. 3). The fourth period is marked by microscopic anatomy, and this—according to M. Delage—has its home mainly in Germany. "The study of marine zoology has, since the publication of the 'Origin of Species,' been found to require more complete arrangements in the form of laboratories and aquaria than the isolated vacation student could bring with him to the seaside. Seaside laboratories have come into existence: the first was founded in France by Coste (1859) at Concarneau (Brittany) with a practical end in view—viz., the study of food-fishes, with an aim to pisciculture. . . . The largest and best-supported pecuniarily is that founded at Naples by Anton Dohrn in 1872; others exist at Trieste, Villefranche, Cette, and at New Haven and Beaufort in the United States; whilst a large laboratory, on a scale to compare with that at Naples, has been (1888) opened at Plymouth by the Marine Biological Association of the United Kingdom" (Ray Lankester, art. "Zoology" in 'Encyclop. Brit.,' vol. xxiv. p. 814).

sively to plants and animals, and here the term was first applied. In order to bring some kind of method into the perplexing study of living forms, two ways presented themselves; and they were consciously or unconsciously followed by morphologists with more or less success. As I mentioned above, one of the chief interests which led to zoological and also to botanical studies was the medical interest. Animals were dissected and observed, as affording by analogy an insight into the structure and processes of the human body. Physiology, the science which deals with the actions of the different parts of the animal or human frame, termed from an early period the functions of the different organs, had made considerable progress during the eighteenth century. It was then found convenient to study the whole organism as an assemblage of different organs or machines, each of which performs a certain function. Thus we have the mechanism on which voluntary motion depends, the mechanism of respiration and of the circulation of the blood through the body, the mechanism of digestion, the mechanism of reproduction, and finally, the mechanism of the nervous system with its specified and localised optical, auditory, and other organs of sense. All these parts or organs could to a great extent be separately studied and described in their mechanical, chemical, and electrical actions. These studies had, since the time of Harvey in England and Haller in Germany, made great progress. The application of chemistry to the processes of respiration and digestion, and finally, the discovery of the galvanic current by Galvani, had given a great impetus to the physiological study of the different

28. Study of separate organs.

organs in living beings, and their functions. In plants, these organs and functions seemed to be much simpler and more easily observed than in animals, and Linnæus had selected the sexual organs, since they were the most easily distinguishable, as a primary character for his classification of the vegetable kingdom. Somewhat later [1] he classifies the animal kingdom according to the internal structure, and characterises animals for the purpose of division according to the heart and the blood. The celebrated dictum, that "minerals grow, plants grow and live, animals grow, live, and feel," which appeared in the last edition of the 'Systema Naturæ,' places a physiological distinction at the base of the classification. This conception, which has been somewhat modified since Linnæus's time to meet our altered views, is an obvious first step towards a description of natural objects. Yet this no more than the second step, which fastens upon the organs of reproduction in plants, on the heart and blood in animals, gives any clue to the comprehension of the great variety and apparent fixity of forms which the living world presents to our observation. In fact, purely morphological considerations were subordinated to physiological ones, and were brought in only to assist in the further subdivision of the two great kingdoms. Linnæus felt the artificiality of his classification—the arbitrariness of the characters he selected for the purpose of division. But a more natural system could only

29. Outdoor studies. be arrived at by an intimate knowledge of and intercourse with living nature, as well as by a careful comparison of its hidden forms and organisation—*i.e.*, by a more de-

[1] See Carus, 'Geschichte der Zoologie,' p. 503, &c.

tailed external and internal morphology. Both lines of study, with their respective methods of observation, research, and reasoning, were equally wanted. The former was more easily attained with plants, the latter promised more immediate fruit in dealing with animals. In following the former, Bernard de Jussieu became the founder of modern descriptive botany; in taking up the latter, in founding comparative anatomy, Georges Cuvier became for a long time the leader in zoology.

Bernard de Jussieu was led to his natural system of 30. Jussieu.
classification, not by any theoretical considerations, but by the practical task of arranging the plants in the garden of Trianon, confided to his care by Louis XV., who was a great lover of botany. He had with him as assistant his nephew, Ant. Laurent de Jussieu, who in 1789 published his 'Genera Plantarum,' which is, so far as method goes, the work of his uncle. "This work produced a veritable revolution in botany, for only since its publication have plants been studied according to the relations which they exhibit and according to the totality of their organisation." [1] It was not one special character or side of their existence, arbitrarily selected by a first superficial observation, which served as a means of description; their different parts or organs were conceived to be correlated—*i.e.*, dependent on each other and united to form the totality of their organisation—their various characters were all taken into account, and looked upon as subordinated one to the other.[2] From the time of

[1] See 'Histoire des Sciences Naturelles,' par Geo. Cuvier, complétée par T. M. de Saint Agy, Paris, 1845, vol. v. p. 298.

[2] Aug. Pyrame de Candolle ('Théorie élémentaire de la Botanique,' Paris, 1819, 2nd ed., p. 69) gives the following account of

Jussieu we find introduced into natural science, mainly for the purposes of classification, the ideas of the correlation of the different parts and the subordination of the various characters of a plant or an animal. Physiology and anatomy, hitherto mainly occupied with the study of the different organs, were henceforth to be occupied with the problem of organisation, the problem of the unity of

31. Problem of organisation.

the various characters and organs. Inspired by Jussieu, De Blainville looked upon the whole development of the natural sciences as the history of our knowledge of organisation,[1] and De Candolle, Jussieu's great successor in botany—the name that in systematic botany ruled the nineteenth century—wrote an 'Organographie végétale,' a rational description of the organs of plants.[2]

the method of the two Jussieus: "Ce qui charactérise la méthode des Jussieu, c'est qu'elle est fondée sur la subordination des caractères. Sentant le vague des simples méthodes de tâtonnement, l'exagération du principe de comparaison uniforme et générale des organes, ils ont les premiers remarqué avec soin, que tous les organes, tous les points de vue sous lesquels on peut les considérer, n'ont pas un égal degré d'importance, ni de permanence, que quelques-uns semblent, pour ainsi dire, dominer les autres; de sorte qu'en établissant la classification d'abord sur ces organes prédominans, puis les divisions secondaires sur ceux qui ont un moindre degré d'intérêt, on est conduit à imiter le plus possible l'ordre de la nature dans celui de la classification. Ce principe simple et peu contestable a été fécond en conséquences importantes; et c'est sous ce point de vue, que l'un des hommes qui a le plus profondément réfléchi sur la marche des sciences et sur le plan général de la nature, a proclamé, dans une occasion solennelle, le livre de M. de Jussieu, 'comme un ouvrage fondamental, qui fait, dans les sciences d'observation, une époque peut-être aussi importante que la chimie de Lavoisier dans les sciences d'expérience.'" (See Cuvier, 'Rapport historique sur les progrès des sciences naturelles,' Paris, 1810, p. 305.)

[1] See the 'Étude sur la vie et les travaux de M. Ducrotay de Blainville,' par Pol. Nicard, Paris, 1890, p. 157 *sq.*

[2] See A. Pyr. de Candolle, 'Organographie végétale ou Description raisonnée des Organes des Plantes,' Paris, 1827, 2 vols., especially vol. ii. p. 245, &c. "The classifications of the scientific taxonomist are of two kinds. Those of the one sort are merely handy reference catalogues. Such are the 'artificial' systems, useful in their day and for their particular purpose, but of no other value. The others, known as 'natural' classifications, are arrangements of objects according to the sum of their likenesses and unlikenesses, in respect of certain characters; in morphology, therefore, such classifications must have regard only to matters of form, external and internal. And natural classification is of perennial importance, because the construction of it is the same thing as the accurate generalisation of the facts of form, or the establishment of the empirical laws of the correlation of structure" (Huxley in 'Life of Owen,' vol. ii. p. 283).

The problem of organisation was much easier in dealing with plants than with animals. In the former there seems to be only one organ or system of organs definitely developed and marked off—namely, the organs of fructification; and these had accordingly served Linnæus and his successors as the leading character for their descriptive classification. In animals there are, or seem to be at least, four or five well-defined and separated systems of organs. The selection, for the purposes of classification and morphology, was much more difficult. Accordingly we find Cuvier, who between the years 32. Cuvier.
1795 and 1817 devoted himself to the morphological and anatomical study of the animal kingdom, hesitating in the selection of the leading character according to which he should classify and arrange it. As I have had occasion to remark above,[1] he finally in 1812 settled on the nervous system as the leading character governing the figure of an animal organism.[2] Before

[1] See vol. i. p. 130 of this history.

[2] On the gradual development of Cuvier's classification see Carus, 'Geschichte der Zoologie,' pp. 602, 612, 614. "It did not escape Cuvier that the idea of subordination is artificial, and that the importance of an organ can only be fixed by experience — namely, through the proof of its constancy. Nevertheless he follows this principle, but naturally becomes vacillating. Thus in 1795 he names the organs of reproduction, to the action of which the animal owes its existence, and the organs of circulation, on which depends the individual preservation of the animal, as the most important, whilst in 1812, following the example of Virey, he declares the nervous system to be that system for the maintenance of which the other systems solely exist" (*loc. cit.*, p. 602).

that, he had already adopted from Lamarck,[1] whose many-sided genius has made a lasting impress on the history of natural science in quite a different direction, the broad morphological division of the animal kingdom into animals with or without backbone, uniting under the former designation the four first classes of Linnæus. The more we follow Cuvier in the development of his classifying attempts, the more we find the form, the figure, the external and internal structure, urged as the aspect from which the organisation of living creatures is to be considered. To him fixity of form is the ever-recurring character of organised beings as distinguished from inorganic structures which depend on fixity of matter.[2] The clearer enunciation of this fixity of form is accompanied in Cuvier's view by the rejection of an idea which, before him, had very largely governed the speculations of naturalists. This idea, by which Charles Bonnet has been immortalised in natural history, is the conception of a graduated scale according to which living creatures can be arranged—viz., the celebrated Échelle des Êtres, coupled with the axiom, "Natura non facit saltus." This idea Cuvier rejects as untenable, and introduces in the place of it the conception of distinct plans called
33. "Types." later "types,"[3] according to which living beings are

[1] "An indirect inducement for a more pointed enunciation of the types of the various classes was given by Lamarck in 1797 when he placed the animals with white blood as 'invertebrates' in opposition to those with vertebræ which expressions (à vertèbres and sans vertèbres) come from him" (ibid., p. 612).

[2] See Cuvier's 'Éloge of Haüy' (El. iii. p. 156, &c.) and the extracts from it and from the 'Règne animal,' given in the first volume of this History, p. 129 and notes *passim.*

[3] According to Carus ('Gesch. d. Zool.,' p. 615), the term "type," which became current later, was introduced by De Blainville, a philosophical naturalist who held a kind of middle position between Cuvier and his opponent, Geoffroy St Hilaire. In 1816 Blainville gave the "principles of a new classification of the animal kingdom, in which, for the first time, the totality of structure of animals was used to characterise larger divisions." He divides animals first of all into three sub-kingdoms—symmetrical, radiate, and those without regular form. De Blainville seems to have been an inspiring teacher, whose ideas became suggestive and fruitful in many other minds. Nearly the whole of the third volume of Comte's 'Philosophie Positive' is written under a sense of obligation to De Blainville, whose 'Cours de physiologie générale et comparée' (1829-32) Comte considers "comme le type le plus parfait de l'état le plus avancé de la biologie actuelle" (vol. iii. p. 269, Paris, 1838). The 'Philosophie Positive' was dedicated to Fourier and De Blainville. How the latter also anticipated the modern conceptions of "Stoffwechsel" and "Metabolism" see Claude Bernard, 'Phénomènes de la vie communs aux animaux et aux végétaux' (1885, vol. i. p. 36).

modelled, and which have always existed. These types or archetectonic models are capable of certain modifications, which, however, do not affect the main features of the plan. The different classes of these main types, called "embranchements," and designated as backboned, molluscous, articulate, and radiated animals, stand near each other in independence and form no scale.[1]

The morphological view of nature took a somewhat different turn in De Candolle, the successor of Jussieu in 34. De Candolle.
botany, who, while greatly indebted to Cuvier, acknow-

[1] It is historically interesting to note that about the time when Cuvier was gradually defining more rigidly his four classes, Lamarck was working at his 'Histoire naturelle des Animaux sans vertèbres,' of which the 'Système,' &c. (Paris, 1801), can be considered the first edition, the larger work appearing from 1816 to 1822. With him there is no mention of a plan or a type. His classes form a progressive series, and he was the first to follow the path from the simple to the more complex. In opposition to Cuvier, he thus wrote: "La nature, dans toutes ses opérations, ne pouvant procéder que graduellement, n'a pu produire tous les animaux à la fois : elle n'a d'abord formé que les plus simples, et passant de ceux-ci jusques aux plus composés, elle a établi successivement en eux différents systèmes d'organes particuliers, les a multipliés, en a augmenté de plus en plus l'énergie, et les cumulant dans les plus parfaits, elle a fait exister tous les animaux connus, avec l'organisation et les facultés que nous leur observons. Or, elle n'a rien fait absolument, ou elle a fait ainsi." ('Hist. des Animaux sans vertèbres,' 2nd ed., par Deshayes et Milne Edwards, Bruxelles, 1837, vol. i. p. 42. Cf. also Carus, *loc. cit.*, p. 615.)

ledges yet another prominent influence in the formation of his ideas. Cuvier, the zoologist, contemplating the existing forms of nature from one of the two main points of view, was impressed with the contrast between the lifeless and the living, seeing in the latter stability of form, not of substance,—what we should now term dynamical equilibrium. To him the vortex is the symbol of life. De Candolle in studying plants is struck with the underlying regularity and symmetry of their formation. His views were formed after very extensive practical occupation with descriptive botany, which was followed by a lengthy residence in Paris, where, next to Cuvier, he came greatly under the influence of the Abbé Haüy, the founder of crystallography.[1] From the Jussieus he learnt the importance of looking at the "ensemble," the "port et aspect" (facies, habitus);[2] from them and Cuvier the value of the principle of the subordination of characters, and the correlation of parts in the organisation of the whole.[3] But he fastens mostly upon the underlying

[1] De Candolle, 'Théorie élementaire de la Botanique,' 2nd ed., Paris, 1819, p. 72: "Je dois encore compter, au nombre des causes qui ont influé sur l'amélioration des méthodes botaniques, d'un côté les perfectionnemens importans que la classification zoologique a reçus, principalement par les travaux philosophiques de M. Cuvier, travaux qui ont réagi sur quelques parties de la Botanique elle-même, et dont je m'honore d'avoir profité; de l'autre, les importans travaux de M. Haüy sur les lois de la crystallisation, et notamment sur les décroissemens des rangées de molécules des cristaux, lois par lesquelles j'ai été conduit à quelques-unes des idées que j'exposerai dans le livre suivant." Cf. also 'Organographie végétale,' Paris, 1827, vol. ii. p. 237.

[2] 'Théorie élémentaire,' p. 89; also, p. 216.

[3] This principle is stated very clearly by Cuvier in many places—*e.g.*, in the celebrated "Discours" prefaced to the 'Recherches sur les Ossemens Fossiles' (3rd ed., 4to, 1825, vol. i. p. 47): "Tout être organisé forme un ensemble, un système unique et clos, dont les parties se correspondent mutuellement, et concourent à la même action definitive par une réaction

regularity and symmetry, and studies the causes which in the actual visible specimens of plant life veil and cover up this symmetry; as Haüy[1] had taught us in crystallography to recognise the primitive forms which appear changed by the phenomena of decrescence.[2] De Candolle accordingly enters very fully into the theory of abortive, degenerate, and coalesced forms, recurring again and again to the statement that the "*ensemble*" of nature tends to make one think "that all organised beings are regular in their most intimate structure, and that various and differently combined abortive efforts produce all the irregularities which strike our glance and embarrass our combinations."[3] And the morpho-

35. Regularity and symmetry.

réciproque. Aucune de ces parties ne peut changer sans que les autres changent aussi; et par conséquent chacune d'elles, prise séparément, indique et donne toutes les autres."

[1] Cf. 'Théor. élem.,' p. 116, where he draws a parallel between the two methods in crystallography represented by Romé de l'Isle and Haüy and similar methods in botany. He reverts to this frequently—*e.g.*, 'Organographie,' vol. ii. p. 237, where he says: "Le premier raisonnait comme ceux des botanistes qui voyaient une feuille ou une corolle comme un tout unique, entaillé sur ses bords par une cause inconnue; le second m'a servi de guide lorsque j'ai tenté de montrer que les découpures diverses des organes végétaux terraient essentiellement aux modes variés et aux degrés divers de leur agrégation."

[2] 'Théorie élémentaire,' p. 186: "Les avortemens, les soudures des parties, leurs dégénérescences, ne sont pas plus des suppositions de désordre ou d'imperfection dans les êtres organisés, que les décroissemens des molécules ne sont des désordres dans la cristallisation."

[3] 'Théorie élémentaire,' p. 97, &c.; also p. 236: "La vraie science de l'histoire naturelle générale consiste dans l'étude de la symétrie propre à chaque famille, et des rapports de ces familles entr'elles; toute la reste n'est qu'un échafaudage plus ou moins industrieux pour parvenir à ce but." And 'Organographie végétale, vol. i. p. x.: "L'organographie est la base commune de toutes les parties de la science des êtres organisés; considérée en ce qui tient à la symétrie des êtres, elle est le fondement de toute la théorie des classifications, &c." And again, ibid., vol. ii. 239: "Plus le nombre des êtres connus a augmenté, plus on les a étudiés avec soin, plus ou s'est convaincu de ce principe que j'ai été le premier, ou l'un des premiers à énoncer dans sa généralité, qu'il est presque certain que les êtres organisés sont symétriques ou réguliers lors qu'on les considère dans leur type, et que les irrégularités apparentes des végétaux tiennent à des phénomènes constans entre certaines limites, et susceptibles d'exister, soit séparément, soit réunis, tels

logical view is still more clearly expressed in the further analysis of their regularity and symmetry. The character of the structure is to be found in the existence or absence, in the relative or absolute position, number, size, and shape of the different organs,[1] whereas the use or functions of the organs, as well as their other sensible properties,[2] are considered to be, not the cause, but the consequence, of their structure, and hence of little importance in the anatomy, and of none in the classification, of plants, whatever may be their value from a physiological point of view. "But symmetry supposes a primitive plan or archetype, and the proofs of symmetry are those of a general order."[3] "The natural classification of organised beings consists in appreciating the modifying circumstances, and in abstracting them so as to discover the real symmetrical type of each group."[4] Here again De Candolle refers[5] to the examples of the crystallographer and the astronomer, who both make abstraction of the disturbing secondary influences in order to arrive at the primitive form and

que l'avortement ou la dégénérescence de certains organes, leur soudures entre eux ou avec d'autres, et leur multiplication d'après des lois régulières."

[1] 'Théorie élém.,' p. 147: "La symétrie organique se compose d'un certain nombre d'élémens dont les principaux sont: l'existence; la position relative ou absolue; le nombre relatif ou absolu; la grandeur relative ou absolue; la forme; l'usage; la durée; . . . les qualités sensibles," &c.

[2] Ibid., p. 170: "L'usage des organes est une conséquence de leur structure, et n'en est nullement la cause, comme certains écrivains irréfléchis semblent l'indiquer; l'usage, quelle que soit son importance dans l'étude physiologique des êtres, n'a donc eu lui-même qu'une médiocre importance dans l'anatomie, et ne peut en avoir aucune dans la taxonomie." . . . "Ce que je viens de dire de l'usage des organes, s'applique à bien plus forte raison encore à leurs qualités sensibles, qui ne sont que des conséquences plus ou moins directes de leur structure," &c.

[3] Ibid., p. 185. [4] Ibid., p. 188.

[5] See especially the chapter "De la Symétrie végétale" at the end of the 'Organographie,' vol. ii. p. 236 *sqq.*

the true orbit. It follows that "we must study the different species as constant things,"[1] and that this is a more "dignified" occupation for a "naturalist than the accumulation of doubtful cases in favour of the non-permanence of species."[2] He agrees with Cuvier in rejecting the older idea of the "échelle des êtres,"[3] and he praises the sagacity of Linnæus, who suggests that the vegetable kingdom resembles a geographical chart,[4] —an idea which, in the hands of several French and German botanists, has become a fruitful conception.

In De Candolle we meet with a repeated accentuation of the recurring symmetry of form, of the existence of definite primitive types, in the vegetable kingdom. Simultaneously with him there was labouring another thinker and keen observer of nature, who was primarily struck by the resemblance exhibited in the different parts or organs of one and the same plant, and searched for the type or plan on which they were modelled. He introduced into the vocabulary of scientific language the expression "metamorphosis of plants." It was Goethe the poet who, in 1790, published under this title his first contribution to morphological science. In subsequent publications and essays, covering the last forty

36. Goethe's metamorphosis.

[1] "Théorie élémentaire," p. 195.

[2] Ibid. [3] Ibid., p. 230.

[4] "Linné a le premier, avec sa sagacité ordinaire, comparé le règne végétal à une carte géographique; cette métaphore, indiquée dans son livre par un seul mot, a été développée ensuite par Giseke, Batsch, Bernardin de Saint-Pierre, L'Héritier, Petit-Thouars, &c. Et quoi qu'on ne doive la prendre que pour une simple image, cette image est tellement juste, tellement féconde en conséquences utiles, qu'il est peut-être convenable d'entrer dans quelques détails ultérieurs. Je suppose pour un moment cette carte exécutée; les classes répondent aux parties du monde, les familles aux royaumes, les tribus aux provinces, les genres aux cantons et les espéces aux villes ou villages," &c. (Théor. élém., p. 231).

years of his extraordinary life, he again and again reverts to the subject, which with him is only one chapter in the extensive science of morphology, of which he was indeed the first to form a general conception. Goethe's ideas hardly influenced the course of science, but in the history of thought they form a remarkable anticipation of later views, and have accordingly been frequently referred to by contemporary writers, notably by Haeckel and Huxley in their important works on Morphology and Evolution. Of the foremost scientific writers, De Candolle was almost the only one[1] who, during Goethe's lifetime, referred to his views with approbation; seeing in his theory of the metamorphosis of the leaf a truly admirable divination[2] of vegetable organisation. Saint - Hilaire's honourable mention of Goethe's morphological contributions to zoology came only just in time to be seen by Goethe himself.[3]

[1] See 'Organographie,' vol. i. p. 551: "Les parties de chaque rangée ou de chaque verticille sont susceptibles de se transformer dans la nature de la rangée qui la touche immédiatement. Ainsi l'on trouve des sépales changés en nature pétaloide (*Primula calcycanthema*), des pétales changés en étamines (*Capsella Bursa - pastoris*), des étamines changées en carpelles (*Magnolia fuscata*), ou bien l'inverse, savoir: des carpelles changées en étamines (*Euphorbia palustris*), des étamines changées en pétales (toutes les fleurs doubles), ou les pétales transformés en nature de calice (*Ranunculus abortivus*). M. Goethe a très-heureusement désigné la première de ces séries de transformations sous le nom de *Métamorphose ascendante* ou *directe*, et la seconde sous celle de *Métamorphose descendante* ou *inverse*."

[2] Ibid. vol. ii. p. 243: "C'est ainsi qu'en voyant la manière véritablement admirable dont M. Goethe, quoiqu' habituellement occupé d'idées si différentes, a comme deviné l'organisation végétale, on est bien tenté de croire qu'il l'a moins inventé qu'il n'a généralisé avec génie quelques faits partiels heureusement choisis." This was written in 1827.

[3] See Goethe's 'Werke' (Weimar edition, Abth. II. Bd. vii.), the review of "Principes de Philosophie Zoologique. Discutés en Mars 1830 au sein de l'académie royale des sciences par M. Geoffroy Saint-Hilaire, Paris, 1830," especially p. 181, and dated Sept. 1830. In 1831 Geoffroy says of the unity of organisation: "Elle est présentement acquise au domaine de l'esprit humain; et l'honneur d'un succès aussi mémorable appartient à Goethe." Quoted by Huxley in 'Life of Owen,' vol. ii. p. 291.

What did great harm to Goethe's correct anticipations was the fact that in optics he had unsuccessfully combated the generally accepted Newtonian theory of colours,[1] and that his morphological glimpses were taken up by Schelling and his school and incorporated in the fantastic speculations of the philosophy of nature. They shared the fate of this and passed into temporary oblivion.

The idea of the fixity of certain forms in nature, of the archetectonic modelling of her objects according to certain archetypes, which Cuvier had put forth as the result of extensive observation and inductive examination of living and fossil forms, which in De Candolle was connected with the conception of geometrical order, regularity, and symmetry, found in Goethe's mind an artistic sanction. "It is," as the historian of botany has remarked, "the idealistic conception of nature which looks upon the organic forms as continually recurring 37. The ideal type.

[1] A full discussion of Goethe's theory of colours will be found in two addresses of Helmholtz: the first, from the year 1853, was reprinted in the first volume of his often - quoted 'Vorträge und Reden'; the second was delivered nearly forty years later at the meeting of the Goethe Society at Weimar, in June 1892. In the latter Helmholtz significantly refers to the great revolution which in the interval had come over scientific thought through the general recognition of the principles of energy and of evolution. By the light of these we are better able to understand the shadowy but nevertheless truthful anticipations contained in Goethe's poetical and scientific writings. Helmholtz traces the errors of Goethe's colour - theory largely to the fact that he worked with imperfect apparatus and impure colours; that "he never had before his eyes perfectly purified homogeneous - coloured light, and hence would not believe in its existence. On this difficulty," Helmholtz continues, "of complete purification of the simple spectral colours, a man like Sir D. Brewster foundered, who was much more experienced and clever in optical experimenting than Goethe, and was equipped with the best instruments" (Goethe's 'Vorahnung kommender naturwissenshaftlicher Ideen,' by H. von Helmholtz, Berlin, 1892, p. 30). Cf. also Helmholtz's Memoir on Brewster's Analysis of Sunlight, 1852. Reprinted in Wissenschaftl. Abhandl., vol. ii.

imitations of eternal ideas in the sense of Plato, and which confounds these abstractions of the mind with the objective nature of real things."[1] Nevertheless, we must recognise that through the vague and poetical expositions of Goethe's writings there is to be seen the fruitful idea of the change, the instability, of forms, as an equally important side of reality.[2] In fact, Goethe oscillates in his half-formed theories between the ideal archetypes of Plato and the more recent conceptions of Darwin and Spencer, as is proved by the vivid, even passionate, interest which he took in the celebrated controversy of Cuvier and Saint-Hilaire in the French Academy of Sciences in the year 1830,—an incident which carries us into the midst of the ideas with which the following chapter will be occupied.

Before we take up those entirely different lines of observation and reasoning, we must note a great expansion and development of the study of the form of natural objects—of morphology—in two independent directions. One of these carried the study of forms into the larger dimensions of time and space, the past

[1] Sachs, 'Geschichte der Botanik,' p. 181.

[2] Of Goethe Huxley says ('Life of Owen,' vol. ii. p. 290): "On the face of the matter it is not obvious that the brilliant poet had less chance of doing good service in natural science than the dullest of dissectors and nomenclators. Indeed there was considerable reason, a hundred years ago, for thinking that an infusion of the artistic way of looking at things might tend to revivify the somewhat mummified body of technical zoology and botany. Great ideas were floating about; the artistic apprehension was needed to give these airy nothings a local habitation and a name; to convert vague suppositions into definite hypotheses. And I apprehend that it was just this service which Goethe rendered by writing his essays on the intermaxillary bone, on osteology generally, and on the metamorphosis of plants." A very full appreciation of Goethe's merit will be found in all the principal writings of Ernst Haeckel, notably in the fourth chapter of the first volume of the 'Natürliche Schöpfungsgeschichte,' 9th ed., Berlin, 1898.

of history and the morphological changes of the earth; the other carried it into those small dimensions where the unaided eye sees only sameness and repetition, but where the microscope reveals the hidden structure, the internal and minute forms, of which living matter is made up.

I have already pointed out how the great travellers of the second half of the eighteenth century—Banks, Pallas, and Humboldt—carried the study of nature beyond the narrow limits of the museum and the workroom into the larger area of nature, of the present and the past world. Camper in Holland, Hunter and Monro in this country, Blumenbach and Soemmering in Germany, Saussure in Geneva, towards the end of the eighteenth and the beginning of the nineteenth century had begun to unite these scattered discoveries and records into something like order and system. It was again 38. Palæontology.
the great merit of Cuvier[1] to publish a monumental

[1] Of the labours of other naturalists who preceded Cuvier, a very full account will be found in a posthumous work of Ducrotay de Blainville, edited by M. Pol Nicard and entitled 'Cuvier et Geoffroy Saint-Hilaire' (1890). The author, as is well known, was for some time a colleague and collaborator of Cuvier, with whom he fell out, partly from personal reasons, partly owing to the whole bent of his scientific researches, which was much more philosophical than that of Cuvier. He had a very great appreciation of Lamarck at a time when that speculative naturalist was unknown or treated with neglect, not to say with ridicule. The criticisms of De Blainville on Cuvier must be taken with caution; nevertheless his works and lectures had a great influence on the development of the more philosophical side of natural science in France, as many allusions of Auguste Comte, Flourens, Claude Bernard, &c., sufficiently prove. In the chapter on Palæontology in the work on Cuvier (p. 380, &c.), De Blainville does full justice to Camper, Blumenbach, Soemmering, and other Continental naturalists, with whose labours Cuvier, through his German education, was better acquainted than his French colleagues. There is also a significant remark of his on the fact that Cuvier was essentially a collector and dissector, a man of the museum and the library, not an outdoor naturalist (p. 241).

work on the subject and to found the science of palæontology. His researches in this subject were based upon the collection of fossil remains which had been begun by Daubenton for the natural history of Buffon, and which he arranged and largely increased; on the collection which Camper had made at Amsterdam; on descriptions which he procured from all the collectors of Europe; notably from Blumenbach; on his excavations together with Brogniart in the environs of Paris. As early as 1798 he announced his intention of collecting everything that was known on fossil remains in a great tableau—a plan which was not realised till 1812, when his many separate publications were united in the great work on the "Ossements fossiles," and was only completed by the greatly revised and augmented edition of 1821. This work is important in morphological science, not only because it contains many accurate and still highly valued descriptions of "extinct species," but also because, in its celebrated introduction[1] on the revolutions on the surface of our globe, it takes a comprehensive view of the changing aspects which succeeding ages, divided by great catastrophes characterised by distinct geological formations

[1] In this introduction (p. 52 of vol. i.) there is also to be found the celebrated passage in which Cuvier says that by the application of his principle of the "correlation of parts" he could, if he only possessed one well-preserved fragment of a bone, determine everything as certainly as if he possessed the whole animal—a statement on which De Blainville (*loc. cit.*, p. 417) has some very pertinent remarks: "Ce ne sont pas des anatomistes véritables comme l'étaient Hunter, Camper, Pallas, Vicq-d'Azyr, Blumenbach, Soemmering et Meckel qui se seraient ainsi avancés, et M. G. Cuvier aurait été bien embarassé lui-même, si on l'avait pris au mot, et cependant c'est cette assertion qui restera formulée dans la bouche des ignorants," &c. Cuvier by this method determined and classed more than 150 mammals (*loc. cit.*, p. 53). A more favourable view of Cuvier's work on fossil remains is taken by Huxley, 'Life of Owen,' vol. ii. p. 297.

and by the fossil remains of extinct organic creations, presented on the surface of our earth. "What is certain," says Cuvier at the close[1] of this celebrated discourse, "is that we are now at least in the middle of a fourth succession of terrestrial animals, and that after the age of reptiles, after that of the palæotheria, after that of the mastodons, the megatheria, there has come the age when the human race, supported by some domestic animals, peaceably rules and cultivates the earth, and that it is only in the countries formed since this epoch in the recent alluvial deposits, peat-bogs, and concretions, that we find in a fossil condition those bones which belong to animals known and now living." Such is the *résumé* of the ideas which had followed — nay, even tormented[2] — Cuvier during his researches into fossil remains, and which led him to the conclusion[3] "that it required great events to bring about the important differences which he recognised"—differences which the slow "influence of weather, or of climate, or of domestication," could not explain, but which required the violent action of sudden "catastrophes,"[4] which frequently "disturbed the life on this planet by frightful events,"[5] "broke off the thread of operations,"[6] "none of the present agencies of nature sufficing to produce her bygone works."[7]

[1] "Discours sur les révolutions de la surface du globe et sur les changemens qu'elles ont produits dans le règne animal," reprinted in the 3rd ed. of the 'Recherches sur les ossemens fossiles,' 1825, vol. i. p. 172.

[2] "Ces idées m'ont poursuivi, je dirai presque tourmenté, pendant que j'ai fait les recherches sur les os fossiles, dont j'ai donné depuis peu au public la collection, recherches qui n'embrassent qu'une si petite partie de ces phénomènes de l'avant-dernier âge de la terre, et qui cependant se lient à tous les autres d'une manière intime" ('Discours,' &c., p. 140).

[3] Ibid., p. 3. [4] Ibid., p. 8.

[5] Ibid., p. 9. [6] Ibid., p. 14.

[7] "Ainsi, nous le répétons, c'est en vain que l'on cherche, dans les

39. Cuvier's catastrophism.

These words, which embody a conception since appropriately termed "catastrophism," and which picture to the mind's eye a succession of morphological changes of the entire aspect of our globe, were written at a time when, in this country especially, through the labours of Hutton, an entirely opposite view was gradually preparing. With this we shall deal in another chapter. The Cuvierian conception of epochs in geology harmonised with that of distinct types of organic creation. These exhibit in space, as those do in time, certain definite and distinct morphological characters—*i.e.*, certain typical forms and structures on a vast or a small scale, around which the features of events and individuals seem to oscillate, and which permit us scientifically to classify, describe, and comprehend them. This conception gave the tone to a long line of researches on the Continent and in this country in geology as well as in natural history.

40. Study of analogies.

In the study of these typical forms and structures in which nature repeats herself, reverting again and again to them, but in every single case departing more or less from them; in the study of this order without monotony, this change without confusion, this variety of forms in which leading features are always recognisable,—the discovery of analogies played a very prominent part. Goethe's metamorphosis of plants is based upon the analogy of their different organs: before he published

forces qui agissent maintenant à la surface de la terre, des causes suffisantes pour produire les révolutions et les catastrophes dont son enveloppe nous montre les traces; et si l'on veut recourir aux forces extérieures constantes connues jusqu'à présent, l'on n'y trouve pas plus de ressources" (ibid., p. 20).

this first morphological fragment he had already—led by analogy—discovered the intermaxillary bone in the upper human jaw. Later he and Oken independently traced the analogy between the skull and the vertebral column in vertebrate animals, a view which was taken up by eminent anatomists, such as Meckel, Spix, and Geoffroy Saint-Hilaire.[1] The tendency which lay in these attempts, of which the metamorphosis of plants and the vertebral theory of the skull are only prominent examples, is one which was naturally provoked by the opposite tendency which anatomical studies had received through Linnæus and Cuvier. Goethe himself gives a clear explanation of its origin. In a remarkable passage in the history[2] of his botanical studies, he mentions Shakespeare, Spinoza, and Linnæus as the three masters who had led him to reflect on the great problems of art, of life, and of nature. Now, he says, the influence of Linnæus lay principally in the opposition which he provoked.

[1] A good account of the part which the vertebral theory of the skull played in comparative anatomy will be found in Whewell's History, vol. iii. p. 369, &c. But see against this Huxley in 'Life of Owen' (vol. ii. p. 304): "The hypothesis that the skull consists of modified vertebræ, advocated by Goethe and Oken, and the subject of many elaborate works, was so little reconcilable with the mode of its development that, as early as 1842, Vogt threw well-founded doubts upon it. 'All efforts to interpret the skull in this way,' said he, 'are vain.'"

[2] See the Weimar edition of his Scientific Works, vol. ii. The passage given in the text is from an earlier account contained in two numbers of the 'Morphologische Hefte' (1817), reprinted *loc. cit.*, p. 389, &c. How Goethe continually hovered between the theory of types and that of development is seen in the following passage (1831, W. W., vol. vi. p. 120): "Das Wechselhafte der Pflanzengestalten, dem ich längst auf seinem eigenthümlichen Gange gefolgt, erweckte nun bei mir immermehr die Vorstellung: die uns umgebenden Pflanzenformen seien nicht ursprünglich determinirt und festgestellt, ihnen sei viehmehr, bei einer eigensinnigen, generischen und specifischen Hartnäckigkeit, eine glückliche Mobilität und Biegsamkeit verliehen, um in so viele Bedingungen, die über dem Erdkreis auf sie einwirken, sich zu fügen und darnach bilden und umbilden zu können."

"For as I tried to take up his sharp and suggestive distinctions, his expressive, useful, but frequently arbitrary laws, there arose in me an inner conflict: what he tried forcibly to hold asunder, tended according to the innermost demands of my nature to be united." And as the process of dividing, classifying, and keeping apart went on among the successors of Linnæus, so it must have produced in many genuine observers of nature a tendency similar to that which Goethe describes. They would emphasise the resemblances and analogies of natural objects and their organs in proportion as the classifiers had separated and distinguished them. And it was just as likely that the artistic mind of Goethe might succeed in "lifting the veil of nature," as Humboldt[1] put it, when he transmitted to Goethe his suggestive work on the geography of plants, and as Huxley[2] repeated in 1894. Indeed it was the former who, on the largest scale, had traced those analogies and correspondences in nature which are so much dearer

[1] See Goethe's own account (in Werke, 2 Abth., vol. vi. p. 163): "Sollte jedoch meine Eitelkeit einigermassen gekränkt sein, dass man weder bei Blumen, Minern, noch Knöchelchen meiner weiter gedenken mag, so kann ich mich an der wohlthätigen Theilnahme eines höchst geschätzten Freundes genugsam erholen. Die deutsche Uebersetzung seiner Ideen zu einer Geographie der Pflanzen nebst einem Naturgemälde der Tropenländer sendet mir Alexander von Humboldt mit einem schmeichelhaften Bilde, wodurch er andeutet, dass es der Poesie wohl auch gelingen könne den Schleier der Natur aufzuheben; und wenn er es zugesteht, wer wird es leugnen?"

[2] See quotation *supra*, p. 246 note; also ('Life of Owen, vol. ii. p. 288): "The cultivator of botany, who went beyond the classification of 'hay,' became familiar with facts of the same order. Indeed, flowering plants fairly thrust morphological ideas upon the observer. Flowers are the primers of the morphologist; those who run may read in them uniformity of type amidst endless diversity, singleness of plan with complex multiplicity of detail. As a musician might say, every natural group of flowering plants is a sort of visible fugue wandering about a central theme which is never forsaken, however it may, momentarily, cease to be apparent."

to the poetical mind of Goethe, and all other artists, than the separations and classifications of the men of science. "It is one of Humboldt's uncontested merits that he, in order to prove the unity which rules in the formation of the earth, searched for analogies in the geological constitution of distant countries. As we see him pointing out numerous novel coincidences between the formations of Mexico and Hungary, so likewise we owe to him suggestive hints for other similar comparisons."[1] But the man in whose labours the tendency of thought which was uncritically followed by Goethe, and magnificently represented in Humboldt, found the clearest scientific expression, so far as animated nature is concerned, was Étienne Geoffroy Saint-Hilaire, 41. Geoffroy Saint-Hilaire.
the friend and colleague and then the great rival of Cuvier.[2] No one recognised more clearly the deeper significance of the great outburst of the two conflicting ways of viewing nature in the Paris Academy of Sciences in 1830 than Goethe himself, who in the eighty-first year of his life was deeply stirred by seeing his favourite ideas espoused by a scientific authority of the first order.[3]

[1] See Julius Ewald in the third volume of the 'Leben Humboldt's' by Bruhns (German edition), p. 184.

[2] See Huxley in 'Life of Owen,' vol. ii. p. 293.

[3] Eckermann in the 'Conversations with Goethe' gives the following remarkable account, under date 2nd August 1830: "The news of the outbreak of the French Revolution arrived to-day, and created excitement everywhere. In the course of the afternoon I went to Goethe. 'Well,' he called out to me, 'what do you think of this great event? The volcano has come to an eruption, everything is in flames, and it is no longer a discussion with closed doors.' 'A dreadful affair,' I replied. 'But what else could one expect under the well-known circumstances and with such a ministry, but that it would end with the expulsion of the Royal Family?' 'We do not seem to understand each other, my friend,' retorted Goethe. 'I am in nowise speaking of those people; I am concerned with quite different things. I speak of that most important conflict which has come

Similarly the aged Gauss, twenty-four years later, listened with emotion when Riemann, in his celebrated dissertation, touched a string that had been vibrating in the master's soul for fifty years, unheard or unheeded by any other thinker.[1] We can best understand the two ways of reasoning in natural objects, which found an expression in the controversy between Cuvier and Saint-Hilaire, if we read the account which Goethe himself subsequently published in a Berlin periodical: "Cuvier labours untiringly as a distinguisher, describing accurately what lies before him, and thus attains a command over a great breadth of facts. Geoffroy Saint-Hilaire, on the contrary, is silently exercised about the analogies of living creatures and their mysterious relations."[2] The two men had worked as colleagues for thirty-eight years, Cuvier continuing and defining more clearly the classifying work of Linnæus, who, for example, had thrown all non-vertebrate animals into one class. This led him

to pass in the Academy between Cuvier and Geoffroy Saint-Hilaire, and which is of such importance to science.' This utterance of Goethe was so unexpected to me that I did not know what to say, and that for some minutes I experienced a complete cessation of my thoughts. 'The matter is of the greatest importance,' continued Goethe, 'and you have no idea what I feel concerning the news of the 19th July. We now have a mighty ally permanently in Geoffroy. But I also see from it how great is the interest of the scientific world in France in this matter, as, in spite of the frightful political excitement, the meeting took place in a crowded house. What is best is, that the synthetic treatment of nature, introduced by Geoffroy in France, cannot again go back. . . . I have for fifty years laboured in this cause; first alone, then supported, and at last, to my great delight, excelled by congenial minds. . . . This event is for me of incredible value, and I rejoice rightly over the ultimate general victory of the cause to which I have dedicated my life, and which also is essentially my own.'"

[1] On this incident see the prefatory notice in Riemann's 'Mathematische Werke,' ed. Weber, Leipzig, 1875, p. 517; also the 13th chapter of this volume.

[2] Goethe in the 'Berliner Jahrbücher für Wissenschaftliche Kritik,' vol. ii., 1830, September, reprinted in Werke II. vol. vii. p. 167 *sqq.*

finally in 1817 to establish the four great classes—the vertebrate, the molluscous, the articulate, and radiated types—in the animal kingdom. His colleague had contributed much to Cuvier's work, but had been increasingly struck by what he termed the "unity of organic composition," which he evermore looked upon as a key[1] to the comprehension of nature: he searched for one plan or type where Cuvier saw four types. In 1818 he published his principle in a celebrated work with the title, 'Théorie des Analogies, ou de Philosophie Anatomique.'[2] It has been correctly stated that he only gives more precise expression to a truth known to Aristotle and proclaimed by Buffon, that the mystery of organisation consists in "unity of plan combined with 42. Cuvier and Geoffroy.
variety of composition." Cuvier emphasised and studied the latter, his colleague the former. For an intimate knowledge and description of natural objects the work of distinguishing is all important; for a comprehension of nature the connection of things, the unity of plan, the filiation and relations of beings, the mutability of species, will ever be the more important and fascinating. The former was a purely scientific, the latter a philo-

[1] See Goethe's detailed Report, *loc. cit.*, Werke II. vol. vii. p. 173. A very full account of this celebrated controversy is also given in the posthumous work of Ducrotay de Blainville, 'Cuvier et Geoffroy Saint-Hilaire, Biographies scientifiques,' ed. Nicard, Paris, 1890, pp. 357-378, which is specially interesting, because Geoffroy's ideas were there traced to Lamarck (p. 351), of whom Goethe takes no notice.

[2] See the "Éloge Historique d'Etienne Geoffroy Saint-Hilaire," par P. Flourens, in the third volume of his 'Recueil des Éloges,' &c., Paris, 1862, pp. 229-281. He quotes, *inter alia*, a passage from Vicq-d'Azyr: "La nature semble opérer toujours d'après un modèle primitif et général dont elle ne s'écarte qu' à regret, et dont on rencontre partout des traces. . . . On observe partout ces deux charactères que la nature semble avoir imprimés à tous les êtres, celui de la constance dans le type et celui de la variété dans les modifications," &c. (p. 276).

sophical, task. Both thinkers were right, but only partially right, as Huxley has clearly shown;[1] but it was natural that Cuvier's position should for a long time be regarded as the stronger; since he had shown how, by detailed research, to increase enormously the stock of actual knowledge about the things of nature; whereas the uncritical and only half practical suggestions of Gœthe had undergone in the wild speculations of Schelling, Steffens, and Oken a development that frightened off men of exact thought. Cuvier saw the necessity of crying halt to these vague dreams which he had the merit of opposing, for the lasting benefit of true science, with the full force of his great authority.[2]

As in France and Germany so also in England, the tendency to distinguish minutely, to describe, to classify, and in doing so to fill the museums with new specimens,

[1] 'Life of Owen,' vol. ii. p. 296: "The irony of history is nowhere more apparent than in science. Here we see the men over whose minds the coming events of the world of biology cast their shadows, doing their best to spoil their case in stating it; while the man who represented sound scientific method is doing his best to stay the inevitable progress of thought and bolster up antiquated traditions. The progress of knowledge during the last seventy years enables us to see that neither Geoffroy nor Cuvier was altogether right nor altogether wrong; and that they were meant to hunt in couples instead of pulling against one another."

[2] As to Cuvier's own wavering on the great question of the fixity of species, see Huxley, *loc. cit.*, p. 294: "During the earlier part of his career, I doubt if Cuvier would have categorically denied any of Geoffroy's fundamental theses. And even in his later years Sir Charles Lyell, many years ago, gave me reasons for the opinion that Cuvier was by no means confident about the fixity of species. There was never any lack of the scientific imagination about the great anatomist; and the charge of indifference to general ideas, sometimes brought against him, is stupidly unjust." And further, p. 295: "In later life, however, Cuvier seems to have become so much disgusted by the vagaries of the *Naturphilosophie* school, and to have been so strongly impressed by the evil which was accruing to science from their example, that he was provoked into forsaking his former wise and judicious critical attitude; and in his turn he advocated hypotheses which were none the better than those of his opponents."

and to discover and arrange systematically unknown and extinct species, got the upper hand for a long time. No one has done better work in this large field than Richard Owen, who has been termed with some propriety the British Cuvier. But in following the lines and filling up the schedules which Cuvier had prepared, Owen and other[1] contemporary workers in the same field have also had the great merit of bringing the Cuvierian view to the point where it clearly leads on to another and more comprehensive view of nature. In the first place, it happened that in finding and describing the remains of extinct animals, increasing difficulty was experienced[2] in deciding to which of the great existing groups of animals they should be assigned. There arose the necessity of interpolating species between groups which we now look upon as widely separated. The necessity arose of forming the conception of what is now termed the "inter-

43. Richard Owen.

[1] Huxley, *loc. cit.*, p. 310: "Unless it be in the 'Ossements fossiles,' I do not know where one is to look for contributions to palæontology more varied, more numerous, and, on the whole, more accurate, than those which Owen poured forth in rapid succession between 1837 and 1888. Yet there was no lack of strong contemporaries at work in the same field. De Blainville's 'Ostéographie'; Louis Agassiz's monumental work on fossil fishes, achieved under the pressure of great obstacles and full of brilliant suggestions; Von Meyer's long series of wonderfully accurate memoirs, with their admirable illustrations executed by his own hands, all belong to Owen's generation."

[2] See on this Carus, 'Geschichte der Zoologie,' p. 648, and Huxley, *loc. cit.*, p. 309, where reference is made to Owen's memoir "on an extinct mammal discovered in South America by Darwin in 1833, which Owen named *Taxodon Platensis*. It is worthy of notice that in the title of this memoir there follow, after the name of the species, the words 'referable by its dentition to the Rodentia, but with affinities to the Pachydermata and the herbivorous Cetacea'; indicating the importance in the mind of the writer of the fact that, like Cuvier's *Anoplotherium* and *Palæotherium*, *Taxodon* occupied a position between groups which, in existing nature, are now widely separated. The existence of one more 'intercalary' type was established."

calary type." Especially through palæontological finds, the landmarks were gradually removed which separated the distinct species and groups of organised beings. It had happened to Cuvier only in single instances that he had to record resemblances between widely separated groups. Such resemblances became more and more frequent and perplexing. In the second place, Owen had the great merit of giving more definite expression to the conception of analogies, as developed principally by the school which Cuvier opposed. In fact, he revised and brought into general use the term "homology," which had already been used by French and German anatomists before him.[1] This term signified

44. Study of homology.

correspondence of parts or organs based not so much on external likeness as on similarity of origin. By admitting the latter conception, the idea of origin, the rigidity of the purely structural classification was lost. Morphology became the science, not of fixed, but of flowing forms and structures. It is remarkable that Owen, in following up this line of reasoning, was pre-eminently attracted to the oracular writings of Oken, whose influence his great forerunner Cuvier had combated with all his

[1] Great importance has been attached to the term "homology," which, to a reader uninitiated in the complicated and changing vocabulary of the natural sciences, presents not a little difficulty. It is a good example of the classical saying of Goethe, "dass wo Begriffe fehlen, da stellt ein Wort zu guter Zeit sich ein." In the attempt to define the current term "homology," in seeking for numerous examples of homologies as distinguished from analogies, naturalists were led to the recognition of real, not only of verbal or logical distinctions. In this respect it is most instructive to read Owen's treatise 'On the Archetype and Homologies of the Vertebrate Skeleton' (1848), the enlarged reprint of a Report to the British Association in 1846. In it he gives a pretty full history of the term homology, which in the first half of the nineteenth century became current with special meanings in three independent sciences. With the precision of the usage, both in geometry and chemistry, the vagueness of the term as used by naturalists stands in characteristic contrast. "The corresponding parts," Sir R. Owen there says (p. 5), "in different animals being made namesakes, are called technically 'homologues.' The term is used by logicians as synonymous with 'homonyms,' and by geometricians as signifying 'the sides of similar figures which are opposite to equal and corresponding angles,' or to parts having the same proportions: it appears to have been first applied in anatomy by the philosophical cultivators of that science in Germany. Geoffroy Saint-Hilaire says, 'Les organes des sens sont *homologues*, comme s'exprimerait la philosophie Allemande; c'est-à-dire qu'ils sont analogues dans leur mode de développement, s'il existe véritablement en eux un même principe de formation, une tendance uniforme à se répéter, à se reproduire de la même façon.'" After remarking on the looseness of this definition, Owen proceeds to give his own, taken from the "Glossary" appended to the first volume of his 'Hunterian Lectures,' as follows: "'Analogue'—A part or organ in one animal which has the same function as another part or organ in a different animal." "'Homologue'—The same organ in different animals under every variety of form and function." He then goes on to distinguish "special," "general," and "serial" homology. For a history of thought the important point in all these discussions is that, besides the similarity of structure and the sameness of function, relations and points of comparison of a different kind were introduced; that these were, with more or less clearness, traced to development; and that through this the genetic view, the doctrine of descent, was prepared by those who, like Owen, were least ready to accept it when it appeared in a definite form. In the light of this new view, of which the next chapter will treat, the whole vocabulary of the older morphologists required recasting. These older views, which traced homology to the existence of definite types, models, or patterns possessing a purely ideal existence, have been termed Platonic, inasmuch as in the philosophy of Plato the existence of a world of ideal forms or archetypes served to explain whatever of order is found in the real world of separate things. "The term 'homology,'" says Prof. Ray Lankester, "belongs to the Platonic school, but is nevertheless used without hesitation by those who reject the views of that school. Prof. Owen . . . would understand by 'homologue' the same organ in different animals under every variety of form and function. . . . But how can the sameness of an organ under every variety of form and function be established or investigated? This is, and always has been, the stumbling-block in the study of homologies without the light of Evolutionism; for, to settle this question of sameness, an ideal 'type' of a group of organisms under study had to be evolved from the human mind, after study of the component members of the group; and then it could be asserted that organs might be said to be the 'same' in two animals which had a common representation in the ideal type" ('Annals and Mag. of Natural History,' 4th series, vol. vi., 1870, p. 34, &c.) See also Huxley in 'Life of Owen,' vol. ii. p. 303, &c.; and J. Arthur Thomson, 'The Science of Life,' p. 32 (1899).

might, and who "provided him with the subject-matter of his severest as well as of his most justifiable sarcasms."[1]

The great extension of the morphological or structural view of nature into distant time and space—into palæontology by Cuvier and Owen, into geography by Humboldt, Ritter, and others—*i.e.*, morphology on an extensive scale—led to an appreciation of the labours of a different class of students of nature, namely, those who—also on a large or a smaller scale—investigated the agencies which bring about and the laws which govern the change of forms. I have now to mention the last great contribution to the purely morphological view, I mean the cellular theory, which tended ultimately in a similar direction.

45. The cellular theory.

The earlier researches into the minute microscopic structure of organised beings—such as those of Malpighi and Grew in the seventeenth century—were conducted by persons who took an equal interest in animal and plant life.[2] But this class of research soon fell into the hands of specialists, with the result that anatomy, the science of animal structure, and phytotomy, that of vegetable structure, were conducted on different lines

[1] Huxley, 'Life of Owen,' vol. ii. p. 315.

[2] Carus ('Gesch. der Zoologie,' p. 395) mentions especially Malpighi (1628-1694) as an exception, inasmuch as he conducted his researches from a purely scientific interest, keeping them free from extraneous practical considerations. "In his anatomy of plants there are laid, moreover, the first foundations, more firmly established by all subsequent researches, of the doctrine of the composition of all organised bodies out of cells, which has given to the whole conception of the living creation a definite starting-point, and in the sequel a firm basis for the genetic view." See also on the same subject, and on the relation of structural and physiological researches in the seventeenth and eighteenth centuries, Sachs, 'Gesch. d. Botanik,' p. 351, &c.

and for different purposes. The fact that the organisation of the higher animals, which, for medical reasons, is more interesting, can be roughly divided into a variety of separate organs or systems of organs, each of which can be, to some extent, studied by itself as we study the parts and workings of a machine, and that for the physician greater interest attaches to the functions of these organs, placed anatomy for a long time under the influence of physiology, which is the science of the performance, not of the structure, of the parts of living creatures. Phytotomy, on the other side, was for a long time neglected, awaiting the greater perfection of the microscope. Thus it came about that down to nearly the middle of the century the morphological study of animals and that of plants were pursued without much mutual benefit or regard. The phytotomists of the seventeenth century had established the fact that plants are built up of minute parts called variously utricles, bladders, vesicles, but mostly cells, and which were compared with the structure of the foam of beer or the cells of a honeycomb.[1] Different forms were assigned to these cavities,

[1] Aug. Pyr. de Candolle begins his 'Organographie' (1827) with the words: "La nature intime des végétaux, vue aux plus forts microscopes, offre peu de diversités. Les plantes les plus disparates par leurs formes extérieures, se ressemblent à l'intérieur à un degré vraiment extraordinaire," &c.; and after going back to the observations of Malpighi and Grew, and referring to the recent ones of Mirbel, Link, Treviranus, Sprengel, Rudolphi Kieser, Dutrochet, and Amici, mentions Kieser's 'Mémoire sur l'Organisation des Plantes' (Harlem, 1812) as the only French book which contains an account of the phytotomic researches carried on by the Germans, who, after the lapse of a century, were the first to take up these studies again. In the second chapter De Candolle says: "Le tissu cellulaire, considéré en masse, est un tissu membraneux formé par un grand nombre de cellules ou de cavités closes de toutes parts; l'écume de la bière ou un rayon de miel en donnent une idée grossière mais assez exacte" (p. 11).

and it was also recognised that they were frequently elongated into tubes or joined so as to form larger vessels. In all these researches and descriptions paramount importance was attached to the form and composition of the framework of this cellular arrangement, and only little to its contents. In fact, the historian of botany[1] characterises the period from 1800 to 1840 as that of the study of the cellular framework of plants. The skeleton, as it were, of plant structure received primarily the greater attention. In the course of these researches, which, with a few important exceptions, were all carried out in Germany, one point was permanently settled, namely, that "the cell is the one fundamental
46. Hugo von Mohl. element of all vegetable structure."[2] No one did more to establish this important fact than Hugo von Mohl, whose name has been somewhat cast into the shade by the more attractive writings of Schleiden. It was Schleiden who first brought the new cellular theory into popular recognition, not without an admixture of errors, which had to be gradually eliminated in the various controversies with which his name is connected.

[1] See Sachs, *loc. cit.*, p. 276, &c. This period finds its consummation in the researches of Hugo von Mohl. It begins with those of Brisseau Mirbel, the first French author who took up this line. His labours were continued and criticised by a long list of German naturalists. Sachs also refers to the erroneous habit these earlier phytotomists had of getting their diagrams of what they saw by the microscope made by other persons who were supposed to be impartial—a custom fortunately abandoned by Mohl, who in his drawings did not give "undigested copies of the objects but his own impressions of them" (p. 281).

[2] Sachs assigns the final establishment of this principle to the year 1831, and considers it as one of Mohl's achievements, since, although it had been already announced by Sprengel and Mirbel, it had not been sufficiently supported by observations. Even the curious but antiquated idea, according to which the spiral fibre formed a fundamental part of plant structure, survived up to 1830 (p. 323).

But the highest value for a history of Thought attaches to this point for a different reason. In it the long-separated lines[1] of botanical and zoological study met again. Immediately after the appearance of Schleiden's 47. Schleiden and Schwann.
epoch-making publication—and partly in consequence of it—Theodor Schwann was induced to collect, in 1839, all the known observations, coming principally from the school of Johannes Müller, which referred to the existence and formation of animal cells, and to utilise them in the enunciation of his great generalisation, "that there is one universal principle of development for the elementary parts of organisms however different, and that this principle is the formation of cells."[2]

[1] The fourth decade of the century was also the period in which physical and chemical methods and ideas were—notably in France and Germany—made useful for anatomical and physiological research in zoology and botany. Sachs, however, significantly warns us against the view, which has since been frequently put forward in an exaggerated form, that the physiology of plants consists in nothing but applied physics and chemistry (*loc. cit.*, p. 393, &c.) That Schwann himself attached the greatest importance to this point can be seen from the preface to his principal work. This appeared in 1839, and was translated into English by Henry Smith, and published by the Sydenham Society in 1847 with the significant title, 'Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants.' The translator has also attached a rendering of Schleiden's 'Contributions to Phytogenesis,' which appeared first in Part II. of Müller's 'Archiv für Anatomie und Physiologie' in 1838, and was also translated in 'Taylor's Scientific Memoirs,' vol. ii. part 6.

[2] Schwann, *loc. cit.*, p. 165. A little farther on he adds the following generalisation, which it is well to read in the light of more recent researches: "A structureless substance is present in the first instance, which lies either around or in the interior of cells already existing; and cells are formed in it in accordance with certain laws, which cells become developed in various ways into the elementary parts of organisms." It is clear that the discovery of what may be called the morphological element or unit of organised structures in this view meant the end of pure morphology. The problem of the explanation of existing forms was handed over to the student of development, to the genetic view and conception of nature. The cellular theory, thus enunciated in its greatest generality by Schwann, has formed a kind of provisional resting-place in the study of the forms and changes of living nature; as Newton's gravitation formula has served as a provi-

Morphologically the microscopic examination of animal and vegetable tissues had thus led not to a clearer definition of the great differences which exist in the forms and structures of the larger and the full-grown organisms, but rather to a conviction of their intrinsic and essential sameness. These differences could not be explained in the purely morphological manner in which Haüy had shown how to trace the difference of crystalline forms to the shapes and configuration of the "molécules intégrantes." The diversity of forms had to be traced to

48. Transition to the study of development.

processes of growth or development—*i.e.*, the purely morphological examination led on to the developmental or genetic study of organic forms. And this was made still more evident when the microscopic examination revealed yet other and more important elements in the composition of organic structures, elements which were seemingly quite shapeless or amorphous. The skeleton, which had so long seemed to contain the key to the understanding of organic forms, the framework of the plant structure, the cell-walls and partitions, with all their geometric figures and arrangements, turned out to be of quite secondary importance compared with the cell contents, the substance called in animals by Dujardin sarcode, and in vegetables by Von Mohl protoplasm, and with the nucleus or cell-kernel, which had been discovered by Robert Brown.[1] Accordingly great interest attached

sional basis for physical astronomy. Both generalisations involve unsolved problems, with the difference that the formulation of the cellular theory is not as precise as that of gravitation.

[1] Both the discovery of the nucleus by Robert Brown and that of the cell contents by Dujardin preceded the enunciation of the cellular theory. Brown's discovery was referred to both by Schleiden and Schwann. In fact, Brown's researches were much better known and followed up in Germany than in England. His papers were translated into German by a number of botanists, and edited in five volumes between 1825 and 1834 by Nees von Essenbeck. He did not collect his original ideas into any great work or propound a new system of classification as did Jussieu and De Candolle, whom he equals in scientific importance; his valuable generalisations were given occasionally in his numerous monographs. Sachs considers him more advanced than the two great rivals just named, inasmuch as he had an appreciation of questions of development which they lacked ('Gesch. d. Botanik,' p. 121). Humboldt called him "botanicorum facile princeps," and succeeded in procuring for him, through his influence with Sir Robert Peel, a pension of £200 per annum.

to these amorphous[1] constituents, and chemical investigations as to their composition were added to the previous microscopic dissection. The purely morphological view

[1] The definition of a cell—*i.e.*, of the morphological or form-element of organised matter, as consisting of a membrane, a cell content, a nucleus, and a nucleolus—stood in contrast with Felix Dujardin's description, in 1835, of a living substance which he met with in his researches in lower animal life, and which he had called "sarcode." In the place of this name—the observation of Dujardin being little noticed—Von Mohl, after having for a time accepted the erroneous theory of Schleiden and Schwann as to cell-formation, introduced the term "protoplasma," which has been retained in science as the name of the elementary constituent of all living matter with very varying definitions, according to the different observations of animal or vegetable organisms and the increasing powers of the microscope; this having revealed structures where before only formless, amorphous substance had been observed. The history of these fluctuations of opinions and definitions can be read both in the older histories (Sachs, Carus) and the more recent accounts. Among these numerous expositions, see especially Yves Delage, 'L'Hérédité et les grands problèmes de la Biologie,' 1895, p. 19, &c.; O. Hertwig, 'The Cell,' translated from the German by H. J. Campbell, 1895; and the most recent work by Dr Val. Häcker, 'Praxis und Theorie der Zellen und Befruchtungslehre,' Jena, 1899, p. 10, &c. The cellular theory has gained enormously in importance and in popular esteem, as has also the study of all micro-organisms, through its application to medicine and hygiene. In 1847 Rudolph Virchow founded his celebrated "cellular pathology," combining the many beginnings of the cellular theory which had been laid by others, in his famous axiom "omnis cellula e cellula." He gave up the theory of the free formation of cells, proclaimed the doctrine of the genesis of cells—even pathological ones—by cell-division, and adopted Goodsir's theory of the uninterrupted filiation of the elements of all living matter, of the autonomous cells. As in general biology, so also in cellular pathology, the last fifty years have witnessed great controversies and many special theories, one of the chief difficulties having been to combine the doctrine of the autonomy or individuality of the cells with a correct view of their filiation and connected life. In spite of these many changes and modifications, the name of Schwann still stands at the opening of every treatise on funda-

had exhausted itself. The fundamental unity of the organisation of living beings had been proved; how was their actual diversity to be explained? This evidently required considerations of a very different kind. What they were we shall see in the next chapter. The position of the morphologist in the middle of the century had thus become one of considerable perplexity.[1] It may be compared to that of the organic chemist about the same time. The older ideas, around which, under the great influence of Cuvier and De Candolle in zoology and botany, of Werner and Humboldt in geology, the morphological classification and description of natural objects had clustered on the Continent, had become obsolete. The doctrine of definite types, of architectonic models, or of distinct ages of creation, separated by catastrophic changes, was becoming untenable; floras and faunas of entirely different appearance had been revealed in other countries and climates in the distant past,[2] or in the great newly-discovered realm of living

mental biology, and that of Virchow at the origin of modern pathology, as the greatest practical application of the cellular theory. An exceedingly good record of the different and changing views referring to the cell will be found in the chapter on "Cell and Protoplasm" in J. A. Thomson's 'Science of Life,' pp. 101-117.

[1] "On comprend aisément le découragement de Robin renonçant à édifier son 'Traité d'Anatomie générale,' après avoir tenté inutilement, dans sa 'Chimie anatomique,' de pénétrer le mécanisme des phénomènes moléculaires s'accomplissant dans les corps organisés. La morphologie, pourtant, n'avait pas dit son dernier mot, et la barrière bio-chimique était moins rapprochée que le ne croyaient les disciples de Comte et de De Blainville" (Herrmann, article "Cellule" in 'La Grande Encyclopédie,' vol. ix. p. 1060).

[2] Owen, in the very instructive "General Conclusions" to the third volume of the 'Anatomy of Vertebrates' (1868), clearly points out how the position of Cuvier has been made untenable by these discoveries: "As my observations and comparisons accumulated, with *pari passu* tests of observed phenomena of osteogeny, they enforced a reconsideration of Cuvier's conclusions to which I had previously yielded assent" (p. 188). "Accordingly these results of extensive,

forms only accessible to the microscope. The metamorphosis of the different organs in the plant had been suggested by Wolf, and more fully demonstrated by Goethe. Unity of organisation had been proclaimed by Saint-Hilaire and De Blainville, and the ultimate identity of the elementary structure of animals and plants had been demonstrated by Schleiden and Schwann. How was the evident relationship of the different types of living beings to be explained? It is interesting to note how the very terms which were then used implied the explanation, though this was only apparent to one or two natural philosophers who were then secretly at work. The word "affinity," which in chemistry has for ages been 49. Affinity.
used to denote, without explaining, the mystery of combinations and separations of different substances, had been imported into philosophical anatomy to denote the deeper structural likeness between animals which at the first glance appeared to belong to different classes. This word ordinarily implies blood-relationship, and might have

patient, and unbiassed inductive research—or, if there were a bias, it was towards Cuvier—swayed with me in rejecting the principle of direct or miraculous creation, and in recognising a 'natural law or secondary cause as operative in the production of species in orderly succession and progression' (1849)" (p. 789). . . . "Each successive parcel of geological truth has tended to dissipate the belief in the unusually sudden and violent nature of the changes recognisable in the earth's surface. In specially directing my attention to this moot point, whilst engaged in investigations of fossil remains, and in the reconstruction of the species to which they belonged, I was at length led to recognise one cause of extinction as being due to defeat 'in the contest which, as a living organised whole, the individual of each species had to maintain against the surrounding agencies which might militate against its existence'" (p. 797). Through this passage, quoted by Owen from the preface (1866) of the same work, a controversy arose, it being taken by a reviewer to prove the admission of the Darwinian theory. There followed an explanation by Owen, rejecting natural selection and the admitted contest as explanations of the origin of species.

suggested the theory of descent: it was used by those who most strongly repudiated such a doctrine.[1]

In the absence of any satisfactory explanation of the continual recurrence of certain definite forms in nature, and the presence of an evident relationship and a clear indication of metamorphosis in single instances, it was natural that morphologists of the first order, such as Owen, and other authorities in science, such as Whewell in England and Alexander Braun in Germany, should have recourse to older views and vague philosophical theories. Owen in 1848 spoke of a specific organising principle which "moulds in subserviency to the exigencies of the resulting specific forms," argues that the knowledge of such a being as man must "have existed before man appeared, for the divine mind which planned the archetype also foreknew all its modifications," and concludes that we learn from the past history of our globe that "nature has advanced with slow and stately steps, guided by the archetypal light, amidst the wreck of worlds, from the first embodiment of the vertebrate idea under its old ichthyic vestment until it became arrayed in the glorious garb of the human form."[2]

[1] Huxley in 'Life of R. Owen,' vol. ii. p. 302.

[2] See Owen's treatise 'On the Nature of Limbs,' 1849, pp. 85, 86. In the essay 'On the Archetype and Homologies of the Vertebrate Skeleton,' he concludes with the following remarks: "Now, besides the ἰδέα, organising principle, vital property, or force, which produces the diversity of form belonging to living bodies of the same materials, which diversity cannot be explained by any known properties of matter, there appears also to be in counter-operation during the building up of such bodies the polarising force pervading all space, and to the operation of which force, or mode of force, the similarity of forms, the repetition of parts, the signs of the unity of organisation may be mainly ascribed. The Platonic ἰδέα or specific organising principle or force would seem to be in antagonism with the general polarising force, and to subdue and

Whewell, in various passages of his 'History' and of his 'Philosophy of the Inductive Sciences,' argues that the explanation of organic forms is to be found in the study of the functions which each organ is destined to perform, and brings morphology back under the guidance of physiology, from which De Candolle and others had only recently liberated it.[1] Alexander Braun, the great German botanist, wrote about the same time: "Although the organism in its growth is subject to physical conditions, the real causes of its morphological and biological speciality lie, nevertheless, not in these conditions: its laws belong to a higher grade of development of reality, to a sphere in which the capacity for spontaneous self-determination becomes evident."[2] Even Johannes Müller,

mould it in subserviency to the exigencies of the resulting specific form" (p. 172). Huxley attributes these theoretical views of Owen to the influence of Lorenz Oken, the principal scientific representative of the school of the "Naturphilosophie." In this respect Owen left the direction of study initiated and so successfully followed by Cuvier. In fact, though opposed to Darwinism, Owen did not, like Cuvier, believe in special creation, as is clearly shown in a passage frequently quoted, taken from the conclusion to the third volume of Owen's great work 'On the Anatomy of Vertebrates' (1868), p. 807: "So, being unable to accept the volitional hypothesis, or that of impulse from within, or the selective force exerted by outward circumstances, I deem an innate tendency to deviate from parental type, operating through periods of adequate duration, to be the most probable nature, or way of operation, of the secondary law, whereby species have been derived one from another."

[1] De Candolle is very clear on this point; he says ('Théorie élémentaire,' p. 170): "L'usage des organes est une conséquence de leur structure, et n'en est nullement la cause, comme certains écrivains irréfléchis semblent l'indiquer; l'usage, quelque soit son importance dans l'étude physiologique des êtres, n'a donc en lui-même qu'une médiocre importance dans l'anatomie, et ne peut en avoir aucune dans la taxonomie; quelquefois seulement on peut s'en servir comme d'un indice de certaines structures à nous encore inconnues; ainsi lorsque je vois la surface unie d'un pétale suinter une liqueur, j'en conclus que cette partie est glandulaire, et je l'assimile aux nectaires; mais cette assimilation, bien que reconnue par l'identité de l'usage, est réellement établie sur l'identité présumée de la structure."

[2] Quoted by Sachs ('Gesch. d. Botanik,' p. 188).

who did more than any other naturalist to base zoology, anatomy, and physiology on the foundation of the exact sciences, physics and chemistry, "assumed the existence of a vital force which, differing from physical and chemical forces, enters into conflict with them, and which in organisms acts the part of a supreme regulator of all phenomena according to a definite plan."[1]

50. Insufficiency of the morphological view.

The insufficiency of a purely morphological description of living beings, the unsuccessful search for the morphological elements out of which organisms are built up, as crystals are formed out of the *molécules intégrantes* of Haüy, led thinkers (up to the middle of the century) to have recourse to older and vaguer conceptions, which, under the name of archetypes, formative influences, vital forces, &c., were destined to help where the purely mechanical view would not suffice. This dilemma was appropriately described somewhat later by one who had—earlier, perhaps, than any other thinker—emancipated himself from the influence of these fanciful conceptions. Herbert Spencer in his 'Principles of Biology,' published in 1863, expresses it in the following words:[2]—

"If we accept the word 'polarity' as a name for the force by which inorganic units are aggregated into

[1] See Du Bois-Reymond, "Gedächtnissrede auf Johannes Müller" ('Reden,' vol. ii. p. 217).

[2] The 'Principles of Biology,' from which this extract is quoted, appeared in successive instalments, beginning in January 1863. It is well to note that this was before the appearance of Haeckel's 'Generelle Morphologie,' which bears the date 1866. It does not appear that Spencer has had any influence on German science, though no doubt many of the conceptions put forward in the numerous treatises of German biologists are anticipated in Spencer's 'Biology,' notably in his conception of the physiological units as intermediate between compound chemical molecules and crystals on the one side, and cells on the other. In the exhaustive

a form peculiar to them, we may apply this word to the analogous force displayed by organic units. But polarity is but a name for something of which we are ignorant. Nevertheless, in default of another word we must employ this. . . . It will be well to ask what these units are which possess the property of arranging

review of these theories, given by M. Yves Delage, a very prominent position is accordingly assigned to Herbert Spencer's biological writings. In fact, he says ('L'Hérédité,' p. 424 note): "Ici"—*i.e.*, in the 'Principles of Biology'—"est montrée, pour la première fois et avec une lucidité parfaite, l'utilité de concevoir des particules spéciales, éléments primitifs de la substance vivante, intermédiaries aux molécules et aux cellules. Les très nombreux auteurs qui ont utilisé la même idée n'en ont créé que des variantes. Spencer est le vrai père de la conception initiale, si féconde comme on le verra." And again (ibid., p. 836): "Brusquement, avec H. Spencer, on tombe en plein moderne. Ici plus de théories vieillottes, plus de procédés surannés. . . . Les phénomènes sont décomposés en leurs éléments avec une puissance d'abstraction qu'aucun philosophe n'a dépassée, des principes généraux sont déduits qui servent à leur tour à juger, à interpréter les phénomènes, à les ramener à leurs causes vraies. Comme résultat de ses méditations, Spencer nous offre les 'Unités physiologiques,' particules matérielles toutes identiques dans une même espèce d'êtres avec lesquelles il croit que l'organisme doit pouvoir se construire de lui-même, par le seul jeu de leurs forces moléculaires. . . . Il a . . . ouvert une voie: sa théorie est un des bras principaux du Delta de ce fleuve qui nous servait de terme de comparaison." The other great arm of the Delta is Darwin's theory of Pangenesis, on which see *infra*, chapter xii. of this volume. Of others, such as Erlsberg, Haeckel, His, Haacke, M. Delage says: "Ils ont réussi seulement à montrer qu'en substituant aux forces polaires des 'Unités physiologiques,' des formes de mouvement ou des propriétés géométriques, on n'arrive pas à un meilleur résultat." Prof. Haeckel in his 'Generelle Morphologie' (1866) has interpolated a special investigation, as it were, between the morphology of living things and the corresponding science of inorganic or purely physical (such as crystalline and chemical) structures and arrangement under the name "Promorphology," investigating with much ingenuity all manner of symmetrical, axial, radial, &c., configurations. J. Arthur Thomson ('Science of Life,' p. 34) remarks that little attention has been paid to this subject since, but, as stated above (p. 223 note), the systematic treatment of crystallography has all through the century appeared to biologists as an enticing and seductive model, and M. Yves Delage's great work gives many examples of this tendency—see, *e.g.*, his remarks on the theories of Haacke, Cope, Nägeli, Erlsberg, and many others, pp. 304, 315, 424, 441, 451, 459, 475, 495, 502, 593, 743, &c.

themselves into the special structures of the organism to which they belong. . . . On the one hand, it cannot be in these proximate chemical compounds composing organic bodies that this specific polarity dwells; . . . the occurrence of such endlessly varied forms would be inexplicable. On the other hand, this property cannot reside in what may be roughly distinguished as the *morphological units*. The germ of every organism is a microscopic cell, or a structureless blastema which nevertheless exhibits vital activities. . . . If, then, this organic polarity can be possessed neither by the chemical units nor the morphological units, we must conceive it as possessed by certain intermediate units which we may term *physiological*. . . . We must conclude that in each case some slight difference of composition in these units . . . produces a difference in the form which the aggregate of them assumes."

51. Herbert Spencer's "physiological units."

Now, there are only two ways open to the purely scientific thinker by which he can reach these intermediate structures lying between the mathematical forms of crystals or the molecular arrangement of atoms, and the visible but apparently structureless forms of cells and protoplasm. One of these is the still more advanced analysis of these microscopic structures by still greater powers of magnifying instruments; the other is the mathematical method of calculating from simple beginnings the complex forms of equilibrium which atoms or molecules are capable of assuming under the action of known forces. It appears unlikely that the powers of the microscope can be much further extended; and the mathematical calculation of even the simplest configur-

ations of attracting and repelling centres, or of linked vortex rings, is already so formidable that much cannot be expected in that direction. These intermediate units, vastly more complex than the most complex chemical molecules, and vastly more minute than the smallest visible grain of protoplasm, must therefore for a long time to come lie in the region of hypothesis, unattainable for the eye or the calculus; an indication rather than a real guide for our scientific researches. Seeing, then, that the study of forms — the morphological view of natural objects in the case of organic beings, where to the naïve contemplation of things these forms seemed full of so much significance, indicative of so much meaning, possessed of so much beauty and striking suggestiveness —has led to no comprehension of the essence of vital phenomena, and hardly even afforded a safe criterion for classification, it is intelligible how the scientific interest has moved away from the consideration of the fixed forms and structures to that of the variation and continued change of these forms. This alteration in the scientific way of looking at the actual forms of nature, goes hand in hand with the tendency we had occasion to notice when dealing with the abstract sciences. Many things which once seemed at rest, or possessed of very simple rectilinear motion, have revealed themselves to the mind's eye as complex states of motion. Colours are exceedingly minute and rapid but well defined vibrations; the dead pressure of gases is the impact of numberless quickly-moving particles; and the wonderful properties of the whirling vortex ring have made us familiar with what has been termed the dynamical or moving equilibrium, the

52. Change of scientific interests.

semblance of apparent rest produced by very rapid rotary motion. Rest and fixity of form seem only to exist apparently or for transient moments in the history of natural events; and even the finished and recurring structures of living beings, which appear to our eyes to be possessed of so much finality and sometimes of so much finish, owe these qualities only to the comparatively short space of time during which we are permitted to gaze at them, and to our ignorance of the slow but endless changes to which they are nevertheless subject.

53. The morphological period.

The period from 1800 to 1860 can be termed the morphological period of natural science. It succeeded the period of the simpler natural history, which had been mainly occupied with classification and description of specimens. During the morphological period the knowledge of the existing things and forms of nature was not only largely extended by excursions into distant lands and periods of history, but forms were also studied *in situ*, and the living things visited in their habitats. A deeper knowledge of the connection and interdependence of natural things and events was thus gained, and the relations and resemblances, the analogies and homologies, of the various forms were impressed on the observer. Besides all this, the microscope revealed the innermost composition and the ultimate structural sameness of living matter, adding moreover the knowledge of an enormous creation which remains hidden to the unarmed eye of the ordinary observer. The morphological view also took note of the relatedness and apparent recurrence of definite forms called types, of the so-called fixity of species and the succeeding characteristic periods of creation, and

sought to explain these morphologically: *i.e.*, it sought in the abstract study of forms—sometimes geometrical, sometimes artistic—the key to an understanding of the recurrence as well as the continued variation of definite types. The relationship was mostly looked upon as ideal, not real. How a gradual change came over this view of nature, how the study of development led on to the modern phase of natural science which is governed by the genetic view, I shall try to show in the next chapter.

CHAPTER IX.

ON THE GENETIC VIEW OF NATURE.

1. Statics and dynamics of living forms.

WHILST the great influence of such leaders in scientific thought as Cuvier, De Candolle, and Humboldt on the Continent, and of Richard Owen in this country, was mainly exerted in spreading the morphological view of nature, describing on a large scale or in minuter detail the typical recurring forms which natural objects or natural scenery present to the eye of the unbiassed observer, another school of naturalists was secretly busy in following up the changes to which all the things of nature seem continually subjected. They were as much impressed with this restless movement of everything as the others were with the continual recurrence of certain definite forms—be they geometrical or artistic. The general ideas which underlay their researches were not new,—they were probably older and more familiar [1] than

[1] Cosmogonies of all sorts abound in almost every literature, ancient or modern, whereas Cosmography, accurate, painstaking, and reliable, is of comparatively recent date. The first attempt to give a purely descriptive picture of nature as a whole, beginning with the larger features of the universe and ascending through terrestrial, inanimate and animate, phenomena to the central and crowning phenomenon of human life, was A. von Humboldt's 'Kosmos'; and it is interesting to note how averse the author was to introduce genetic expositions. In fact, it has been truly remarked that Humboldt's influence went to

the types and epochs of the other and dominant school; but they were difficult to grasp, being not unfrequently fantastic compromises between the legends of religious tradition and the beginnings of scientific thought. For a long time they evaded the endeavour to put them into

encourage purely morphological and to discourage genetic considerations. Accordingly the many beginnings of a scientific account of the origin and historical development of the things around us, of which Lyell gave the first fairly accurate summary in the first volume of his 'Principles of Geology' (1st ed., 1830), were hardly noticed in the 'Kosmos' (vol. i., 1845, vol. ii., 1847). None of the celebrated cosmogonical hypotheses, which we shall deal with in this chapter,—neither the 'Protogæa' of Leibniz nor the 'Époques de la Nature' of Buffon, neither Kant's nor Laplace's nebular theory, nor even the brilliant introduction to the 'Ossemens fossiles' of Cuvier, though the latter, and still more Laplace, must have had a great personal influence on him,—receive any adequate attention in the pages of 'Kosmos.' They are rarely referred to, and then only as works of imaginative value, for which the true scientific groundwork, extensive observation, and especially the experiences and results of travel, are wanting. Humboldt, whose mind was stored with these riches in an abundance and variety unequalled before or since, limited himself to a portraiture, to a panoramic and morphological, to a structural and architectonic view of things, with which he combined a deep sense of the reaction which the contemplation of nature must have on the artistic faculty. (See the Introduction to the second, the most brilliant, volume of 'Kosmos.') Genetic theories were to his mind premature and foreign to his purpose. "The mysterious and unsolved problems of development do not belong to the empirical region of objective observation, to the description of the developed, the actual state of our planet. The description of the universe, soberly confined to reality, remains averse to the obscure beginnings of a history of organic life, not from modesty, but from the nature of its objects and its limits" ('Kosmos,' vol. i. p. 367). "The world of forms, I repeat, can in the enumeration of space relations only be pictured as something actual, as something existing in nature; not as a subject of an intellectual process of reasoning on already known causal connections. . . . They are facts of nature, resulting from the conflict of many, to us, unknown conditions of active push-and-pull forces. With unsatisfied curiosity we approach here the dark region of development. We have here to do, in the proper sense of the frequently misused word, with world-events, with cosmical processes of immeasurable periods. . . . The present form of things and the precise numerical determination of relations has not hitherto succeeded in leading us to a knowledge of states traversed, to a clear insight into the conditions under which they originated. These conditions are not therefore to be termed accidental, as man calls everything that he cannot explain genetically" (vol. iii. p. 431).

exact language. It is only in the second half of the nineteenth century that the many independent lines of reasoning, the fragments of the great doctrine of development, have been united together, that the search after the principles or laws which govern the restless change has been rewarded by a certain number of definite results, and that what was once vague, fanciful, and legendary has become a leading idea in all the natural sciences. As in other instances which we have had occasion to notice, so also in this case, the appearance of clearer and more definite ideas has been heralded and helped by a novel mode of expression, by a new vocabulary. The word "evolution" has in this country done much to popularise this way of regarding natural objects and events: abroad, the word has not met with the same popular acceptance. It was known there and used in science and literature when it was yet unknown in this country, and has in consequence not been monopolised in the same way as in the English language, to denote the continuous and orderly development of states and forms of existence.[1] Moreover, it has been identified in this

2. "Evolution."

[1] On the older and modern use of the word "evolution" in the English language see Huxley's article in the 9th ed. of the 'Ency. Brit.' It is reprinted in his collected essays with the title "Evolution in Biology." According to Huxley, the term "evolution" was introduced in the former half of the eighteenth century in opposition to "epigenesis." The two terms denoted the two theories of the generation of living things, by development of pre-formed germs (pre-formation) or by successive differentiation of a relatively homogeneous rudiment (after-formation). Harvey, the expounder of the latter theory against Malpighi, who embraced the former, calls the first "metamorphosis." Leibniz, Bonnet, and latterly Haller, were "evolutionists" in the older sense of the word; Harvey, C. F. Wolf, and the modern school of embryologists, with von Baer as its most eminent representative, were adherents of the originally Aristotelian theory of "epigenesis." "Nevertheless," as Huxley says, "though the conceptions originally denoted by 'evolution' and 'development' were shown to be untenable, the words retained their application to the process by which the embryos of living beings gradually make their appearance; and the terms 'development,' 'Entwickelung,' and 'evolutio' are now indiscriminately used for the series of genetic changes exhibited by living beings, by writers who would emphatically deny that 'development' or 'Entwickelung' or 'evolutio,' in the sense in which these words were usually employed by Bonnet or by Haller, ever occurs." The word evolution has, however, acquired in the English language, mainly through the influence of Mr Spencer's writings, a much wider sense than evolution in biology implies: in fact, it takes the place of the German "Werden," a word much used in the philosophical writings influenced by the Hegelian doctrine, which indeed taught a logical or dialectic development of things, as Herbert Spencer and his school teach a mechanical development. There seem to be given to us by observation only two elementary processes of change, or of "Werden" (in Greek γίγνεσθαι, in French "devenir," in English "becoming," in Latin "fieri," in German also the synonym "geschehen"). These are, on the on one hand, the process of mechanical motion, and on the other hand the process of logical thought: the one being the movement of external things, ultimately of atoms, the other the spontaneous movement of what Hume called ideas. When the thinking mind fixes its attention on the "fieri" rather than the "esse" of things there are accordingly two clues available, the mental or the physical, the logical or the mechanical. Many times taken up in earlier ages, both have been consistently applied only in the nineteenth century, the latter by Herbert Spencer, the former fifty years earlier by Hegel, whose philosophy is fundamentally as much a logical as the former is a mechanical system of evolution. The narrower meaning of evolution in biology is usually given in French by the word "transformisme," in German by "Entwickelungslehre" or "Darwinismus." See on the general subject Prof. James Sully's able article on "Evolution" in the 9th ed. of the 'Ency. Brit.'

country with a special philosophical teaching, that of Mr Herbert Spencer, which, whilst in many points coinciding with scientific views of development, has some special and peculiar features which will occupy us further on in our survey of thought. Having sought therefore for a term which is to comprise all the contributions to scientific thought which deal with the change and development of natural objects and events, I propose to use the older word "genesis," and to call this view "the genetic view of nature": it is, in general, the view which seeks to give answer to the question,

3. "Genesis."

How have things come to be what they are? What is their history[1] in time?

The first great philosopher of modern times who seems to have approached the question of the genesis of the objects of nature in the modern scientific spirit was Leibniz, who, in composing his local history of the origin of the Guelphs and the antiquities of Brunswick, pushed his researches into prehistoric times and made use of the geological and mineralogical data supplied in the Harz forest and mountains to arrive at conjectures as to the past history of the earth. His ideas, based upon local facts and observations on stratification and fossil remains, were collected in a famous tract entitled 'Protogæa,' which during his lifetime was only known in abstract,[2] and was published in 1749, many

4. Leibniz's 'Protogæa.'

[1] Although the word "genesis," through its use in the Scriptures, has acquired the meaning of a narrative of the origin or beginning of things, this meaning is not necessarily implied in the word γίγνεσθαι, and the genetic view of nature, or things in general, may limit itself to the study of observable, actual change, renouncing altogether the question of origins. The German words, "werden" and "geschehen," are in this respect less ambiguous and less ambitious, and many philosophers may accordingly prefer "evolution" to "genesis."

[2] On the connection of Leibniz's genetic studies with his History of Brunswick, which expanded under his hands into the 'Annales imperii occidentis Brunsvicenses' (edited by Pertz in the first three volumes of 'Leibnizens Gesammelte Werke,' Hannover, 1843-47, 4 vols.), see the introduction by Scheidt to his complete edition of the 'Protogæa,' Göttingen, 1749 (reprinted in the second volume of Dutens' 'Leibnitii Opera Omnia,' 1768); the words of Leibniz himself in the 'Plan' of his History (quoted by Pertz, vol. i. p. xxiii): "Præmittetur his annalibus quædam dissertatio de antiquissimo harum regionum statu qui ante historicos ex naturæ vestigiis haberi potest"; the address of Ehrenberg, 'Ueber Leibnitzens Methode' (Berlin, 1845); the account in Guhrauer's 'Life of Leibniz' (1846, vol. i. p. 205, and an interesting note in the appendix). Fontenelle, who knew of the 'Protogæa' only by the abstract (ed. 1693) in the Leipsic 'Acta,' and from correspondence with Eckhardt, Leibniz's executor, says in his 'Eloge de Leibniz': "Il la [viz., the History] faisait précéder par une dissertation sur l'état de l'Allemagne, tel qu'il était avant toutes les histoires et qu'on pouvait le conjecturer par les monuments naturels qui en étaient restés; des eoquillages pétrifiés dans les terres, des pierres où se trouvent des empreintes de poissons ou de plantes qui ne sont point du pays, médailles incontestables du déluge," &c., &c. How very much Leibniz was—in this as in many other ideas—in advance of his age can be seen from his correspondence with the Swiss naturalist Scheuchzer of Zürich: "Merentur Alpes vestræ, si quis alius Europæ locus, hanc eruditi inquilini curam et cæteros montes utili exemplo præibunt, quem admodum magnitudine vincunt. . . . Germanorum nostrorum non ea est diligentia quam vellem: itaque Historias regionum naturales habemus nullas, cum Angli Scotique nobis egregiis exemplis præiverint" (quoted by Guhrauer in the note referred to). An interesting reference is made in § xvii. of the 'Protogæa' to the use of the microscope, then only recently invented, and largely used by Leuwenhoek in connection with the examination of the formation and crystals of the celebrated "Baumann cave": "Et velim microscopia ad inquisitionem adhiberi, quibus tantum præstitit sagax Leuwenhoekii diligentia, ut sæpe indigner humanæ ignaviæ, quæ aperire oculos, et in paratam scientia possessionem ingredi non dignatur." A very fair account of the contents of the 'Protogæa' is given in W. D. Conybeare's 'Report on the Progress . . . of Geological Science' in the first volume of Brit. Assoc. Reports, p. 366, &c.

years after his death. He conceived that both fire and water[1] had been at work in forming the surface of the earth, and suggested that similar examinations of other localities[2] would be required in order to arrive at general conclusions. Such were subsequently supplied by Werner, de Saussure, Pallas, Hutton, Cuvier, and William Smith, before the systematic exploration of the whole globe became in the nineteenth century one of the tasks of geological science. A few years after the publication of Leibniz's speculations, which pointed to an accumulation of local observations as the means of arriving at a history

[1] 'Protogæa,' § iv.: "Donec quiescentibus causis atque æquilibratis consistentior emergeret status rerum. Unde jam duplex origo intelligitur firmorum corporum; una, cum ab ignis fusione refrigescerent, altera cum reconcrescerent ex solutione aquarum. Neque igitur putandum est lapides ex sola esse fusione. Id enim potissimum de prima tantum massa ac terræ basi accipio."

[2] Ibid, § v.: "Hæc vero utcumque cum plausu forte dici possint de incunabilis nostri orbis, seminaque contineant scientiæ novæ, quam Geographiam naturalem appelles. . . . Et licet conspirent vestigia veteris mundi in præsenti facie rerum, tamen rectius omnia definient posteri, ubi curiositas mortalium eo processerit, ut per regiones procurrentia soli genera et strata describunt."

of the earth, another philosopher of the highest rank took an important step in the direction of the study of the genesis of things natural, on the largest scale. It was Immanuel Kant, the philosopher of Königsberg, who, stimulated by the perusal of the cosmical theories of Thomas Wright of Durham,[1] applied the principles of the Newtonian philosophy in a first attempt to trace out the great stages in the formation of a planetary system.

5. Kant's nebular theory.

[1] The work of Wright is not so rare as it is represented to be by foreign writers, as I picked up two copies from a second-hand catalogue several years ago. It is chiefly interesting as having induced Kant to venture on his genetic speculations, which appeared anonymously at Königsberg in 1855, and for a long time remained unknown. About the same time as Kant, the celebrated mathematician J. H. Lambert published his 'Cosmological Letters on the Structure of the Universe' (Augsburg, 1761), many ideas in which coincide with the later expositions of Herschel and Laplace, which were based on quite different considerations. The speculations of Wright, Lambert, and Herschel were what we may call morphological, whereas it is the merit of Kant and Laplace to have built upon the ideas as to the architecture of the universe a plausible theory of its genesis. A full account of Wright's suggestions, which were accompanied by very beautiful mezzotint engravings executed by himself, is given by Prof. R. A. Sampson of Durham in the 'Proceedings of the Society of Antiquaries' of Newcastle-upon-Tyne, vol. vii. p. 99.

Kant's theory has been dealt with by Helmholtz in his Königsberg address (1854), "Ueber die Wechselwirkung der Naturkräfte" ('Vorträge und Reden,' vol. i.), by Faye ('Sur l'Origine du Monde,' Paris, 1885, 2nd ed.), by C. Wolf ('Les Hypothèses Cosmogoniques,' Paris, 1886, which contains a translation of Kant's work), and by G. F. Becker (Amer. Journal of Science, 1898). It is, however, to be noted that recent writers on Astronomy are inclined to speak of the genetic theories of the universe very much in the same way as Humboldt treated them in his 'Kosmos,' which professedly excluded the historical aspect in favour of a purely descriptive treatment, recognising the many difficulties which stand in the way of a consistent elaboration of the "nebular hypothesis." See A. Berry's 'History of Astronomy' (1898), p. 409; R. Wolf, 'Handbuch der Astronomie' (vol. i., 1890), p. 594; G. H. Darwin, 'The Tides' (1898), p. 302; also J. Scheiner, 'Der Bau des Weltalls' (Leipzig, 1901). On the additional great support which has been given to a genetic conception in general in the second half of the nineteenth century by Thermodynamics and Spectrum Analysis I shall speak later on. The writings of M. Faye in France, and of Sir Norman Lockyer in this country, utilise to the fullest extent the arguments derivable from these sources, and mark a great contrast to the manner in which cosmological questions were treated by A. von Humboldt.

The speculations of Wright had been purely geometrical. He had drawn attention to the apparent unity of organisation in the stellar system, as established by the accumulation of stars in a certain belt, popularly called "the milky way." He also suggested that the whole system was moving in a certain direction. Kant pointed out the analogy with the solar system, in which, viewed from the centre, the planetary masses would likewise appear situated in a narrow belt, moving all in the same direction. From these data he proceeds to show how, taking for granted an initial movement and the action of gravitation, the formation of rings, like those of Saturn, can be explained; further, how these might be broken up and concentrated in satellites. In fact, he recognised how, under the influence of gravitation, the solar system might have been gradually formed out of matter which was previously scattered through the whole of that space which the system still occupies. Kant also descended somewhat further into detail, and proceeded to discuss the possible retardation of the earth's rotation through tidal friction.[1]

[1] The tract in which Kant develops his views on this subject was occasioned by a prize offered by the Berlin Academy in 1754 for an answer to the question whether the time of revolution of the earth had suffered any retardation, and if so, through what causes? Kant did not compete for the prize, deeming his reflections not capable of being sufficiently perfected to deserve to be submitted. So he simply published them in a local Königsberg paper, from which they were later reprinted in the collected works, forming one of the first of Kant's publications. At the end of this tract he announces his 'Cosmogonie' which appeared the following year with the title 'Natural History of the Heavens,' &c. Kant had the satisfaction of seeing many of his speculations verified by the subsequent discoveries of inductive research, notably through Sir William Herschel's observations of nebulæ; and the German edition of Herschel's great memoir 'On the Construction of the Heavens' ('Phil. Trans.,' 1784), which appeared in Königsberg in 1791, by Sommer, contains an extract from Kant's

The two lines of speculation, originated by Leibniz and Kant as to the genesis of things on this earth and in the universe, mark two distinct ways of approaching the genetic problem. They were both isolated, and it was not till well on in the course of our century that they were again taken up and independently developed—the one by geologists, the other by physical astronomers. They remained for a long time without mutual influence; till, within the last generation, they were brought together, their different results deduced, and a reconciliation attempted. To this I shall revert later on.

6. Laplace. Forty years after Kant, Laplace put forward his so-called nebular hypothesis at the end of the popular exposition which he gave of his mechanical theory of the heavens. He apparently knew nothing of Kant's attempt, and his views differ materially from those of Kant, in so much as he assumes in the rotating nebular mass an attracting nucleus from which, in the course of condensation through attraction, the planetary rings and bodies were thrown off as the centrifugal velocity balanced the attracting forces. For a long time this sketch of a possible genesis of the planetary system was paraded in popular

work. The merits of Kant have only been tardily recognised; they were unknown to Laplace, and only imperfectly known to more recent authorities, such as Helmholtz and Lord Kelvin, who were fully prepared to do him justice. Lord Kelvin, in his Rede Lecture of 1866, refers to Kant as the first to publish "any definite estimate of the possible amount of the diminution of rotatory velocity experienced by the earth through tidal friction" ('Pop. Lects. and Addr.,' vol. ii. p. 65), and in the controversy which took place between him and Huxley on "Geological time" the theories of Kant were frequently referred to. See his lecture on "Geological Time," 1868 (*loc. cit.*, p. 10, &c.); Huxley on "Geological Reform," 1869 (reprinted in 'Lay Sermons,' No. XI.) The best account in the English language of Kant's contributions to cosmogony will be found in an article by G. F. Becker in the 5th vol., 4th series, of the 'American Journal of Science,' 1898.

works on astronomy as an established theory, whereas Laplace himself had put it forward with great reserve, and only as a likely suggestion.[1] There is, however, no doubt that it powerfully influenced the minds of many students of nature in the direction of a genetic view of phenomena.

The attempts referred to so far can be described as belonging to the Romance of Science. I now come to the more solid contributions—to a real genetic theory of the things of nature. These are not much older than our century. They belong to two entirely independent lines of research which were followed up in England and on the Continent respectively—the former in palæontology, the latter in embryology. Although they were carried on quite independently of each other, they had this in common, that they both resorted to a study of life—as preserved in geological strata or as now existing around us—for a guide in comprehending the genesis of Things on a larger scale.

It may be well to remark here that the contemplation of the phenomena, the forms and the processes exhibited in the living portion of creation, has not always, and even not generally, in the course of history led to those theories which our age is elaborating, and which will in future times possibly be looked upon as one of its char-

[1] Laplace himself says: "Je présente cette origine du système planétaire avec la défiance que doit inspirer tout ce qui n'est point un résultat de l'observation et du calcul." The elaborate exposition of the architecture and system of the universe contained in A. von Humboldt's 'Kosmos,' which was professedly inspired by Laplace (see 'Kosmos,' vol. vi. p. 8), gives us little, if anything, about the history of the universe, professing to be only a "Weltgemälde" and not a "Welterklärung." The time for genetic theories had not yet come, and both Kant's and Laplace's cosmogonies are only casually referred to.

acteristic achievements—the genetic view. There is another view which a superficial glance at organic life, with its well known phases of birth, culmination, and decay, has frequently impressed upon the observer; there seemed another lesson to learn than that which our age is trying to master.

7. "Cyclical" view.

That other view can best be termed the "cyclical" view of things, the doctrine that every thing runs in a cycle[1] and repeats itself, that all change is periodic and recurrent, that there is nothing new under the sun.[2]

[1] Mr Thomas Whittaker has given me various references to the writings of ancient philosophers which bear on this subject. He finds the cyclical or recurrent aspect of the world-process prominently put forward by the Stoics. Zeller ('Philosophie der Griechen,' vol. iii. I. p. 136, &c., 2nd ed.) says in his account of the stoical philosophy: "Out of the original substance the separate things are developed according to an inner law. For inasmuch as the first principle, according to its definition, is the creative and formative power, the whole universe must grow out of it with the same necessity as the animal or the plant from the seed. The original fire—according to the Stoics and Heraclitus—first changes to 'air' or vapour, then to water; out of this a portion is precipitated as earth, another remains water, a third evaporates as atmospheric air, which again kindles the fire, and out of the changing mixture of these four elements there is formed—from the earth as centre—the world. . . . Through this separation of the elements there arises the contrast of the active and the passive principle: the soul of the world and its body. . . . But as this contrast came in time, so it is also destined to cease; the original substance gradually consumes the matter, which it segregated out of itself as its body, till at the end of this world-period a universal world-conflagration brings everything back again to the primæval condition. . . . But when everything has thus returned to the original unity, and the great world-year has run out, the formation of a new world begins again, which is so exactly like the former one that in it all single things, persons, and phenomena return exactly as before; and in this wise the history of the world and the deity . . . moves in an endless cycle through the same stages." Zeller, in a note to this passage, remarks that "the conception of changing world-periods is frequent in the oldest Greek philosophy; the Stoics found it first in Heraclitus. The further statement, however, that the succeeding worlds resemble one another down to the minutest detail, is to be found, to my knowledge, before Zeno only in the Pythagorean school . . . and is connected with the doctrine of metempsychosis and the world-year."

[2] Mr Whittaker quotes a passage from Aristotle's 'Metaphysics,' towards the end of the 12th book (Berlin ed., p. 1074, b. 10-12): "Κατὰ τὸ εἰκὸς πολλάκις εὑρημένης εἰ τὸ δυνατὸν ἑκάστης καὶ τέχνης καὶ φιλοσοφίας καὶ πάλιν φθειρομένων.

Poets and philosophers have repeated this theme in endless variations, probably without improving upon the classical and perfect expression which it has found in ancient[1] poetry and in the sacred writings. History has been written with the professed object of gaining, by analogy, an insight into the drift of modern or future events, and economic and political theories have been based upon the likelihood of a recurrence of what has happened before. Especially has the teaching been impressed upon us that the universal fate of all development is to lead to death and decay, and to make room for the endless repetition of the same recurring phases

Every art and every kind of philosophy having probably been found out many times up to the limits of what is possible and again destroyed;" and remarks, "This notion of cycles refers to human civilisation, not to the universe, which is one eternal system with a fixed central mass, and with its outer part in a moving equilibrium. Empedocles undoubtedly had a theory of recurrent cycles in the universe. The four elements,—which he first brought together as elements of the whole, early thinkers having taken one or other of them as a first principle from which the rest are evolved,—according to Empedocles, are necessarily aggregated and segregated by the predominance of principles which he calls love (φιλία) and hate (νεῖκος). The four periods are: 1. Predominant love (the σφαῖρος), a state of complete aggregation; 2. decreasing love and increasing hate or strife; 3. predominant strife (ἀκοσμία, complete separation of the elements); 4. decreasing strife and increasing love. These are cosmic periods. It has been supposed—Zeller takes this view—that we are living in the fourth cosmic period, the period of increasing love."

[1] The best known passage is that from the celebrated fourth eclogue of Virgil, where, after describing the return of the golden Saturnian age, the poet continues (vv. 31-36):—

"Pauca tamen suberunt priscæ vestigia fraudis,
Quæ tentare Thetim ratibus, quæ cingere muris
Oppida, quæ jubeant telluri infindere sulcos.
Alter erit tum Tiphys, et altera quæ vehat Argo
Delectos heroas: erunt etiam altera bella,
Atque iterum ad Trojam magnus mittetur Achilles."

Dugald Stewart ('Philos. Works,' vol. iii. p. 167) refers to this with the following quotation from Clavius's 'Commentary on the Treatise on the Sphere,' by Joannes Sacro Bosco: "Hoc intervallo, quidam volunt, omnia quæcumque in mundo sunt, eodem ordine esse reditura, quo nunc cernuntur," and he also attributes this theory of recurrence to an extreme application of the mathematical spirit (vol. iv. p. 207). How this idea of recurrent cycles fascinated and haunted Fr. Nietzsche see Seth's article, 'Contem. Rev.,' vol. 73, p. 734.

of existence.[1] This view was considerably strengthened by the popular interpretation of the teaching of modern astronomy, which laid great stress on the periodicity of the planetary movements, and the stability and inherent readjustment of the solar system. Also the insight gained by the first application of chemical knowledge to

[1] The idea of recurrent, periodic repetition seems opposed to the modern idea of progress and development as taught by Leibniz and Herder abroad, by Spencer in this country; still it seems almost impossible in a purely mechanical system to avoid introducing the conception of an ultimate recurrence, so long as one deals with finite space, time, or number, however great they may be. The only escape seems to be in assuming an infinite process or an immaterial principle which is not subject to mathematical treatment, the latter being inherently one of repetition. It is interesting to note how Herbert Spencer at the end of 'First Principles' relapses into the cyclical conception: "Thus we are led to the conclusion that the entire process of things, as displayed in the aggregate of the visible universe, is analogous to the entire process of things as displayed in the smallest aggregates. Motion as well as matter being fixed in quantity, it would seem that the change in the distribution of matter which motion effects, coming to a limit in whichever direction it is carried, the indestructible motion thereupon necessitates a reverse distribution. Apparently, the universally coexistent forces of attraction and repulsion, which necessitate rhythm in all minor changes throughout the universe, also necessitate rhythm in the totality of changes—alternate eras of evolution and dissolution. And thus there is suggested the conception of a past during which there have been successive evolutions analogous to that which is now going on; and a future during which successive other such evolutions may go on—ever the same in principle but never the same in concrete result" ('First Principles,' 1st ed., p. 536). The other great system of modern philosophy which aims at a reconciliation of the mechanical and spiritual aspects—the philosophy of Lotze—though it dwells less than Spencer's system on the genetic problem, gives a different view of cosmic development. "The series of cosmic periods cannot be a number of phases, in each of which the one purpose of the universe does in fact maintain itself: it must rather be a chain, each link of which is bound together with every other in the unity of one plan. The One can manifest itself in various forms only when such variety of forms is necessary for the expression of its meaning—in a definite order of succession only when this order corresponds to a craving for development in its nature. As we required that each section of the world's history should present a harmony of the elements firmly knit throughout, so we must now require that the successive order of these sections shall compose the unity of an onward advancing melody" ('Microcosmus,' Eng. transl. by Hamilton and Jones, Book IV. chap. 3).

physiology and agriculture in the school of Liebig, and the first chapters of meteorology, seemed to favour the idea that the elements and forces of nature were engaged in cyclic movements which return again and again in the same fashion. To the same cyclical view the doctrine of the fixity of species, as well as that of the repetition of various creations, lent further support; hence it continued up to the middle of our century[1] to be fre-

[1] In Germany Moleschott's 'Kreislauf des Lebens,' a popular exposition of the conceptions developed in the second quarter of the century through chemistry and embryology, represented adequately the cyclic conception of life and development in a catching phrase. Much later we find — *inter multa alia* — in Michael Foster's 'Text-book of Physiology' a concise description of the process in nature which has always served as a type for the cyclic conception: "When the animal kingdom is surveyed from a broad standpoint it becomes obvious that the ovum, or its correlative the spermatozoon, is the goal of an individual existence; that life is a cycle beginning in an ovum and coming round to an ovum again. . . . The animal body is in reality a vehicle for ova; and after the life of the parent has become potentially renewed in the offspring, the body remains as a cast-off envelope whose future is but to die." Another example may be found in Mohr's 'Geschichte der Erde,' where the circulation of different elements in nature is considered. The conception of periodic cycles has found poetical expression in Rückert's beautiful poem, "Chidher," which is evidently the poetical rendering of an Arabian legend quoted by Lyell ('Principles,' vol. i. p. 31):—

"Chidher, the ever youthful, spake:
I passed a city on my way,
A man in a garden fruit did break,
I asked how long the town here lay?
He spoke, and broke on as before,
'The town stands ever on this shore,
And will thus stand for evermore.'

And when five hundred years were gone
I came the same road as anon,
Then not a mark of the town I met.
A shepherd on the flute did play,
The cattle leaf and foliage ate.
I asked how long is the town away?
He spake, and piped on as before,
'One plant is green when the other's o'er,
This is my pasture for evermore.'

And when five hundred years were gone
I came the same road as anon,
Then did I find with waves a lake,
A man the net cast in the bay,
And when he paused from his heavy take,
I asked since when the lake here lay?
He spake, and laughed my question o'er,
'As long as the waves break as of yore
One fishes and fishes on this shore.'

And when five hundred years were gone
I came the same way as anon.
A wooded place I then did see,
And a hermit in a cell did stay;
He felled with an axe a mighty tree.
I asked since when the wood here lay?
He spake: 'The wood's a shelter for evermore,
I ever lived upon this floor,
And the trees will grow on as before.'

And when five hundred years were gone
I came the same way as anon,
But then I found a city filled
With markets' clamour shrill and gay.
I asked how long is the city built,
Where's wood and sea and shepherd's play?
They pondered not my question o'er
But cried: 'So was it long before,
And will go on for evermore.'
And when five hundred years are gone
I'll go the same way as anon."

quently put forward and popularly accepted. It is useful then to note that in the course of the second half of the century we were more and more growing out of the cyclical and realising the meaning of

8. Supplanted by genetic view.

the genetic[1] view of things natural. We have been taught in astronomy to inquire into the origin of our solar or any similar system and the conditions of its duration, to ask concerning the central heat of the sun whence it came and how long it will last—a question unknown to Laplace,—to consider the effects of tidal friction, to learn that all the movements in nature are irreversible as distinguished from completely reversible ones, which only exist in abstraction; and, finally, we are met with the doctrine of the immortality of the germplasma, an idea, the meaning and significance of which I shall have to explain later on. All these novel theories and views combine to impress upon us the general significance of the terms "genesis, evolution, development," the fact that everything in and around us, in spite of the seeming recurrence of smaller movements and phenomena, and of the periodicity of the minuter and elementary changes, is slowly, continuously, and inevitably tending in a definite direction, which is certainly not that of a cyclical recurrence.

9. Geology.

Leaving aside for a moment these more general views, which have been clarified in the course of our century, it is interesting to note how they gradually emerged in

[1] Perhaps it would be more correct to say that we were learning to consider the changes within the larger cycles, confining ourselves to the study of one branch only of the periodic or cyclical movement of things around us, that branch which we are pleased to call the ascending or progressive branch.

the teachings of the several natural philosophers who initiated the genetic conception of natural phenomena. One of the earliest who broke with the older and introduced the modern methods was James Hutton, who towards the end of the preceding century led that school in geology which is called after him, and which violently opposed the ideas introduced from the Continent. The controversy culminated in the wrangle of the Neptunists and Vulcanists, those who looked to the agency of water and those who upheld that of fire as the principal cause of geological change. This difference, which at the time impressed the popular mind, is hardly that by which, in a history of scientific thought,[1] this controversy has become important. Hutton's position is marked rather by his opposition to catastrophism, and by his doctrine that geological changes, such as the decay and reproduction of rocks, were going on with the utmost uniformity, being always in progress. This he opposed to the Wernerian view, which believed in the existence of certain "fundamental rocks," which were "triumphantly

[1] The great merits of James Hutton, his extensive and original geological studies, his opposition to catastrophism, were overlooked through the theoretical discussions and the unfortunate title of his book. The world had grown tired of 'Theories of the Earth' and the discussion of fundamental problems. A spirit of observation had set in; the Geological Society was formed, and theories were for the time discountenanced. (See vol. i. p. 290, note 1, of this 'History.') The attacks also of Kirwan and De Luc, which turned upon the stale argument that Hutton's ideas were opposed to the scriptural records, had their effect in circles in which everything connected with the revolution against Church and State was distasteful. As Huxley has told us, Hutton came before his time. To him belongs the merit of having initiated the line of research and reasoning which, through the brilliant labours of Charles Lyell a generation later, swept away the older geology, and prepared the way for the genetic study of nature on a large scale. (See the "Historical Sketch" in the first volume of Lyell's 'Principles of Geology,' and Huxley's address on "Geological Reform," 1869.)

appealed to if anybody ventured to doubt the possibility of our being able to carry back our researches to the creation of the present order of things."[1] Hutton destroyed these characters, which were considered by many as sacred, and declared that in the economy of the world he could find "no traces of a beginning nor signs of an end." And yet, as Lyell has shown, his principles were only imperfectly carried through, for though he maintained that "the strata which now compose our continents have once been beneath the sea, and were formed out of the waste of pre-existing continents,"[2] he imagined that when the decay of old continents had furnished the material for new ones these were upheaved by violent and paroxysmal convulsions. He therefore required "alternate periods of general disturbance and repose, and such he believed had been and would for ever be the courses of nature."[3] A strange mixture of the genetic and cyclical views of natural phenomena! Professor Huxley[4] has explained these seeming incon-
10. Hutton. sistencies in the theory of Hutton, whom, together with Sir Charles Lyell, he has described as having founded the "uniformitarian" school of geology, by the influence which the discoveries of physical astronomy, brought out at that time by Laplace and his contemporaries, had upon Hutton. Thus Hutton writes: "From seeing revolutions of the planets, it is concluded that there is a system by which they are intended to continue those revolutions. But if the succession of worlds

[1] See Lyell, 'Principles,' 3rd ed vol. i. pp. 90, 91.

[2] Ibid., p. 89.

[3] Lyell, p. 92.

[4] Huxley, on "Geological Reform," quotes largely from Hutton's 'Theory of the Earth' (1758) and Playfair's 'Illustrations of the Huttonian Theory' (1802).

is established in the system of nature, it is in vain to look for anything higher in the origin of the earth. The result, therefore, of this physical inquiry is, that we find no vestige of a beginning, no prospect of an end." The beginnings of the genetic view of geological phenomena, which in Hutton were still mingled with catastrophism, were further developed by Sir Charles Lyell in his celebrated 'Principles of Geology.' When 11. Lyell.
he entered upon his geological researches, which were conducted during his very extensive travels all over Europe, a new element had already been introduced into science, of which neither Hutton nor Werner had been able to avail themselves extensively. This was the identification of geological strata according to the fossil remains which were contained in them,—a realisation of the plan of work already dimly foreshadowed in Leibniz's 'Protogæa,' but nevertheless accepted even by Humboldt as only a doubtful indication.[1] This valuable branch of geological science had been started by William Smith in his 'Tabular View of the British Strata' in 1790, and further elaborated in his geological map of England (1815), which was the fruit of his own unaided labours, "for he had explored the whole country

[1] The Wernerian school are generally accused of having neglected the historical record afforded by fossil remains, and Humboldt, in his 'Essay on the Superposition of Rocks in both Hemispheres' (1823), says (Eng. transl., p. 52): "In the present age naturalists are no longer satisfied with vague and uncertain notions, and they have sagaciously observed that the greatest number of those fossils, buried in different formations, are not specifically the same; that many species which they have been enabled to examine with precision vary with the superposed rocks. . . . Ought we to conclude from this assemblage of facts that all the formations are characterised by particular species? that the fossil shells of the chalk, of the muschelkalk, of the Jura limestone, and of the Alpine limestone, all differ from each other? This would be, in my opinion, to carry the induction much too far."

on foot without the guidance of previous observers or the aid of fellow-labourers,"[1] and "had thus singly effected for the whole of England what many celebrated mineralogists had only accomplished for a small part of Germany in the course of half a century."[2] Simultaneously with Smith in England, Cuvier and Brongniart were exploring the Paris basin. Thus the three different nations of Europe with whom I am mainly concerned in this work furthered independently the main divisions of geological inquiry. "The systematic study of what may be called mineralogical geology had its origin in Germany, where Werner first described with precision the mineral character of rocks; the classification of the secondary formations belongs to England, where the labours of Smith were steadily directed to these objects; the foundation of the third branch, that relating to the tertiary formation, was laid in France by the splendid work of Cuvier and Brongniart."[3] To these words of Lyell we can now add that the theoretical explanations were first suggested, and the correct line of reasoning on this accumulated evidence initiated, by Sir Charles Lyell himself.

The key to the doctrines of Lyell was the study of existing causes — the attempt to show how the slow agencies which we now see at work in nature around us are sufficient to explain the successive changes[4]

[1] Lyell, 'Principles,' vol. i. p. 101.

[2] An expression of d'Aubuisson, quoted by Dr Fitton, 'Phil. Mag.,' vols. i. and ii., also 'Edin. Rev.,' Feb. 1818.

[3] See Lyell, *loc. cit.*, p. 103.

[4] Id. ibid., vol. iii. p. 273: "It is only by carefully considering the combined action of all the causes of change now in operation, whether in the animate or inanimate world, that we can hope to explain such complicated appearances as are exhibited in the general arrangement of mineral masses."

which the recognisable strata of the earth's crust with their fossil remains indicate as having occurred in former ages. It was an attempt to "reconcile the former and the present state of nature."[1] This was to break with the idea of great and general convulsions, to which the Continental school resorted in their explanations, and it also meant upsetting the vague notions which set a limit to the time[2] which should be allowed for the operations of natural causes. It is possible to admit that in both directions, in their uniformitarian explanation and in their geological time-reckoning, the new school frequently went too far, the indications of actual catastrophes and paroxysmal convulsions being to many observers quite unmistakable. On the other side, the arguments based upon physical astronomy, mechanics, and thermodynamics, which afford an independent basis for geological time-reckoning, were not yet elaborated,[3] or were deemed too crude[4] to be of value; and for a good while geologists were permitted

[1] Lyell, vol. i. p. 114.

[2] Id. ibid., p. 241: "When difficulties arise in interpreting the monuments of the past, I deem it more consistent with philosophical caution to refer them to our present ignorance of all the existing agents, or all their possible effects in an indefinite lapse of time, than to causes formerly in operation but which have ceased to act."

[3] See Lyell, vol. i. p. 154, &c., also vol. ii. p. 274: "It has long been a favourite conjecture that the whole of our planet was originally in a state of igneous fusion, and that the central parts still retain a great portion of their primitive heat. Some have imagined with the late Sir W. Herschel that the elementary matter of the earth may have been first in a gaseous state, resembling those nebulæ which we behold in the heavens, and which are of dimensions so vast that some of them would fill the orbits of the remotest planets of our system. . . . Without dwelling on such speculations which can never have any direct bearing on geology," &c.

[4] See Lyell, vol. i. p. 206, where he refers to "astronomical causes of fluctuations in climate," and to the calculations of Sir J. Herschel and the fact that "this matter is still under discussion," and that "MM. Fourier and Herschel have arrived at very different opinions."

to draw indefinitely on the great bank of time,[1] just as in former ages they had been quickly brought to book by existing prejudices.[2]

Whilst these contributions to the genetic view of nature on the large scale were being independently worked out, the sciences which deal with the minute and hidden phenomena of organic growth had made great progress in the same direction. Here a definite scheme of development was quite evident to the most casual observer. In these sciences indeed we have to do with what is called in the German language "the history of development" *par excellence*, a term which is inadequately rendered by "Embryology" in French and English. For

12. Embryology.

it is an error which has frequently and for long periods obscured the correcter view to assume that the changes and processes which characterise the development of embryonic or germ life are essentially different from those which exist in the larger and more complex adult organism. The abolition of the fundamental distinction between the processes of embryonic and of adult or full-

[1] Lyell, vol. iii. p. 358: "Confined notions in regard to the quantity of past time have tended more than any other prepossessions to retard the progress of geology, . . . and until we habituate ourselves to contemplate the possibility of an indefinite lapse of ages having been comprised within each of the more modern periods of the earth's history, we shall be in danger of forming most erroneous views in geology."

[2] One of the first to attack the uniformitarian doctrine in geology and to apply the principles of modern physical science to geological and cosmical questions in this country was Lord Kelvin. His influence belongs, however, mainly to the post-Darwinian period, and begins with his celebrated memoir 'On the Secular Cooling of the Earth' (Edin. Trans., 1862, reprinted in the 3rd vol. of 'Math. and Phys. Papers,' p. 295). See also the 2nd vol. of his 'Popular Lectures and Addresses.' According to the introductory statement in the former paper his doubts regarding the uniformitarian teaching began as early as 1844. I shall refer to these speculations at the end of this chapter.

grown life, the unification of thought on these matters, is quite as important in the history of science as the abolition of the supposed fundamental difference between animal and vegetable growth or between normal and abnormal (or pathological) development. The reduction of all these seemingly so different changes to the one great problem of cellular structure, cellular growth, and cellular division marks one of the greatest achievements of our century. "Our position with regard to the cell is similar to that of investigators towards the whole animal or vegetable body a hundred years ago, before the discovery of the cell theory."[1]

Anticipations of this generalisation, of the condensation of the whole problem of animal and vegetable embryology, of generation, growth, and organic development in the formula, "omnis cellula ex cellula," have indeed existed since the time of Harvey, who, in addition to the great discovery of the circulation of the blood, laid down the thesis, "omne vivum ex ovo."[2] The further correct

[1] See O. Hertwig, "The Cell," 'Outlines of General Anatomy and Physiology.' Transl. by Campbell, 1895, p. 11.

[2] One of the best expositions of Harvey's ideas is to be found in Huxley's article on "Evolution in Biology" in the ninth edition of the 'Encyclopædia Britannica.' He there also refers to Aristotle's opinions. "One of Harvey's prime objects is to defend and establish, on the basis of direct observation, the opinion already held by Aristotle, that in the higher animals at any rate the formation of the new organism by the process of generation takes place, not suddenly by simultaneous accretion of rudiments of all, or of the most important of the organs of the adult, nor by sudden metamorphosis of a formative substance into a miniature of the whole, which subsequently grows, but by epigenesis, or successive differentiation of a relatively homogeneous rudiment into the parts and structures which are characteristic of the adult." In the sequel of his exposition, after maintaining epigenesis or after-formation against evolution in the older sense or preformation, Huxley, however, makes a passing remark that "though the doctrine of epigenesis, as understood by Harvey, has definitely triumphed over the doctrine of evolution, . . . it is not impossible that, when the analysis of the process of develop-

generalisation which he ventured to put forward, that growth and development of the germ or embryo consisted in the addition or formation of new parts and

13. Epigenesis and evolution.

structures through division or differentiation, was, however, obscured and cast into the shade by the opposite doctrine, termed evolution, according to which every form or particle of organisation was minutely pre-formed in an invisible germ, and growth consisted merely in a process of enlargement, as a particle of "dry gelatine may be swelled up by the intussusception of water." The supporters of this doctrine, to which the celebrated names of Leibniz, Boerhaave, Haller, and Bonnet belonged, seemed unable to conceive of any force in nature which was capable of producing organisation, and were thus compelled to accept in some form or other the doctrine of the pre-existence of germs, a theory which has in modern times been revived under an altered form.

The real foundation of scientific embryology, of the study of the genesis of vegetable and animal organisms, is now pretty unanimously[1] traced to Caspar Friedrich

14. C. F. Wolff.

Wolff, whose 'Theoria generationis' appeared in 1759. His observations refer alike to plant and to animal life, and his distinct object was to refute the theory of evolu-

ment is carried still further, and the origin of the molecular components of the physically gross, though sensibly minute, bodies which we term germs is traced, the theory of development will approach more nearly to metamorphosis than to epigenesis. . . . The process, which in its superficial aspect is epigenesis, appears in essence to be evolution in the modified sense adopted in Bonnet's later writings; and development is merely the expansion of a potential organism or original pre-formation according to fixed laws."

[1] See J. A. Thomson, *loc. cit.*, p. 121. Yves Delage, 'L'Hérédité,' p. 357, note; and especially O. Hertwig, 'The Biological Problem of To-day,' transl. by P. C. Mitchell (Heinemann's Scientific Handbooks, 1896), p. 4, &c.

tion and replace it by the correcter doctrine of epigenesis —*i.e.*, of repeated or after-formation. Haller[1] thought very highly of this attack on his own view, but was not convinced by it; and although in botany Wolff's views on the cellular structure of plants were adopted in France by Mirbel, and those on metamorphosis were unknowingly reproduced by Goethe, his influence on embryology dates actually only from the year 1812, when Meckel translated one of his treatises and thus drew attention to his great merits. Wolff tried to refute the theory of evolution or pre-formation, supplanting it by that of epigenesis or after-formation, through actual observations of the development of germs in plants and animals in definite instances. In botany his views, after lying dormant for a long period, led ultimately to the famous cellular theory of Schleiden and Mohl. In zoology, shortly after Meckel's republication of his treatise in 1812, there were published the researches of Pander, who, in his treatise on the development of the chick, "gave a fuller and more exact view of the phenomena less clearly indicated by Wolff, and laid the foundation of the views of all subsequent embryologists."[2]

15. Pander and K. E. von Baer.

Pander was a Russian by birth, and so was his greater contemporary and friend, Karl Ernst von Baer,[3] a man

[1] As Prof. J. Arthur Thomson says ('Science of Life,' p. 120), "A single sentence, 'Es gibt kein Werden—there is no Becoming,' sufficiently indicates Haller's position."

[2] J. A. Thomson in article "Embryology" ('Ency. Brit.,' 9th ed., p. 165).

[3] The work of von Baer (1792-1876) remained for a long time unknown and unrecognised outside of Germany. Huxley made him known in this country by translating extracts from his principal writings for Taylor's 'Scientific Memoirs' in 1853, nearly thirty years after von Baer had begun the brilliant series of his researches. It can be said of him that he, even more than his forerunners, Pander and Döllinger, withdrew natural

who occupies a unique position in the history of natural science. He introduced the principle and aspect of development into the midst of those studies which, under the important but one-sided influence of Cuvier and his school, were in danger of being confined within the limits of morphology and comparative anatomy. Through a long series of most important embryological investigations, conducted during the years 1819-1837, he demon-

science from the spell under which it was kept for a long time in the West of Europe by the great authority of Cuvier. Geographically also, von Baer's activity was centered in Königsberg (where he was one of a brilliant company who made the University celebrated) and St Petersburg. Though a great admirer of Cuvier, whose biography he wrote, and an adherent of the doctrine of animal types, which he independently arrived at, he introduced three distinct lines of research into his scientific labours, to all three of which Cuvier was either foreign or distinctly averse — viz., microscopic research, study of embryological development, and the philosophical spirit of the "Naturphilosophie." He was not dazzled by the latter; but whilst avoiding its extravagances and premature generalisations which then flooded German science, he always appreciated the search for the connection and unity of all the things of nature which was characteristic of that school. Baer stood, historically and philosophically, in the middle between the extreme morphological and genetic views represented respectively by Cuvier before and by Darwin after him. Already in 1815, when studying under Döllinger at Würzburg, he was guided by the idea that "nature follows in her creations certain general themes (types), and that she varies these in the different species." Von Baer also combined the geographical and anthropological interest, so largely represented by Humboldt and Ritter, with his morphological and genetic studies. In fact, it is doubtful whether in any naturalist of the very first order the different interests which the nineteenth century inherited and created were more equally and impartially balanced than in him. The embryological researches of von Baer stimulated many ardent students in Germany, such as Purkinje, Rathtke, Bischoff, and it is mainly through them that this branch of science was cultivated and made generally known. The name of the distant originator thus became somewhat forgotten, so that in French science we do not find von Baer as frequently and appreciatively mentioned as he deserves. Ample information on von Baer's scientific and personal character can be found in later publications: foremost in his 'Autobiography,' published in 1865; in his 'Life,' by Stieda (1877); and in an elaborate work by Professor R. Stölzle, entitled 'K. E. von Baer und seine Weltanschauung' (Regensburg, 1897). This work contains very ample and useful references and extracts from Baer's writings and correspondence. Very important are also von Baer's miscellaneous writings and essays, which were published by Vieweg in Brunswick, in three parts (2nd ed., 1886).

strated in the completest manner the truth of epigenesis. In fact, he had recognised development as the "sole basis of zoological classification; while in France Cuvier and Geoffroy St Hilaire were embittering each other's lives with endless merely anatomical discussions and replications, and while in Germany the cautious study of nature was given up for the spinning of Natur-philosophies and other hypothetical cobwebs."[1]

The position which Karl Ernst von Baer occupies in the history of science and thought is in many respects interesting and unique. He lived early enough in the century to experience the full influence of Cuvier's authority, and lived long enough to witness the great change which Darwin's writings brought on in all the natural sciences; whereas his great contemporary, Johannes Müller, passed away before the name of Darwin was known outside of his own country. In unison with Müller, and yet in an independent manner, he effectually liberated German science from the undue influence of the speculative school. And he has, probably more than any other great naturalist, recognised the importance of the three aspects which a contemplation of natural objects forces upon us: the apparent or real fixity of certain forms (the morphological view), the continued and orderly change[2] of these forms (the genetic view), and the apparent or real existence of a

[1] Huxley in Taylor's 'Scientific Memoirs,' New Series, p. 176.

[2] Very important in this respect is a lecture delivered by von Baer in 1834, with the title 'Das allgemeinste Gesetz der Natur in aller Entwickelung (reprinted in the Brunswick edition, vol. i. p. 39 *sqq.*) "We must conclude that, so far as observations now give material for inferences, a transformation of certain original forms of animals in the succession of generations is very probable, but only to a limited extent (p. 60), a view which von Baer maintained to the end against extreme Darwinism (see p. 37).

design in this process of change (the teleological view). Though his own researches did so much to give prominence to the genetic view, to the conception of development, he retained and elaborated the doctrine of types; and though he effectually handled the modern methods of the mechanical or exact sciences, he realised the full importance of studying the things and processes of nature in their actual and living connection,[1] and not merely in the artificial isolation of the laboratory or the dissecting-room. And he never became an adherent of the doctrine so prevalent with many of the followers of Darwin, that the apparent purpose of forms and processes in organic nature could be mechanically explained. During the period of his greatest scientific activity he was little known outside of Russia and Germany; in England, Carpenter and Huxley alone drew attention to his embryological and genetic studies; but since the tide of Darwinism has somewhat subsided, or has ceased to be all-absorbing, it is to the writings of Baer that many naturalists revert. In fact they belong to the few books of this class written during the pre-Darwinian age that bear to be read and re-read with profit by those who take a philosophical and not merely a historical interest in the development of natural science.

16. Von Baer's comprehensive views.

Perhaps the fact that von Baer was as great in relation to the morphological as he was in relation to the genetic and the teleological conceptions of natural phenomena prevented him from producing that revolutionary impression on the minds

[1] See the introduction to the second part of his 'Entwickelungsgeschichte der Thiere' (Königsberg, 1837).

of his contemporaries which Darwin did, and for which he indeed largely prepared the way. Instead of opposing the genetic change and development of the forms of natural objects to their apparent fixity, he rather reconciled both views with each other by maintaining[1] "that in order to obtain a just insight into the mutual affinities of animals it is before all things necessary to distinguish the different *types of organisation* from the different *grades of development*." He considered that[2] "the idea of animal organisation does not vary at equal intervals, but is realised in certain principal forms which again break up into variations of a lower grade"; and he[3] "arrived at the four principal divisions of the animal kingdom established by Cuvier." In 1828, in his work on the 'Development of Animals,' he discusses[4] "the prevalent notion that the embryo of higher animals passes through the permanent forms of the lower animals"—*i.e.*, "the doctrine of the agreement of individual metamorphosis with the ideal metamorphosis of the whole animal kingdom." Von Baer had himself added greatly[5] to

[1] See Huxley's translation, *loc. cit.*, p. 178.

[2] Ibid., p. 182.

[3] Ibid., p. 183.

[4] See K. E. von Baer's 'Ueber Entwickelungsgeschichte der Thiere Beobachtung und Reflexion,' Königsberg, 1828. The above extracts are taken from the fifth scholion: "Ueber das Verhältniss der Formen, die das Individuum in den verschiedenen Stufen seiner Entwickelung annimmt." See also Huxley's Translation, *loc. cit.*, pp. 186, 189.

[5] Prof. J. A. Thomson summarises as follows von Baer's own results: "It was von Baer who first clearly discriminated the great events in a life-history; (*a*) the primary process of egg-cleavage, and the establishment of the germinal layers; (*b*) the gradual differentiation of the tissues (histogenesis); and (*c*) the blocking out of the organs (organogenesis), and the shape-taking of the entire organism (morphogenesis) ('Science of Life,' p. 123). The classical work of von Baer is dedicated to his friend Pander, from whom and Döllinger he acknowledges having received the first impulses towards

the existing knowledge of the early development of the germs of animals by discovering the ovum in the body of the mammalia before fructification, and by this and other discoveries secured his claim to be considered the greatest embryologist of his own age, and perhaps of all time. He goes on to examine to what extent the morphological differences which the animal kingdom exhibits in its various members can be taken as a guide to the genetic differences in the growth and development of the higher organisms. He, in fact, tried to ascertain how far the facts of classification throw a light on the facts of development, how far the changing embryo of the higher animal gradually passes through the permanent forms of the lower animals. He combats the idea that the classification or morphological arrangement can be uni-serial—*i.e.*, brought into one continuous line or order.

his researches. He wishes to distinguish carefully between facts and theory, and is very cautious as to the latter, a trait which runs through all his writings. It is also very interesting to see how in his biography of Cuvier (posthumously published by Stieda) he considers it a merit of that great naturalist not to have indulged in genetic theories. "It is evident that Cuvier in his youth had also a genetic system in view, such as Oken afterwards followed up, but that he must soon have found out that this task was unattainable for him. He abandoned it, and sought rather to draw from the manifoldness of the formed product inferences regarding the conditions of its genesis. Thus he arrived at the teleological conceptions which he developed on various occasions. German naturalists drew from all this, especially in the age of Schelling's 'Natur-philosophie,' the conclusion that Cuvier was not a philosophical mind. To me it seems that we recognise in it Cuvier's desire for clearness. He dropped the higher task because he found that it would not lead him to clear views" ('Lebensgeschichte Cuvier's von K. E. von Baer,' ed. Stieda, 1897, p. 72). English readers, to whom the genetic view has only become familiar since Darwin or perhaps Lyell, will find with astonishment how in the writings of Baer, before Lyell and even before the appearance of Cuvier's final system, genetic ideas were thought to be prevalent, and were criticised elaborately and received with the utmost caution even by the great propounders of the doctrine of development.

Animals differ according to the type of organisation to which they belong. Thus the "embryo of the vertebrate animal is from the very first a vertebrate animal, and at no time agrees with an invertebrate animal."[1] Having, however, once fixed the existence of special organic forms, he asks whether within the limits of such form no law can be discovered to formulate the development of the individual. He believes there can,[2] and he proceeds to explain it in terms which for the most part might appear unaltered in the most modern work on evolution. He states that the more special type is developed from the more general, "and that the more different two animal forms are, so much the further back must their development be traced to find them similar." Indeed he thinks it probable that "in the condition of the actual germ all embryos which are developed from true ova agree," and he anticipates the cellular theory of Schwann, established by observation ten years later, by suggesting that the simple vesicle is the common fundamental form "from which all animals are developed, not only ideally but actually and historically."[3] In further examining the process of development, von Baer introduces the very suggestive term[4] *differentiation.* "The higher and lower development of the animal coincides perfectly with that histological and morphological differentiation which gradually arises in the course of the development of the individual."[5] Development, in fact, is the estab-

[1] *Loc. cit.*, p. 220 ; transl., p. 210.
[2] Ibid, p. 221.
[3] *Loc. cit.*, p. 224 ; transl., p. 213. On this anticipation see, however, von Baer's later explanation in 'Reden, &c.,' vol. ii. p. 250.
[4] The German term is "Sonderung," which Huxley renders by the English term "Differentiation."
[5] *Loc. cit.*, p. 229, 230 ; transl., p. 219.

lishing of differences, and in reality "the embryo never passes through the form of any other animal, but only through the condition of indifference between its own form and others." And he sums up his reflections by stating that the "development of an individual of a certain animal form is determined by two conditions: first, by a progressive development of the animal by increasing histological and morphological differentiation; secondly, by the metamorphosis of a more general form into a more special one."[1]

17. Von Baer's views in modern terms.

In order better to understand the difference which separates these various reflections, though breathing so much the air of the more modern theory of evolution, from later views, and to prepare for a real comprehension of the great step taken by Darwin, it will be helpful to resort to modern nomenclature. None of the terms of that vocabulary which was invented by Darwin and his followers to bring home to the popular mind the main points of his revolutionary doctrine are to be found in the earlier writings of von Baer. Nevertheless they are useful in defining the views of the great naturalists who preceded Darwin. Since we have become familiar with the idea of the origin and the transmutation of the different animal and vegetable species, we are accustomed to apply the genetic view not only to the growth and development of individual living things in nature, but to everything else. When von Baer speaks of development, when he tells us that "the history of development is the true source of light for the investigation of organised bodies," he means development in the narrower sense,

[1] *Loc. cit.*, p. 231; transl., p. 220.

that which Haeckel has termed "Ontogenesis," the genesis of the individual being. From this Haeckel distinguishes "Phylogenesis," the genesis of the phyla, the genera, and species. Now, in discussing the relation of the order which prevails in the natural systems of animals to the stages of development of individual embryos, von Baer does not seem to have had before his mind the genesis of one species out of another, a view which he in fact ridicules[1] after a very modern fashion. He looked

[1] *Loc. cit.*, p. 200; transl., p. 187 (1828): "This idea—viz., that the higher forms of animals in the single stages of the development of the individual, from its first origin to its completed development, answer to the permanent forms of the animal series— . . . could not fail to be widely accepted, since it was supported by a multitude of special demonstrations. Certain of its advocates were so zealous that they no longer spoke of similarity but of perfect identity, and assumed that the correspondence had been demonstrated in all cases and to the minutest details. . . . By degrees it became the custom to look upon the different forms of animals as developed out of one another, and then many appeared to forget that this metamorphosis was after all only a mode of conceiving the facts. . . . At length, in sober seriousness, and with all due particularity, we were informed exactly how they arose from one another. Nothing could be easier. A fish, swimming towards the shore, desires to take a walk, but finds his fins useless. They diminish in breadth for want of use, and at the same time elongate. This goes on with children and grandchildren for a few myriads of years, and at last, who can be astonished that the fins become feet? It is still more natural that the fish in the meadow, finding no water, should gape after air, thereby, in a like period of time, developing lungs; the only difficulty being that in the meanwhile a few generations must manage to do without breathing at all. The long neck of the heron arose from a habit its ancestors acquired of stretching out their necks for the purpose of catching fish. . . . An immediate consequence of the assumption of this idea as a natural law was that a view which had once been very general, but had subsequently been pretty generally given up,—that of the universal progression of the different forms of animals,—gradually got footing again. . . . It must be confessed that the natural law being assumed, logical consequence required the admission of the view in question. There was then only one road of metamorphosis, that of further development, either attained in one individual (individual metamorphosis) or through the different animal forms (the metamorphosis of the animal kingdom); and disease was to be considered as a retrogressive metamorphosis, because universal metamorphosis, like a railroad, allows motion backwards or forwards, but not to one side."

upon this order as systematic only, and ideal;[1] he thinks merely of arrangement or "taxonomy." We may say that he deals with phylotaxy (called at that time taxonomy), not with phylogenesis. He conceives that ontogenesis, the historical development of the individual thing, throws light on the "mutual relations of organised bodies";[2] he wishes to make ontogenesis helpful in taxonomy or in phylotaxy. This term did not then exist, but it is useful in order to enable us to understand the change which came over natural science when
18. Phylotaxy and phylogenesis. the attempts at phylotaxy were succeeded by the schemes of phylogenesis, when reasons were established for taking in real earnest the idea then fancifully[3] put forward that the natural order of living beings represented the order in which they had developed out of each other in time. These reasons did not at that time exist.

A suggestion in this direction had indeed been thrown out, and an elaborate theory had been published about

[1] In his later writings von Baer notes especially the difference between a purely ideal and a genetic or genealogical relationship. See 'Reden, &c.,' vol. ii. p. 386 (2nd ed.)

[2] 'Entwickelungsgeschichte' (1828), p. 231; transl., p. 221.

[3] In a later publication of von Baer's (see 'Reden, &c.,' 2 Theil, No. V., "Ueber Darwin's Lehre") the aged author tries to define more exactly the part which his early writings played in the gradual establishment of a genetic conception of nature. If Haller arrived ultimately at the dictum "es gibt kein Werden," we may say that von Baer as emphatically asserted the opposite, that "es gibt kein Sein." In Baer we have progressed from the study of the "esse" (fixed forms) to that of the "fieri" (processes of change and development). See the expositions in the introduction to the article on Darwin. He there also mentions Meckel and Oken as the two principal exponents of the extreme view then put forward and opposed by himself, that the human being in its development passes through the different higher forms of the animal creation, and he maintains that Johannes Müller, who had in the first edition of his 'Physiology' accepted this view, struck it out in the second. He also refers to a passage in a Memoir of 1859, published just before the appearance of the 'Origin of Species,' in which he maintains his belief "that formerly organic forms were less rigid."

ten years before von Baer[1] took up "the subject, which then presented itself as the richest which an anatomist could take up, the history of development," and twenty years before his first larger publication on this subject. Lamarck's[2] 'Philosophie Zoologique' appeared in 1809. 19. Lamarck.
Though known to von Baer, it does not seem to have ever been much appreciated by him, but it was the first serious attempt to deal with phylogenesis, as von Baer's researches were the first consistent studies in ontogenesis.

It is of interest to inquire into the reasons which induced Lamarck to form opinions so entirely different from those which, through the influence and the authority of Cuvier, were then prevalent among naturalists, and to oppose the idea of variability and of descent to that of

[1] Von Baer himself describes—using these words—how in the year 1819 the play of accident or good fortune "threw this subject into his hands." Stieda, p. 67.

[2] Since the interest in the speculations of J. Baptiste de Lamarck (1744 - 1829) has been revived through the writings of Charles Darwin, the historical antecedents of his ideas have also been studied, and his as well as Geoffroy's theories have been brought into connection with the views contained in Buffon's 'Epoques de la Nature.' See especially the interesting analysis in Edmond Perier's 'La Philosophie Zoologique avant Darwin,' 1884. "Ainsi surgissent, posés par Buffon, ce partisan d'abord si résolu de la fixité des espèces, tous les problèmes dont la solution aura été sans aucun doute la pensée dominante de la seconde moitié de ce siècle. . . . Et toutes ces grandes idées que Buffon devine en quelque sorte, vers lesquelles il est invinciblement entrainé par la puissante et rigoureuse logique de son génie, sont précisément celles qui commencent aujourd'hui, appuyées sur un ensemble imposant de recherches, à triompher de tous les scrupules" (p. 68). "Trois grands hommes y vont poursuivre, par des voies diverses, l'œuvre de Buffon: Lamarck, Geoffroy St Hilaire, et Cuvier" (p. 72). For the historical connections of Lamarck's ideas see also Huxley's article in the 9th ed. of the 'Ency. Brit.,' in which he points to a great change which took place in Lamarck's views between 1794 and 1809. In fact, the theories which have given to Lamarck so distinguished a position in the history of the genetic view of nature belong to the latter half of his long life. I know of no other recent example of so late a development of quite original ideas except perhaps the critical philosophy of Kant.

the fixity and independence of species. And it is equally interesting to mark the causes which militated against the more general acceptance of his views, and which cast the 'Philosophie Zoologique' into oblivion. To the first question Lamarck has himself, in the introduction[1] to his great work, furnished us with the means of replying. He there tells us that when the real study of natural history began, and each of the different kingdoms of nature received the due attention of naturalists, animals with a backbone—viz., mammalia, birds, reptiles, and fishes—received the greater attention.[2] Being in general larger, with parts more developed and more easily determinable, they, as it were, obtruded themselves on the attention of man, for whom they are both more useful and more formidable. The other large group of animals, classed together first by Lamarck himself as "Invertebrates," are mostly very small, with organs and faculties less developed, and thus much further removed from man and his interests. Of this by far more numerous class of beings, those called insects had alone at the end of the former century received considerable attention, whereas all the others, classed together by Linnæus as "worms," formed a kind of chaos, an unknown land.

[1] Lamarck's later genetic views are contained in the 'Philosophie Zoologique,' which appeared in 1809, and was republished with a biographical notice by Charles Martin in 1873. I quote from this edition. His principal ideas are also summarised in the introduction to his great work, 'Histoire des Animaux sans Vertèbres' (1816), which in fact he represents as containing the "pièces justificatives de ce que j'ai publié dans ma Philosophie Zoologique." This great work was republished in 1837 by Deshayes and Milne-Edwards. I quote from this edition, which is in three volumes.

[2] See 'Philosophie Zoologique,' Discours préliminaire, vol. i. p. 29; also 'Animaux sans Vertèbres,' Introduction, vol. i. p. 11.

It was to some extent accidental[1] that Lamarck, after having devoted himself for many years to the exclusive study of plants, should on the occasion of the foundation of the different chairs for the natural sciences at the "Museum" suggested by Lakanal, have allotted to him the cultivation of this department, unknown to himself as it was to others, and where even the systematising genius of Linnæus had abstained from trying to make order. Thus it came about that Lamarck brought to the study of the animal world a mind trained in a very different region of science,[2] and that he approached this study

[1] See the "Introduction Biographique," by Martins, 'Philos. Zool.,' I. xiii. "La Convention gouvernait la France, Carnot organisait la victoire. Lakanal entreprit d'organiser les sciences naturelles: sur sa proposition, le Muséum d'histoire naturelle fut créé. On avait pu nommer des professeurs à toutes les chaires, sauf pour la zoologie; mais dans ces temps d'enthousiasme le France trouvait des hommes de guerre et des hommes de science, partout où elle en avait besoin. Étienne Geoffroy Saint-Hilaire était âgé de vingt-et-un ans, il s'occupait de minéralogie sous la direction d'Haüy. Daubenton lui dit: 'Je prends sur moi la responsabilité de votre inexpérience; j'ai sur vous l'autorité d'un père; osez entreprendre d'enseigner la zoologie, et un jour on puisse dire que vous en avez fait une science française.' Geoffroy accepte, et se charge des animaux supérieurs. Lakanal avait compris qu'un seul professeur ne pouvait suffire à la tâche de ranger dans les collections le règne animal tout entier. Geoffroy devant classer les vertébrés seulement, restaient les invertébrés, à savoir les insectes, les mollusques, les vers, les zoophytes, c'est-à-dire le chaos, l'inconnu. Lamarck, dit M. Michelet, accepta l'inconnu . . . il avait tout à apprendre, tout à créer dans ce monde inexploré, ou Linné avait pour ainsi dire renoncé à introduire l'ordre méthodique qu'il avait su si bien établir parmi les animaux supérieurs." Lamarck was accordingly about fifty when he undertook this novel study, which, as Huxley pointed out, was to work such a change in his views (*loc. cit.*)

[2] He had written in six months his 'Flore française,' which was prefaced by his 'Clé dichotomique.' This was in 1778. "Rousseau avait mis la botanique à la mode; les gens du monde, les dames s'en occupaient. Buffon fit imprimer les trois volumes de la 'Flore française' à l'imprimerie royale" (*loc. cit.* p. 11). Lamarck had also qualified as a naturalist by extensive travels in many European countries as a companion to Buffon's son.

from that side which at the time was the least known, and probably the least promising: he approached it, as it were, from below. But this had the consequence of giving to his original mind in two ways a special direction. First of all, it enabled him to look at natural objects from a more general point of view, not as a zoologist or as a botanist, but as a naturalist and a biologist—*i.e.*, from the more general view of the phenomena of Life.[1] Indeed he himself seems to have been

20. The term "Biology." the first, if not to use, at least to introduce in his published writings, the term "biology."[2] And secondly,

[1] 'Philos. Zool.,' Discours prélim., p. 31: "Le vrai moyen de parvenir à bien connaître un objet, même dans ses plus petits détails, c'est de commencer par l'envisager dans son entier; par examiner d'abord, soit sa masse, soit son étendue, soit l'ensemble des parties qui le composent; par rechercher quelle est sa nature et son origine, quelles sont ses rapports avec les autres objets connus; en un mot, par le considérer sous tous les points de vue qui peuvent nous éclairer sur toutes les généralités qui le concernent." P. 32: "La nécessité reconnue de bien observer les objets particuliers a fait naître l'habitude de se borner à la considération de ces objets et de leurs plus petits détails, de manière qu'ils sont devenus, pour la plupart des naturalistes, le sujet principal de l'étude. Ce ne serait cependant pas une cause réelle de retard pour les sciences naturelles, si l'on s'obstinait à ne voir dans les objets observés que leur forme, leur dimension, leur parties externes, mêmes les plus petites, leur couleur, &c., et si ceux qui se livrent à une pareille étude dédaignaient de s'élever à des considérations supérieures, comme de chercher quelle est la nature des objets dont ils s'occupent quelles sont les causes des modifications ou des variations auxquelles ces objets sont tous assujettis, quels sont les rapports de ces mêmes objets entre eux, et avec tous les autres que l'on connait," &c.

[2] Lamarck in his 'Hydrogéologie,' in an appendix (p. 188) which seems to be a rehearsal of his opening lecture of 1801, announces a work, 'Biologie,' as a sequel, being the third and last part of the Terrestrial Physics. This work was not published, but was probably comprised in his 'Philosophie Zoologique.' See Prof. A. S. Packard's excellent work on Lamarck, 'The Founder of Evolution, his Life and Work,' London and New York, 1901. As Lamarck's writings are very scarce and his teaching only imperfectly understood, frequently misrepresented, even by competent authorities, and in popular opinion surrounded by mystery and sometimes treated with ridicule, the work of Prof. Packard is most welcome. It contains copious extracts—unfortunately all translated—from the earlier biological writings and lectures, which are otherwise almost inaccessible. According to Huxley (Lecture "On the Study of Biology," 1876, and "Evolution in Biology," 'Ency. Brit.,' 9th ed.), there were simultaneously three independent attempts to treat the phenomena of organic life as a whole and in connection, emanating from Bichat and Lamarck in France, and from G. R. Treviranus in Germany. The great but unfinished work of the latter, with the title 'Biologie oder Philosophie der lebenden Natur,' was begun in 1796, when the author was only twenty, but the first volume was not published till 1802, one year after Lamarck's 'Hydrogéologie.' Haeckel in his 'Natürliche Schöpfungs geschichte' gives some account of Treviranus' ideas (Band I. Vorlesung 4). Although so much has been written about "Biology," the definition of the science is still uncertain. Prof. Goebel says: "The word Biology is one of those conceptions of modern times which have not yet arrived at a generally accepted limitation. Some understand by it the whole science of living things, others only the doctrine of the phenomena of life in contrast to the purely descriptive branches." ('Pflanzenbiologische Schilderungen,' Marburg, 1889, vol. i. p. 1). With Lamarck biology was only one division of a general science of nature, for he says ('Hydrogéologie,' p. 8): "Toutes ces considérations partagent naturellement la physique terrestre en trois parties essentielles, dont la première doit comprendre la théorie de l'atmosphère, la Météorologie, la seconde celle de la croûte externe du globe, l'Hydrogéologie; la troisième enfin, celle des corps vivants, la Biologie."

it introduced him to the study of animal life from that side where organisation, the phenomena and the organs of life were the simplest, rudimentary as it were, and unformed. Here the great differences of form, the morphological differences which the observation of the higher and more developed creatures force upon our attention, disappear; not the marked differences, but the numerous relations, the endless varieties and resemblances, seem to command our consideration. These seem to be much more likely to "make us understand the beginnings of all organisation as well as the cause of its complexity and of its development."[1] Now in descending in the scale of the living objects of nature, Lamarck was struck by the fact that many of the phenomena of life which in the higher animals seemed to originate within were in the lower creatures produced

[1] Philos. Zool., vol. i. p. 30.

or excited from outside, and he was thus led to the conception that nature herself, through the environment, did a great deal for the lower creatures which in the gradual development of the higher ones she knew how to make them do for themselves.[1] In fact, the idea is worked out in the 'Philosophie Zoologique,' that if we commence the study of living creatures from below, and from the side of vegetable life, we are inevitably led to the conviction that the surrounding conditions and
21. "Environment." influences, the environment, are gradually and slowly modifying the elementary organisms, and through habit and inheritance[2] developing the higher ones, endowing them with more specialised organs and more complex powers and activities.

Lamarck is aware that these ideas sound strange and novel, and he is quite prepared to admit in the reception of them by his readers the same inevitable force of habit which, as it only permits gradual modification of the forms

[1] Philos. Zool., 'Avertissement,' p. 13: "Ayant remarqué que les mouvements des animaux ne sont jamais communiqués, mais qu'ils sont toujours excités, je reconnus que la nature, obligée d'abord d'emprunter des milieux environnants la puissance excitatrice des mouvements vitaux et des actions des animaux imparfaits, sut, en composant de plus en plus l'organisation animale, transporter cette puissance dans l'intérieur même de ces êtres et qu'à la fin elle parvint à mettre cette même puissance à la disposition de l'individu." P. 12: "Ayant considéré que sans les excitations de l'intérieur, la vie n'existerait point et ne saurait se maintenir en activité dans les végétaux, je reconnus bientôt qu' un grand nombre d'animaux devaient se trouver dans le même cas; et comme j'avais eu bien des occasions de remarquer que, pour arriver au même but, la nature variait ses moyens, lorsque cela était nécessaire, je n'eus plus de doute à cet égard."

[2] Ibid., p. 13: "Je pus saisir le fil qui lie entre elles les causes nombreuses des phénomènes que nous offre l'organisation animale dans ses développements et sa diversité, et bientôt j'aperçus l'importance de ce moyen de la nature, qui consiste à conserver dans les nouveaux individus reproduits tout ce que les suites de la vie et des circonstances influentes avait fait acquérir dans l'organisation de ceux qui leur ont transmis l'existence."

of nature, so also opposes a great resistance to any sudden change of opinion. "But it is better," he says, "that a truth once perceived should struggle a long time to obtain merited attention than that everything that the ardent imagination of man produces should be easily accepted."[1] Whereby it may appear to us worthy of note that Lamarck did not stop to reflect on the existence of those sudden changes by which such powers as the "ardent imagination of man" are continually breaking through the slow action of habit. The doctrine of the mutability and variability of species, of the influence of the environment on the habits, and through them and inheritance on the forms of living creatures, was thus opposed to the prevalent doctrine of the fixity of species and the permanence and recurrence of types. Through these generalisations, and through the larger view which Lamarck took of the phenomena of nature and of life, he stepped outside of that school of natural studies which was then dominant in his country, and approached the teachings of the German philosophers of nature, such 22. The "Naturphilosophie."
as Schelling, Oken, and Steffens, with whom Goethe is frequently associated, who, rather than limit themselves to the patient study of detail, indulged in fanciful theories on the origin of life, the genesis and metamorphosis of forms, and the ideal significance of natural phenomena and processes. A wide gap separated the speculations of the author of the 'Flore française,' the 'Histoire des Animaux sans Vertèbres,' and the 'Mémoires sur les Coquilles fossiles des environs de Paris' from those of the German school, yet it cannot be denied that in

[1] Philos. Zool., p. 15.

many passages of the 'Hydrogéologie,' where he speculated on matters of chemistry, geology, and meteorology without the necessary foundation of facts, such as he possessed in botany and zoology, he laid himself open to the criticism and ridicule[1] of his more cautious opponents. Thus it happened that the most original contributions to science were forgotten or disregarded for more than half a century, after which time Lamarckism became a familiar term in speculative science, denoting one of the great ideas with which the genetic view of nature operates — viz., the influence of environment, adaptation, acquired habits, in the development of living organisms.

23. Lamarck and von Baer.

In the history of the genetic view of nature, the position of Lamarck may be regarded as, in a certain sense, complementary to that of von Baer. Both brought the study of living forms back to that of their origins —Lamarck to the study of the lowest forms of animal creation, the great variety and abundance of which he was the first to attempt to put into some order; von Baer to the study of the embryonic beginnings of the higher organisms, on which important subject he was one of the first to throw some light. Though widely

[1] See, *inter alia*, what Cuvier wrote in his 'Eloge de Lamarck,' which was read posthumously in the Academy by Silvestre, 26th November 1832 ('Mem. de l'Acad. des Sciences,' vol. xiii. p. xx), with omissions to tone down its severity: "Quelque intérêt que ces ouvrages excitassent par leurs parties positives, personne ne crut leur partie systématique assez dangereuse pour mériter d'être attaquée; on la laissa dans la même paix que la théorie chimique"; and further on he touches on one of the weakest points of all genetic speculations (p. xxii): "Le temps sans borne qui joue un si grand rôle dans la religion des mages, n'en joue pas un moins grand dans toute cette physique de M. de Lamarck, et c'était sur lui qu'il se reposait pour calmer ses propres doutes et pour répondre à toutes les objections de ses lecteurs."

different in their mental attitude, the two men agreed in looking for the advancement of natural science in an understanding of the simpler, unspecified, and undifferentiated forms or stages of existence out of which they conceived the more complex to have grown or developed by a process of specialisation or differentiation. Many other naturalists and philosophers contributed, partly independently, partly through the influence of Lamarck's systematic and von Baer's embryological labours, to elaborate the same view and strengthen the same tendency of thought and research. Nor were there wanting suggestions as to the ultimate philosophical drift of the line of reasoning. It is doubtful whether these speculations, like those of Oken in his 'Physio-philosophy,' did not retard rather than promote the acceptance of the genetic view by scientific thinkers:[1]

[1] On the position of Goethe and Oken in the history of the genetic view, see Carus, 'Geschichte der Zoologie,' p. 723; von Baer, 'Reden und wissenschaftliche Abhandlungen,' Bd. II. p. 258, &c. Both consider Lamarck as the real originator of a scientific theory of Descent. Von Baer gives an amusing account of the extent to which, as early as 1829, actual genealogical trees were given in Jacob Kaup's 'Skizzirte Entwickelungsgeschichte und natürliches System der Europaeischen Thierwelt.' Von Baer sums up his historical account in the following words (p. 264): "In general I believe that at that time, when the succession of different animals and plants in the history of the earth—and generally from imperfect to more perfect organisms —occupied the thoughts of naturalists, and when, at the same time, the study of development of single organisms had taken a new start, the notion of their Transformation was pretty generally accepted." The view expressed here by von Baer would probably have to be limited to German naturalists at that date. It must, however, be admitted that the fairest exposition and criticism of the arguments of Lamarck at that early date is probably to be found in Lyell's 'Principles of Geology' (vol. ii. Bk. III. chap. i. to iv.) He there also considers the arguments derived from embryology as contained in the researches of Thiedemann, confirmed by Serres ('Anatomie Comparée du Cerveau,' 1824), and comes finally to the result that—1. "There is a capacity in all species to accommodate themselves." 2. "That the mutations thus superinduced are governed by constant laws." 3. That "some acquired peculiarities of form, structure, and instinct are

they belong, therefore, more to the history of philosophical than to that of scientific thought. There is, however, one instance of which it is necessary to take a passing notice.

24. The 'Vestiges.'

In the year 1844 a book appeared which in nine years, up to 1853, ran through nine large editions. It was anonymous,[1] and bore the title 'Vestiges of the

transmissible to the offspring." 4. That "indefinite divergence" from the original type is "prevented." 5. That "the intermixture of distinct species is guarded against by the aversion of the individuals composing them to sexual union." 6. That "it appears that species have a real existence in nature, and that each was endowed, at the time of its creation, with the attributes and organisation by which it is now distinguished." The reviewers of Lyell's work—such as Whewell ('Quarterly,' vol. xlvii. p. 113)—treat Lamarck with much less gravity than Lyell himself, who evidently had studied the 'Philosophie Zoologique' carefully and with much interest; which, I am afraid, was not the case with many others who then and long after only quoted certain extreme passages and examples which had been spread in general literature in a garbled fashion. Contrast in this respect what Lyell wrote to G. Mantell in 1827 ('Life of Lyell,' vol. i. p. 168), where he admits having "devoured Lamarck with pleasure," and though disagreeing with him, admits that it is impossible to say "what changes species may really undergo," with the remarks of Charles Darwin—otherwise so careful and moderate—when he talks of "Lamarck nonsense" ('Darwin's Life and Letters,' p. 23) and his "veritable rubbish" (p. 29), and attributes to him statements which such a careful student of his writings as Prof. Packard had been unable to trace (see his work on 'Lamarck,' 1901, p. 74). One would be inclined to agree with Darwin that such absurdities have done the subject more harm than good, but to attribute them rather to garbled paraphrases and quotations by Lamarck's critics (see Darwin to Hooker, 1853, 'Life,' vol. ii. p. 39) than to Lamarck himself. More than thirty years after the publication of the 'Principles,' when, in consequence of the appearance of the 'Origin of Species,' the subject of Transmutation was much discussed, Lyell wrote to Darwin that he had re-read Lamarck, and admitted that, "remembering when his book was written, he felt he had done him [Lamarck] injustice" ('Life, &c., of Sir Charles Lyell,' 1881, vol. ii. p. 365). In the same letter Lyell states that forty years ago (1823) Prévost, a pupil of Cuvier's, told him his conviction "that Cuvier thought species not real, but that science could not advance without assuming that they were so."

[1] The anonymity of the work was long maintained, and though, after various guesses as to the authorship—attributing it, *e.g.*, to Lyell or Darwin—had been made, it was generally believed that Robert Chambers (1802-1871) was the author, this was not publicly admitted till Alex. Ireland—the last survivor of the few friends to whom the secret was committed—published (1884) the twelfth edition of the book,

Natural History of Creation.' This book contained a very clear and popularly intelligible statement of the genetic or development hypothesis as applied to cosmic, geological, and organic phenomena. The importance of the book did not lie in its own original contributions, but in the great controversy which it occasioned. In this controversy most of the arguments for and against the

with an introduction, in which he "told for the first time" the "story of the authorship." It is of interest, after the lapse of half a century, to read the various—mostly hostile—criticisms of the book in the reviews and magazines of the day. The attacks came from two distinct sides: from scientific authorities, who—each in his own specific branch—challenged the correctness of single facts, mostly without inquiring whether, in spite of many misstatements, sufficient evidence was not after all adduced to prove the main thesis; and, secondly, from both scientific and popular writers, who used the well-known arguments, that the teaching of the book was unorthodox, both in a religious and scientific sense. In fact, they displayed in a great degree scientific and religious dogmatism and intolerance, and in some cases considerable temper. To this larger section of the critical attacks belonged the reviews in all the leading periodicals of the day, headed by the 'Edinburgh Review' (Adam Sedgwick), the 'North British' (Sir David Brewster), the 'Eclectic,' the 'North American' (Bowen and Asa Gray), the 'British Quarterly.' Tolerance and appreciation were, however, shown by some of those more recent reviews which were professedly the organs of freedom, enlightenment, and progress, notably the 'Prospective' (F. W. Newman) and the 'Westminster' in two articles, in the first of which the genetic view of the 'Vestiges' is suggestively contrasted with the purely descriptive of the 'Kosmos.' Looking at the whole controversy, the 'Westminster Review' (xliii. 130) seems, in the light of history, justified in maintaining that, after "having attentively considered the objections which have been urged in numerous able criticisms to the theory and the arguments of the author," after noting that "learned men have discovered that he is less familiar than they with the pedantry of science," that "they have triumphed in the detection of slips of the pen, mistakes in technicalities, and some inaccuracies of fact," the conclusion is nevertheless justified that "these detract but little from the merit of a work which may be fairly characterised as the most skilful generalisation that has yet (1848) appeared of the results of geological, astronomical, and physiological researches made to bear upon the history of the first and most momentous of all problems—the order and plan of creation." It is known that some scientific men of first rank, such as Baden Powell of Oxford, and the physiologist W. B. Carpenter (who, according to Huxley, was the only authority in this country acquainted with the 'Entwickelungsgeschichte' of von Baer), distinctly supported the doctrine of the 'Vestiges'; and Darwin himself, who had studied the 'Vestiges' with evident care

genetic aspect, which have since become familiar, were very ably stated by scientific as well as by popular writers. Earlier anticipations of the genetic view were recalled, the historical sketch given in Lyell's 'Principles' was supplemented by reference to many great and many forgotten authorities, who in more or less distinct terms had given expression to their belief in a gradual development of the existing forms and phenomena of nature out of simpler beginnings, which they described with more or less precision. It cannot be denied that the enormous literature which accumulated during the ten years following the publication of this book unsettled

25. Popular influence.

the popular mind in this country, and prepared it for a really able, dispassionate, and exhaustive exposition of the whole subject, and especially of the crucial problem to which it was narrowed down, the question regarding the fixity or variability, the historical origin and development or the sudden creation and persistence, of animal and vegetable species. The genesis of the cosmos as suggested by Laplace, the geological history of our earth as worked out by Lyell, the fact of organic growth and development as given by embryology, seemed clear

(see 'Life of Darwin,' vol. i. p. 333), gave probably the fairest verdict on the book in the historical preface to the later editions of his own great work, where he says: "The work, from its powerful and brilliant style, though displaying in the earlier editions little accurate knowledge and a great want of scientific caution, immediately had a very wide circulation. In my opinion, it has done excellent service in this country in calling attention to the subject, in removing prejudice, and in thus preparing the ground for the reception of analogous views" ('Origin of Species,' 6th ed., 1872, p. xvii). In a history of European thought it is well to mention that the 'Vestiges' had no influence on the Continent, for reasons partially stated in the text. A little later, however, a similar "scandale" (as the 'Grande Encyclopédie' has it—art. "R. Chambers and L. Büchner") arose in Germany on the publication of 'Kraft und Stoff.'

and plausible enough, but there remained the last stronghold of the older view, the existence of definite forms of animal and vegetable life. Were these to be merely classified and reduced to separate types, as the morphological view was contented to reduce them, or was the growing evidence of variability to be interpreted in favour of a gradual development of the higher out of the lower and simpler forms of life? Above all, how was the highest type of all, man himself, to be regarded in such a comprehensive scheme of development? In Germany many great naturalists[1] were quite prepared for a consistent genetic or developmental view of nature; in France at that time the question was not agitated at all, the suggestive writings of Lamarck and St Hilaire having been

26. Genetic view in Germany and France.

[1] This does not refer to the earlier writings of Goethe, Oken, Treviranus, and others, whose merits, since the appearance of the 'Origin of Species,' have been variously estimated by Huxley in England and by Haeckel in Germany: their speculations had, with the generalisations of the 'Naturphilosophie,' been swept away by the inductive school represented in botany at that time by von Mohl, Nägeli, and Hofmeister; in zoology by the embryological school with von Baer at its head. Of W. Hofmeister (1824-1877), whose labours begin about ten years before the appearance of Darwin's great work, Julius Sachs says: "The results of his 'Comparative Researches' (1849 and 1851) were magnificent beyond all that has been achieved before or since in the domain of descriptive botany, . . . the conception of what was meant by the development of a plant was completely changed, . . . the reader was presented with a picture of the genetic connection between cryptogams and phanerogams which could not be reconciled with the then reigning belief in the constancy of species. . . . When, eight years after Hofmeister's 'Comparative Researches,' Darwin's theory of descent appeared, the affinities of the large divisions of the plant-world lay so openly, so deeply founded, and so clearly before the eyes of students of nature, that that theory had only to recognise what had been made evident in this line by genetic morphology" ('Gesch. d. Botanik,' p. 215, &c.) In another direction Nägeli, by his mechanical theory of "the growth and internal structure of organisms," which he reduces to "physical, chemical, and mechanical processes" (1860), fell in with Darwin's attempt to "reduce the earlier purely formal consideration of organic structures to a causal (genetic) view" (ibid., p. 373).

entirely overruled by the authority of Cuvier.[1] In England, where geology and natural history were always popular pursuits, the question was one of more than scientific interest: it was one which had been appropriated by general literature,[2] and the larger bearings of

[1] Huxley describes the position of France and Germany to the doctrine of descent as follows: "In France the influence of Elie de Beaumont and of Flourens, to say nothing of the ill-will of other powerful members of the Institute, produced for a long time the effect of a conspiracy of silence. . . . Germany took time to consider; Bronn produced a . . . translation of the 'Origin' . . .; but I do not call to mind that any scientific notability declared himself publicly in 1860. None of us dreamed that in the course of a few years the strength (and perhaps, I may add, the weakness) of 'Darwinismus' would have its most extensive and most brilliant illustrations in the land of learning. If a foreigner may presume to speculate on the cause of this curious interval of silence, I fancy it was that one moiety of the German biologists were orthodox at any price and the other moiety as distinctly heterodox. The latter were evolutionists *à priori* already," &c. ('Life of Darwin,' vol. ii. p. 186). The two men abroad to whose opinion English biologists of that day would probably attach the greatest value were Karl Ernst von Baer and Milne-Edwards. The former "wrote to Huxley in August 1860, expressing his general assent to evolutionist views" (*loc. cit.*, p. 186, note). It was von Baer from whom Huxley admits to Leuckart that he learnt the "value of development as the criterion of morphological views" ('Life of Huxley,' vol. i. p. 163). Von Baer later on qualified his adherence, admitting development only within the regions of the different types which he had established (see the second volume of his collected papers). The opinions of the great contemporary French zoologist, Henri Milne-Edwards (1800-1885), are fully given in the last chapter of his very interesting 'Rapport sur les progrès récents des Sciences zoologiques en France' (1867), where he also refers to the writings of Isidore Geoffroy Saint-Hilaire, who in France continued to some extent the line of research and reasoning which, through his father, Etienne Geoffroy, and Lamarck, dates back to Buffon, Bonnet, and other philosophical naturalists, of whom, under the name of "Transformistes," M. Edmond Périer has given a connected account in his very valuable historical work, 'La Philosophie zoologique avant Darwin' (1884). Milne-Edwards remained to the end unconvinced by the arguments of Darwin. He had already in 1853 set forth his ideas referring to the general problems of zoology, and he repeated them in 1867 (*loc. cit.*, p. 432 *sqq.*) It is, however, well to note that ever since 1827 (*loc. cit.*, p. 453, note) he had contributed largely to the furtherance of the genetic view by his principle that progress in nature depends on division of labour. In his subsequent writings he dwells with much success on this principle of the "division of physiological labour." (See Spencer, 'Biology,' vol. i. p. 160.)

[2] About ten years after the controversy about the 'Vestiges' had

which had been fully demonstrated to the educated and reading public. There has always existed in this country a class of literature which is almost entirely wanting, or has died out, on the Continent. The value of this class of literature has been differently gauged, but it never-

filled the columns of the foremost British periodicals, we find in Germany a similar agitation originating through the publication of several works which have since been generally considered as the purest expression of Materialism. The controversy begins in 1852 with the publication of Rudolf Wagner's 'Physiological Letters,' Moleschott's 'Kreislauf des Lebens,' and Carl Vogt's 'Bilder aus dem Thierleben'; it came to its height after the appearance (in 1855) of L. Büchner's 'Kraft und Stoff,' and occupied the meeting of scientific and medical men which was held in Göttingen in 1854. The subject belongs essentially to the history of philosophical thought, and can be studied in the very fair and exhaustive 'History of Materialism' written by F. A. Lange, with a distinctly idealistic tendency (English translation, three vols., by Thomas, 1880). I mention the subject in this connection, because in Germany and England attempts were made about the same time to found a general philosophy of life upon the teachings of science. This had been done about two generations earlier in France by the "Sensualistes" and the "Idéologues." For a French public neither the English nor the German controversy presented any essentially new feature, or disclosed any novel argument. The older orthodox conceptions had been abandoned very largely in France in the eighteenth century, and at once replaced by conceptions derived from science. In Germany a similar movement took place, likewise during the eighteenth century; but, instead of exact science, it was the prevailing idealistic philosophy which was appealed to for the purpose of gaining new foundations, and science only came in when the speculative restoration was generally considered to have failed. In England, which had really supplied the beginnings both for the French sensualistic philosophy through Locke, and for German criticism through Locke and Hume, the older orthodox foundations were not materially shaken before the middle of the nineteenth century. The author of the 'Vestiges' distinctly appeals to science, though in a religious spirit, desiring to make it helpful for a general philosophical, and not merely an industrial, purpose. Again, the English movement, which really culminated in Herbert Spencer, differs from the German, being more influenced by biological conceptions, whereas in Germany the extreme system of Büchner took purely mechanical, though ill-defined, ideas—force and matter—as the shibboleth. It is significant, as showing the great general importance of Darwinism, that through it both the controversy over the 'Vestiges' in England and that over 'Materialismus' in Germany were soon cast into oblivion, though they had both to some extent prepared the way (see Lange, 'Gesch. des Mat.,' p. 570, Ausg. 1867; and Haeckel, 'Schöpfungsgeschichte,' vol. i. p. 98, 9 Aufl.)

theless forms an important feature in the development of English thought, if not also of English science. It is the apologetic literature, those works which deal with what have been termed the "Evidences." In the absence of any scientific theology based upon accurate historical research and philosophical criticism, such as has existed with many good and some evil results since the end of the eighteenth century in Germany, the need was felt for defending or interpreting those answers to the great problems of Nature, Man, and Life, which seemed bound up with the Christian belief, or suggested by the sacred writings. The teaching of science had not become, as in France, a purely secular occupation; instruction was not separated from education; apologetics had not become doubtful through the bad faith and duplicities of cynics like Voltaire, nor ridiculous through the puerilities of shallow writers such as Campe in Germany. Many serious minds were occupied with the growing discrepancies between scientific and popular religious teaching, and believing they could discern the drift of the former, they made various more or less successful attempts to effect a reconciliation between the moving and developing conceptions of scientific thought and the fixed and unalterable ideals of religious belief. Such attempts must be doomed to failure, or at best they offer an individual solution, interesting only if it happens to be the inspiration of a poet or if it represents the creed of one of the few great and soaring intellects which appear once or twice in a century. The conviction is gradually gaining ground that scientific and religious thought emanate from two separate centres,

27. Apologetic literature in England.

that although they inevitably come into frequent contact, the study of their independent origin and history and their different psychological method is more valuable than a temporary and merely ephemeral compromise of their respective doctrines. Happily this country has produced many great and a few thinkers of the first order, in whom the greatest that scientific thought has achieved was in harmony with a truly religious spirit. In contemplating these illustrious examples, and bowing before their greatness, the popular mind will probably find its conviction of the possibility of an ultimate reconciliation of both aspects more strengthened than by leaning on the doubtful support of a voluminous apologetic literature, which proposes to give general proofs where only individual faith can decide.

I deemed it appropriate to offer these few remarks on the whole of the voluminous literature[1] from Butler

[1] The largest and best known type of publication in this class of literature, which is practically unknown on the Continent, but which belongs to our period, is found in the Bridgewater Treatises "On the Power, Wisdom, and Goodness of God, as manifested in the Creation." The circumstances under which this series was published are set forth in the preliminary notice to the first treatise. The Earl of Bridgewater, heir to the title and fortune of Francis Egerton, third Earl of Bridgewater, who constructed from the plans of James Brindley, and in accordance with the idea of his father, Lord Chancellor Egerton, the first of the large canals in England, from his coal mines at Worsley to Manchester and Liverpool, left in his will to the Royal Society the sum of £8000, which, with its accruing interest, was to be paid to the person or persons selected by the President and appointed to write and publish one thousand copies of a work with the above title, — "illustrating such work by all reasonable arguments, as, for instance, the variety and formation of God's creatures in the animal, vegetable, and mineral kingdoms; the effect of digestion, and thereby of conversion; the construction of the hand of man, and an infinite variety of other arguments; as also by discoveries, ancient and modern, in arts, sciences, and the whole extent of literature." The series contained works by such foremost men of science as Sir Charles Bell, William Whewell, William Prout, and William Buckland.

to Drummond whilst I was dealing with the 'Vestiges,' because the latter is probably the last example of that class of books in which purely scientific thinkers took any great interest. Similar publications which have since appeared have made no impression on the course of scientific thought, though they may have won a place in the popular literature of their day. To bring about that complete separation and independence of the scientific and the religious arguments in this country which has been recognised during the whole of the nineteenth century on the Continent, two books have probably contributed more than any others: Dean
28. Mansel and Darwin. Mansel's Lectures,[1] 'On the Limits of Religious Thought,' through its unanswerable logic; and Darwin's 'Origin of Species,' through treating fearlessly a scientific argument which was based upon observation and expanded by legitimate inference without any reference to the ulterior consequences which might be drawn from it. It required some courage to attack a problem beset with such difficulties and which had become hackneyed

[1] It is a remarkable coincidence, showing the general tendencies of English thought about the middle of the century, that Dean Mansel's "Bampton Lectures" appeared just a year before the 'Origin of Species.' The argument of the Lectures "On the Limits of Religious Thought" was that which was elaborated by Sir William Hamilton on the lines of Kant's 'Critique of Pure Reason' in his celebrated article in the 'Edinburgh Review' on the "Philosophy of the Unconditioned." A further appreciation of this line of reasoning, which had its beginning in Hume's sceptical writings a hundred years previously, belong to a different section of this 'History.' We shall there see that in the negative portion of this analysis lie also the germs of the ideas put forward by Herbert Spencer and Huxley under the well-known terms of the "Unknowable" and "Agnosticism," and there is no doubt that both Hamilton and Mansel had a considerable influence in forming Huxley's attitude in this respect. He says, in 1863 ('Life,' vol. i. p. 242): "I believe in Hamilton, Mansel, and Herbert Spencer so long as they are destructive, and I laugh at their beards as soon as they try to spin their own cobwebs."

through periodical and popular literature. Others who, before Darwin, treated similar controversial subjects, such as Whewell, Babbage, Herschel, Lyell, Baden Powell, and the author of the 'Vestiges,' had always taken into account the possible inferences which might be drawn from their scientific statements, and had oftentimes toned them down so as not to offend existing opinions.[1] Darwin thought it more modest and more becoming for an independent scientific thinker to state his side of the question completely and simply, without presuming to attack or to support a view of things which lay outside of the dominion and the powers of science. And this is not the least of the many reasons why his work has created an era, especially in this

[1] The position adopted by several of the eminent forerunners of Darwin is interestingly analysed by Huxley in the chapter on the "Reception of the 'Origin of Species'" contributed to the second volume of the 'Life and Letters of Charles Darwin.' Of Lyell, who had come nearest to the doctrine of unbroken descent of species, Huxley says (vol. ii. p. 193): "I see no reason to doubt that if Sir Charles Lyell could have avoided the inevitable corollary of the pithecoid origin of man—for which to the end of his life he entertained a profound antipathy—he would have advocated the efficiency of causes now in operation to bring about the condition of the organic world, as stoutly as he championed that doctrine in reference to inorganic nature." And Lyell himself wrote to Darwin in 1863 ('Life of Lyell,' vol. ii. p. 365): "I remember that it was the conclusion he [Lamarck] came to about man that fortified me thirty years ago against the great impression which his arguments at first made on my mind." Treviranus, the author of the 'Biologie,' the contemporary of Lamarck, was quite consistent in his views of descent and mutability, for he declares against catastrophism, believes in the evolution of higher species from the zoophytes, and even in that of a higher species than man (see 'Biologie,' vol. ii. p. 225, &c.) Neither in Germany nor in France, at the beginning of the century, did those prejudices exist which in 1859 prevented even Darwin from developing to the full the consequences of his main thesis. This was done in his later works. See his letter to A. R. Wallace, 22nd Dec. 1857 ('Life,' vol. ii. p. 109): "You ask whether I shall discuss 'man.' I think I shall avoid the whole subject, as so surrounded with prejudices; though I fully admit that it is the highest and most interesting problem for the naturalist. My work, on which I have now been at work more or less for twenty years, will not fix or settle anything."

country, not only in the region of scientific, but quite as much in that of philosophical, thought.

29. Triumph of the genetic view.

So far as the purely scientific aspect is concerned, the 'Origin of Species' firmly established the genetic or developmental in the place of the morphological view, or the earlier purely systematic and classificatory treatment of the objects and processes of nature; and it is interesting to note how the period from the publication of the 'Vestiges' to that of the 'Origin of Species,' the fifteen years from 1844 to 1859, was also the period during which Humboldt published his 'Kosmos'—the *résumé* of the labours of a lifetime. This was the consummation of that aspect of nature which I have termed the purely morphological one, and which in his mind was expanded to the panoramic view: the attempt to unroll before his readers a picture or panorama of the whole world as the scientific mind was then able to see it. Nature appeared mapped out in bold and characteristic lines and colours, without allowing the questions of past history or future development,—the origin, life, and fate of the cosmos,—to present itself at all. The fact that this latter question was professedly excluded as foreign, or premature, is probably the reason why the book attracted so little attention in this country, where a new manner of treating all the problems of natural science was being inaugurated; but it is interesting to learn from Darwin that his whole life was influenced[1]

[1] See 'Life and Letters of Charles Darwin,' vol. i. p. 25: "During my last year at Cambridge I read with care and profound interest Humboldt's 'Personal Narrative.' This work, and Sir J. Herschel's 'Introduction to the Study of Natural Philosophy,' stirred up in me a burning zeal to add even the most humble contribution to the noble structure of natural science. No one or a dozen other books influenced me nearly so much as these two. I copied out from Humboldt long passages about Teneriffe," &c. Also vol. i. p. 337: "I never forget that my whole course of life is due to having read and re-read as a youth Humboldt's 'Personal Narrative.'"

and his studies directed by reading and re-reading Humboldt's 'Personal Narrative.' The 'Kosmos' of Humboldt closed the older, the 'Origin of Species' of Darwin opened the new, epoch of natural science: the former was retrospective, the latter prospective. Both works owe their origin to a visit to the same portion of the globe, to a study of the subtropical scenery and life of South America—Humboldt having visited the inland, Darwin specially the maritime and island scenery.[1] It is further of interest to note how the

30. Humboldt's 'Kosmos' and the 'Origin of Species.'

[1] Besides Darwin and Lyell, to whom, of British naturalists as representing the genetic view in the middle of the century, I have so far confined my remarks, there were at that time two other eminent men working in the same direction. The views of these two were likewise much influenced by travel and by the study of plant and animal life in distant countries. I refer to Sir J. D. Hooker and Mr A. Russel Wallace. The important part which these men played in the gradual conception and birth of the ideas which were for the first time comprehensively set forth in the 'Origin of Species' is lucidly and impartially told by Huxley in the well-known chapter which he contributed to the second volume of the 'Life and Letters of Charles Darwin,' edited by his son, Professor Francis Darwin, in 1887. Few episodes in the history of thought have been treated with greater mastery. Few botanists have possessed a greater personal knowledge of different and greatly varying floras than Sir J. D. Hooker, who succeeded to the position and labours of his father, Sir W. J. Hooker, at Kew. After having accompanied Captain Ross on his Antarctic expedition for the discovery of the South magnetic pole, he became best known by his 'Himalayan Journal' (1854). It was in constant correspondence and intercourse with Hooker that Darwin, from 1844 to 1859, wrote his first great work. The important original contributions of Mr Wallace are well known, and the story how his paper, "On the Tendency of Varieties to depart indefinitely from the Original Type," reached Darwin when he had got half through the larger work which he was then writing, how this coincidence hastened the publication of the two papers by Wallace and Darwin, which "contained exactly the same theory," in the 'Journal of the Linnæan Society' (Zoology, vol. iii. p. 45), has been told by Lyell and Hooker (ibid., letter to the secretary), and by Darwin himself (Autobiography, in 'Life,' &c., vol. i. p. 84). No mystery lies upon the history of the first enunciation of the doctrine of natural

same year which witnessed the appearance of the work of Darwin was also that of the invention of Spectrum Analysis, that great instrument by which astronomy, doomed by the purely mathematical treatment to become simply "une question d'analyse," was once more enrolled among the natural sciences; the means being supplied for that natural history of the heavens which is now one of the most progressive and fascinating branches of science. The reader who has realised from the foregoing exposition how the genetic view of nature was anticipated by earlier writers on cosmology, such as Leibniz and Laplace, how it obtained in geology through Hutton and Lyell, how it became dominant in embryology through von Baer, and how the morphological treatment broke down through the recognition of the variability of species and the impossibility of defining clearly the landmarks of zoological and botanical classification, will readily understand the importance and timeliness[1] of Darwin's work, which proposed to deal

selection, no national or personal jealousies obscure the issues which were then at stake; neither of the two great naturalists has ever put forward any complaint that the other has not fairly and generously dealt with his own merit. Since the death of Darwin Mr Wallace has written the well-known book which, under the title of 'Darwinism' (London, 1889), gave to many readers the first comprehensive account of the celebrated theory which is generously identified with the sole name of only one of its original propounders.

[1] Both propounders of the theory of natural selection have in their subsequent writings referred to those who prepared the way before them, and Mr Wallace has taken special pains to explain why a doctrine which was so well prepared, and even anticipated, had not been more distinctly accepted before the appearance of the 'Origin of Species' ("Darwinism," chap. i.): "Notwithstanding the vast knowledge and ingenious reasoning of Lamarck, and the more general exposition of the subject by the author of the 'Vestiges of Creation,' the first step had not been taken towards a satisfactory explanation of the derivation of any one species from any other. Such eminent naturalists as Geoffroy St Hilaire, Dean Herbert, Professor Grant, von Buch, and some others, had expressed their belief that species

specially with the actual fact and the function of variation in the domain of living beings. He pushed the problem of variation and variability into the foreground, and discussed one of its main features—viz., its possible effect and results. Since his time the eye of every botanist, every zoologist, and every embryologist has been directed towards the variability, transition, and genesis of forms, to their history rather than to their portraiture, whereas before him it was mostly attracted by their seeming fixity and recurrence. Variations have been studied on the large and on the minute scale in geological strata at home and abroad, and the vexed question has been raised as to their causes and laws,—Darwin having been mainly occupied with their existence and operation, the results which they brought about, the gradual alterations of the forms of living things. On this side he tells us that he found an important clue through reading a book which had appeared at the very end of the eighteenth century, Malthus's 'Essay on the Principle of Population.'[1]

31. "Variation."

arose as simple varieties, and that the species of each genus were all descended from a common ancestor; but none of them gave a clue as to the law or the method by which the change had been effected. This was still 'the great mystery'" (p. 6). "Darwin, by his discovery of the law of natural selection and his demonstration of the great principle of the preservation of useful variations in the struggle for life, has not only thrown a flood of light on the process of development of the whole organic world, but also established a firm foundation for all future study of nature" (p. 9).

[1] This essay appeared first in 1798, and in the enlarged and much improved form in which it is now known in 1803. Darwin seems to have come upon it accidentally. In his Autobiography ('Life,' vol. i. p. 83) he writes: "In October 1838—that is, fifteen months after I had begun my systematic inquiry—I happened to read for amusement 'Malthus on Population,' and being well prepared to appreciate the struggle for existence which everywhere goes on, from long-continued observation of the habits of animals and plants, it at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavourable

32. Malthus.

The ideas and reflections contained in this celebrated essay, which has played a prominent part in the philosophical literature of economics, could not have occurred to any one who had studied human society or nature merely in individual specimens or isolated cases; for they referred not so much to the natural history of a single being, as to the peculiar relations and complications which arise in a community or society of beings, some of these being applicable quite as much to animal and plant life as to the life of men. In fact, it was a chapter in the science of bionomics. Malthus, Darwin, and Wallace were not "laboratory naturalists, to whom the peculiarities and distinction of species, as such, their distribution and their affinities, have little interest as compared with the problems of histology and embryology, of physiology and morphology."[1] The problem of population, whether it refers to man or other living creatures, is one that will force itself upon those who study nature and mankind on the large, on the outdoor, scale, not as does the collector or dissector of specimens. How has the face of the earth been peopled by plants, animals, and human beings? What are the forces which

ones to be destroyed. The result of this would be the formation of new species. Here, then, I had at last got a theory by which to work," &c. Prof. Haeckel, in his 'History of Creation,' has dwelt exhaustively on this connection of Darwin with Malthus, quoting a letter of Darwin's to him, dated 8th October 1864, in which he says that for years he could not comprehend how any form should be so eminently adapted to its special conditions of life, but that when through good fortune Malthus's book on Population came into his hands, the idea of natural selection came into his mind ('Schöpfungsgesch.,' chap. vi.) In the first paper which Darwin published in the 'Journal of the Linnæan Society' ("Letter to Asa Gray," vol. iii. p. 51), he uses the term "Natural Selection," and refers in the abstract which he there gives to Malthus; whereas Wallace (ibid., p. 56) introduces the term "Struggle for Existence."

[1] Quoted from Wallace, 'Darwinism,' preface, p. vi.

ensure the multiplication, what are those which check the increase, of population? As all living things are dependent on each other, forming the great household or economy of nature or the smaller one of human society, a certain adjustment must exist by which a definite place and part are allotted to every individual and to every class of individuals. Malthus had studied the problem from a political point of view. Here it was felt to be of human and social importance, but his principle was applicable to all living creatures. For everywhere, even in the remotest and only recently discovered countries, we see at work the luxuriant and productive powers of nature on the one side, on the other side the many difficulties and obstacles by which they are forcibly and automatically kept in check, resulting in the ever-recurring spectacle of a "struggle for existence." The more 33. "Struggle for existence."
we penetrate into the hidden and remoter provinces of nature, into the luxuriant "fauna and flora" of tropical regions, or realise the enormous population among the lower forms of life, the more the conviction forces itself upon us that the apparent equilibrium is only maintained by the phenomenon of "crowding out" on a scale compared with which the spectacle unfolded by Malthus in his special application to human societies is quite a miniature display. This process of "crowding out" must have been at work during the untold ages which modern geology has made known to us, and the effects of it must indeed have been extraordinary, and well worthy of study. That living beings, if left to their natural instincts, multiply at an enormous rate, and would, except for certain automatic checks, in a very short time

people the whole habitable portion of the globe, is a fact which has only been realised since Malthus, and, on a much larger and more general scale, Darwin and Wallace have drawn attention to it.[1] This being generally admitted, the questions arise: What are these automatic checks, and what results do they produce? It is evidently quite a new line of reasoning, unknown to former naturalists, or only sporadically and fragmentarily pursued by them; but it introduces us at once into nature itself, away from the class-room and the museum, where we hear of the forces and laws of nature in their abstract mathematical development, or where we behold specimens arranged peacefully and lifelessly side by side. We are face to face with the fierce and continuous conflict which is unceasingly going on around us, and realise the endless changes which it must be producing.

34. Outdoor studies.

Among the many influences which the Darwinian view has had in opposite directions on the thought of our age, none is greater or more fundamental than this, that whereas before Darwin naturalists stepped

[1] On the publication of the 'Origin of Species,' Darwin received many letters pointing out earlier anticipations of his views. The more important of these—bearing upon descent and change—have been referred to in the present chapter. The special principle of natural selection seems to have been already foreseen by Dr Wells in 1813, and published in his famous 'Two Essays upon Dew and Single Vision' in 1818. "In this paper he distinctly recognises the principle of natural selection, and this is the first recognition which has been indicated" ('Origin of Species,' historical sketch to later editions). Another anticipation was that of Patrick Matthew in 1831, in his work on 'Naval Timber and Arboriculture.' "Unfortunately the view was given very briefly in scattered passages in an appendix to a work on a different subject, so that it remained unnoticed until Mr Matthew himself drew attention to it in the 'Gardeners' Chronicle' on April 7, 1860. . . . He clearly saw the full force of the principle of natural selection" (*loc. cit.*, p. xvi). Neither of these writings was known to Darwin in 1859.

out of doors only from curiosity, and in search of new specimens, prompted by the love of travel and adventure, or as companions to commercial and colonising expeditions, they are now forced to do so, because one of the greatest agencies in nature—"the struggle for existence"—can only be studied in nature herself. Before Darwin the study of nature was artificial; through his influence it has become natural. From the point of view of the history of thought, this is surely a much greater result than any of the several theories or special arguments which are connected with his name. These are indeed numerous, each making, as it were, a distinctly new departure in scientific reasoning, characterised by that unmistakable sign[1] of all that is really novel in the realm of thought, the creation of a new vocabulary of distinct terms and phrases. Varieties were known to botanists before Darwin, but who studied "variation" and "variability"? or who spoke of the "divergence of character"? Breeders of stock and pigeon-fanciers knew what "selection" meant, but the

[1] The late Hewett Cottrell Watson, author of the 'Cybele Britannica'—one of a most valuable series of works on the topography and geographical distribution of the plants of the British Islands—wrote to Darwin shortly after the publication of the 'Origin of Species,' 21st November 1859: "I am tempted to write you the first impressions, not doubting that they will, in the main, be the permanent impressions. Your leading idea will assuredly become recognised as an established truth in science—*i.e.*, 'Natural Selection.' It has the characteristics of all great natural truths, clarifying what was obscure, simplifying what was intricate, adding greatly to previous knowledge. You are the greatest revolutionist in natural history of this century, if not of all centuries. . . . Now these novel views are brought fairly before the scientific public, it seems truly remarkable how so many of them could have failed to see their right road sooner. How could Sir C. Lyell, for instance, for thirty years read, write, and think on the subject of species *and their succession*, and yet constantly look down the wrong road?" ('Life of Darwin,' vol. i. p. 352, and vol. ii. p. 226.)

35. "Natural selection" and "sexual selection."

terms "natural selection" and "sexual selection" appeared for the first time in Darwin's writings. The "struggle for existence," and the resulting "survival of the fittest" individuals, represent definite processes always going on consciously or unconsciously in nature and in human society; nor is it less significant that many other phrases have been coined, by which the same idea has been made useful in other domains of research. "Hybrids," "mongrels," "rudimentary organs," and "monstrous" developments, which in earlier times were subjects of mere curiosity, have been raised to scientific importance as indicative of the concealed and mysterious agencies by which natural forms are altered or maintained, and natural processes encouraged or checked. "Environment" and "adaptation" open out great vistas of inquiry, whilst nearly all those different lines of search and of reasoning have latterly become centred in the great problem of "heredity"—the central question of biological science. In addition to these, the older terms of the naturalists and anatomists have received new interpretations. It has been shown by Darwin himself how the vague endeavours of system-makers, since Linnæus, after a "natural" as distinguished from

36. Meaning of natural classification.

a merely "artificial system of classification" of living beings, implied "something more" than mere resemblance, and that this something more is "propinquity of descent—the only known cause of the similarity of organic beings—it being the bond, hidden by various degrees of modification, which is partially revealed to us by our classifications."[1] In the light afforded by

[1] 'Origin of Species,' 1st ed., p. 413.

this idea, the whole work of classification has since Darwin's time been taken up anew; and though it is probably premature to fix upon any elaborate scheme as likely to afford a correct view of the main lines of descent in the two great realms of animal and plant life, single pedigrees, such as those of the rhinoceros and the horse, have, with the assistance of the geological record, been successfully worked out, the missing links having unexpectedly turned up.[1]

In addition to this great service of directing the glance of the naturalist outside, and of helping to overcome the bewildering effects which the aspect of nature must produce on every one who is not prepared for research by some definite aim and a distinct habit of reasoning, the Darwinian spirit has further proved its usefulness by the great increase of our knowledge of the things and phenomena of nature which has taken

[1] "It is certain that, before the theory of descent was accepted or even discussed, genealogical trees were used to represent possible relationships among human races, or possible affinities among animals. It was used as a 'graphic' way of expressing classification, and was true just in proportion as the classification was true. The naturalist traveller, Peter Pallas, was one of the first to use it to express affinities among animals, though it is possible he saw a deeper meaning in his symbol. But when the theory of descent took hold on men's minds, the genealogical tree became more than a graphic register of affinities,—it was used to express the supposed facts of descent. To Ernst Haeckel belongs the credit, or, as some critics would say, the responsibility, of introducing the use of genealogical trees into zoology and botany. In his 'Generelle Morphologie' (1866) and in his 'Schöpfungsgeschichte' (1868, 9th ed. 1897), he displayed numerous genealogical trees designed to show the descent of various stocks and types of animals and plants. There can be no doubt that in so doing he focussed the idea of descent into vividness, and, by the very definiteness of the notation, forced naturalists to a criticism of the reality of the supposed lines of descent. Prof. L. von Graff says of Haeckel's 'Stammbäume,' 'There is due to them the immortal credit of having given the first impetus to the grand revolution in the animal morphology of the last decades'" (J. A. Thomson, 'The Science of Life,' 1899, p. 15).

place since the publication of Darwin's works, by the industry of friend and foe, with the object of proving or of disproving and modifying Darwin's theories. Whole chapters, such as those referring to the fertilisation of plants through insects, to the part which colour plays in the world of flowers or in the plumage of birds and in the wings of butterflies and moths, have been added to our handbooks of natural history;[1]

37. Fertilisation of plants and "Mimicry."

[1] Two remarkable instances may be mentioned. It was known to Christ. Conrad Sprengel that many flowers are "dichogamous"—*i.e.*, that though the organs for self-fertilisation exist in the same flower, nevertheless, because of a want of timekeeping or for other reasons, pollination is done by crossing, wherein the visits of insects are instrumental through elaborate existing arrangements. "Variously coloured spots serve as honey-guides and pathfinders to the exploring insects, hairs protect the nectar from rain and yet offer no obstacle to desirable visitors, other arrangements secure that the insects are dusted with pollen" (J. A. Thomson, 'The Science of Life,' p. 192). Sprengel published his observations in a remarkable book (1793) with the title 'The Secret of Nature discovered in the Structure and Fertilisation of Flowers.' Such was the enthusiasm of this true naturalist, that he, "after being ejected from the rectorate of Spandau for neglecting his flock in favour of flowers, settled down to a frugal life in Berlin, and gave lessons in languages and botany. The commonest plant became new by what he had to say about it; a hair, a spot, gave him opportunity for questions, ideas, investigations" (ibid., p. 191). Sachs ('Gesch.,' p. 449) considers Sprengel's little work to contain "the first attempt to explain the genesis of organic forms out of definite relations to their environment." For sixty years this bionomical classic was forgotten. Darwin in 1841 heard of it through Robert Brown, who, according to Dr Gray ('Nature,' 1874, p. 80), "in common with the rest of the world, looked on Sprengel's ideas as fantastic." The book impressed Darwin, who in 1837 had written in his notebook: "Do not plants which have male and female organs together, yet receive influence from other plants?" as being "full of truth." (See 'Life of Darwin,' vol. i. p. 90; vol. iii. p. 257.) The other important research which has been much stimulated by the two great propounders of Darwinism, is the study of the meaning of colours in plants and animals and the allied subject of "Mimicry." "It is the wonderful individuality of the colours of animals and plants that attracts our attention—the fact that the colours are localised in definite patterns, sometimes in accordance with structural characters, sometimes altogether independent of them, while often differing in the most striking and fantastic manner in allied species. We are thus compelled to look upon colour not merely as a physical but also as a biological characteristic, which has been differentiated and specialised by natural selection, and must, therefore, find its explanation in the principle of adaptation or utility" (Wallace, 'Darwinism,' p. 189). The term "Mimicry" was first introduced by H. W. Bates in his paper on "Mimetic Butterflies," read before the Linnæan Soc., Nov. 1861, and hailed by Darwin ('Life,' vol. ii. p. 392) as "one of the most remarkable and admirable papers" he ever read. The subject had been passed over in the first editions of the 'Origin,' but was introduced in later editions, and has always served as one of the most valuable illustrations and proofs of the theory of natural selection. The whole matter is admirably expounded by Mr Wallace in his long article in the 'Westminster Review,' July 1867, reprinted in his 'Contributions to the Theory of Natural Selection' (1870, pp. 45-129), and again in 'Darwinism.'

the older division of zoology and botany having to a large extent been removed by a study of the interdependence of the many forms of living things and their connection with peculiarities of climate and soil. The Darwinian attitude to the study of natural objects has also introduced into the natural sciences the exact spirit of research,—accurate measurements, together with elaborate countings, being resorted to in order to decide the range of variability of species, the rate of increase in numbers, and the proportion of the surviving to the lost or wasted specimens. A large amount of statistical information[1] has thus been accumulated, and natural history is becoming to some extent an exact science. That it will ever be so to a very large extent is doubtful: it is one of the great merits of Darwin that he has introduced a special method into the sciences of nature—the method of a judicious balancing of evidence. He was fully "aware that scarcely a single point was discussed in his works on which facts cannot be adduced, often apparently leading to conclusions directly opposite to those at which he arrived, and that a fair result can be obtained only by fully stating and balancing the facts

38. The judicial method.

[1] On the development of statistical methods in the service of the theory of evolution, see chap. xii. below.

and arguments on both sides of each question."[1] It is quite a different process of investigation and method of thought from that which the abstract sciences use, where every agency is first considered in its isolated action and mathematically calculated, and a complex effect is rightly looked upon as merely the resultant of specific, well-defined forces, compounded according to rigid dynamical formulæ. That the whole of nature, as well as all observable phenomena, are in reality only the result of such a composition of definite simple actions, and can be studied as such, may be quite correct; but that this method, however useful in isolated cases, and especially however fruitful in the application to artificial mechanisms, will never lead to a just comprehension of any large cluster of phenomena, or to an appreciation of the totality of things which surrounds us, must be evident to any one who at once appreciates the rigidity and universality of mathematical calculations, and sees how soon they fail to become of practical use when we attempt to attack any complex problem through them. Now, all processes in nature herself, as distinguished from the laboratory, are eminently complex, and far transcend the powers and grasp of the mathematical calculus, so far as the human mind is able to employ it. In fact, the outdoor naturalist must attack the problem of nature and life by quite a different method: he must, like a judge, confront and appreciate the evidence of many witnesses who are speaking on all sides to him, and he must, with an open and unbiassed mind, judiciously combine such evidence in the sentences which he passes or the

[1] 'Origin of Species,' 1st ed., p. 2.

generalisations which he attempts. Absolute mathematical certainty is almost unknown in such cases: they can only be made out with more or less clearness and probability.

It seems to me that the new phase into which scientific thought has entered, mainly through the influence of Darwin, has not been sufficiently appreciated by those of his critics who have compared his methods with those of earlier philosophers and naturalists. Darwin has been called by some the Newton of the natural sciences,[1] and again by others his method has been unfavourably contrasted with that of Newton and Cuvier.[2] Some of these 39. Darwin and Newton compared.

[1] It is in many instances only a *façon de parler*. Maxwell similarly called Ampère the Newton of Electrodynamics; and Young has been called the Newton of Optics. Mr Wallace says ('Darwinism,' p. 9): "We claim for Darwin that he is the Newton of natural history, and that, just so surely as that the discovery and demonstration by Newton of the law of gravitation established order in place of chaos, and laid a sure foundation for all future study of the starry heavens, so surely has Darwin, by his discovery of the law of natural selection and his demonstration of the great principle of the preservation of useful variations in the struggle for life, not only thrown a flood of light on the process of development of the whole organic world, but also established a firm foundation for all future study of nature."

[2] The most important publication of this kind is the late Professor Albert Wigand's work, in three volumes, 'Der Darwinismus und die Naturforschung Newton's und Cuvier's' (Braunschweig, 1874-1877). The author significantly classes Humboldt also among those who belong to that period and school of research which has—unfortunately, in his opinion—been superseded by the modern genetic treatment (see vol. iii. p. 14). It is not likely that a perusal of these volumes will, in the mind of the reader, change the current of thought which is now, even more than twenty-five years ago, running in genetic lines, nor will it do anything towards diminishing the sense of importance which attaches to this modern movement. Nevertheless, the book is valuable as giving a very complete *résumé* of what was said "pro and con" Darwinism during the first fifteen years of its existence. It is interesting to see what a small part French scientific opinion played during that period as to the theories of descent and mutability of species, which had both their origin and their first great exponents in France. The book does not appear to have had much influence in its time, but more recently the criticisms of Wigand, von Baer, and other writers seem to receive greater attention since the central biological problems have been pushed into the foreground. Of

comparisons refer to the law of "natural selection," which is placed in parallel with Newton's law of "universal gravity." Now, although "natural selection," the automatic process which ensures the survival of the fittest and the extinction of the less adaptive members in a crowd of living beings, is a definite formula which allows us to understand and clearly define one of the many factors which are at work in the development, in the genesis and growth, of living beings, it is only one. It is not a prime mover or force, like the force of gravity; it is a check upon the over-luxuriance of other existing forces of production and development. These are only very imperfectly known; whereas Newton not only discovered the "law of gravitation," but also the correct expression for the general and all-pervading laws of motion which obtain, even where gravitation or any similar force ceases to be a valid conception. Again, Newton's greatness does not rest on the "law of gravitation" alone, but much more on the general foundations of dynamics and natural philosophy which he has laid. So also Darwin's greatness is not limited to the formula of "natural selection," but depends on the novel conception which he has introduced into the study of nature on the large scale and as a whole, viewing it as a scene of conflict and ceaseless development. From this time dates the study of nature as a whole[1] in contradistinction to that of natural

this I shall treat in the next chapter. See also the various writings of Hans Driesch, such as 'Analytische Theorie der organischen Entwicklung' (Leipzig, 1894); 'Die Biologie als selbständige Grundwissenschaft (1893), especially p. 7 of the latter.

[1] Though this was prepared, as Darwin himself points out, by A. von Humboldt.

objects and processes. The general laws which obtain in this great field, and which would correspond to Newton's laws of motion—the laws of variation and of heredity—have not yet been discovered; but it is again Darwin more than any other naturalist who has called attention to these prime movers in the living universe. He has pushed into the foreground the two great problems of "variation" and "heredity."[1] 40. Unsolved problems.

[1] Darwin in his subsequent writings urged another important problem, to which he had already in his first and greatest work drawn passing attention. This is the agency of "sexual selection." It occupies by far the larger portion of his third great work, which appeared in 1871 with the title 'The Descent of Man and Selection in Relation to Sex.' In the introduction he says, "During many years it has seemed to me highly probable that sexual selection has played an important part in differentiating the races of man; but in my 'Origin of Species' I contented myself by merely alluding to this belief. When I came to apply this view to man, I found it indispensable to treat the whole subject in full detail. Professor Haeckel is the sole author who, since the publication of the 'Origin,' has discussed in his various works, in a very able manner, the subject of sexual selection, and has seen its full importance." The problem of "sexual selection" is introduced in the 'Origin' (p. 87) in the following words: "Inasmuch as peculiarities often appear under domestication in one sex, and become hereditarily attached to that sex, the same fact probably occurs under nature; and if so, natural selection will be able to modify one sex in its functional relations to the other sex, or in relation to wholly different habits of life in the two sexes, as is sometimes the case with insects. And this leads me to say a few words on what I call Sexual Selection. This depends not on a struggle for existence, but on a struggle between the males for possession of the females: the result is not death to the unsuccessful competitor, but few or no offspring. Sexual selection is thus less rigorous than natural selection." A great deal has been written about sexual selection, and in general it may be said that the question belongs to quite a different category from that of natural selection. Some of the foremost champions of the latter doctrine, notably Mr Wallace, reject sexual selection as unnecessary in the whole scheme. The characteristic feature of natural selection is this, that it is a purely automatic process, dependent on overcrowding, whereas in sexual selection it becomes much more difficult to see how the process works automatically. Nowadays the question of natural selection is hardly any longer doubtful; it is a fact. As to sexual selection, the statistical proofs that there is a superabundance from which to choose are still wanting. To understand sexual selection, or even to define it, we need to form some conception of the reason and origin of sexual differentiation, and this cannot be arrived at without a theory of life

And, besides this, it is well to remember that Newton was condemned by some of his contemporaries on the basis of the philosophy of Bacon; Fresnel and Young were condemned on the ground of Bacon and Newton combined. In like manner the novel line of reasoning adopted or largely cultivated by Darwin has been attacked as being opposed to Bacon, Newton, and other great thinkers before him. In all these cases it is the results, and not the theory, of the process of reasoning which have justified its continued employment. Without attempting to elaborate the parallel too minutely, we may say that as Newton created Natural Philosophy and took one brilliant step in fixing for all time one of the great laws of the material universe, so Darwin has founded the study of nature as distinguished from that of the objects and processes of nature, and has enunciated one of the great factors which obtain in the living portion of nature: through him a history of nature, the genetic view of nature on a large scale as distinguished from the older natural history, has for the first time became conceivable. The word history indeed suggests other analogies. Political history, what we ordinarily term history proper, has in the course of our century undergone changes and developments similar to those in the history of nature. Confined once to a casual, unmethodical, uncritical, and incomplete record of isolated

which rests on something more than the two purely statistical or numerical facts of overcrowding and of variation—*i.e.*, the fact that no two individuals are absolutely alike. The importance of the phenomenon of sex in the economy of living nature has been studied, and given rise to many theories. A very good account of these will be found in P. Geddes and J. A. Thomson, 'The Evolution of Sex,' 1889. In the following chapter, where I deal with the various attempts to define "Life," I shall revert to this subject.

events or biographies, it has been gradually united and organised as a whole, largely through the same judicial sifting of manifold evidence and elaboration of critical methods of research. Of this I hope to treat in a different portion of this work: here I only wish to draw attention to the enlarged aspect, which in both instances has, through the same process of development, come over our studies. When once we rise from the contemplation and examination of details and single facts, and grasp the connection and economy of the whole as a subject worthy of special attention, we involuntarily introduce two new elements into our research—the element of conjecture and the element of speculation. The former is needed to fill up the many gaps which we find in the actual records when we wish to string them together into a united and intelligible whole; the latter is the inquiry into the general principles which underlie any and every development of the kind we have in view. The creation by Darwin of the science and history of nature, as distinguished from the science and history of natural objects and single processes, has been accompanied and strengthened by the appearance of conjectural and speculative attempts; just as the cultivation of the science of general history has gone hand in hand with, and has been supported by, the brilliant results of philological conjecture and the philosophy of history.[1] Of

41. Genetic view on a large scale.

[1] In an eloquent passage Professor Parker compares the work of the naturalist of to-day with that of the philologist. This passage occurs in his Memoir on the Fowl (1868), and is quoted in his book 'On the Morphology of the Skull' (by Parker and Bettany, London, 1877, p. 362): "Whilst at work I seemed to myself to have been endeavouring to decipher a palimpsest, and one not erased and written upon again just once but five or six times over. Having erased, as it were, the characters of the culminating type —those of the gaudy Indian bird

these I shall treat elsewhere. It may be a question capable of very opposite answers whether the philosophy of history, such as it has been offered in the brilliant generalisations of Kant, Herder, Hegel, and Buckle, has really aided the science of history proper; whereas no question can arise as to the indispensable service that has been rendered to historians by the criticism and conjectural emendation of texts and other monuments of antiquity. With Darwinism the matter stands differently: no person who peruses the great and increasing literature of the subject can deny the enormous assistance which the philosophical ideas of evolution have rendered to the cause of Darwinism—how the latter, when it appeared, found ready made, though then only slightly appreciated, the philosophical canons and terms which were so well fitted to its systematic enunciation and literary *mise en scène*. This was the independent work of Mr Herbert Spencer.[1] The other well-known

42. Philosophical theories.

43. Herbert Spencer.

—I seemed to be among the sombre grouse; and then, towards incubation, the characters of the sand-grouse and hemipod stood out before me. Rubbing these away in my downward work, the form of the tinamou looked me in the face; then the aberrant ostrich seemed to be described in large archaic characters; a little while and these faded into what could just be read off as pertaining to the sea-turtle; whilst underlying the whole the fish in its simplest myxinoid form could be traced in morphological hieroglyphics."

[1] The part and position which belongs to Mr Herbert Spencer in the history of evolution as a scientific doctrine has not yet received due attention or adequate recognition. There is, however, no doubt that the principal features of the genetic view of natural phenomena were clearly before his mind as early as 1852, when he wrote his short essay on "The Development Hypothesis" in 'The Leader,' republished in the first volume of his 'Collected Essays.' It has been pointed out by Romanes ('Darwin and After Darwin,' vol. i. p. 257) that though the attempts towards a genetic conception of organic nature were numerous, if not abundant, before Darwin, yet this view only broke through and became dominant on the appearance of the theory of natural selection. He says: "If we may estimate the importance of an idea by the change of thought which it effects, this idea of natural selection is unquestionably the most important idea that has ever been conceived by the mind of man. Yet the wonder is that it should not have been hit upon long before;" and after referring to the forgotten anticipations of Wells and Matthew, Romanes proceeds: "Still more remarkable is the fact that Mr Herbert Spencer—notwithstanding his great powers of abstract thought and his great devotion of those powers to the theory of evolution, when as yet this theory was scorned by science—should have missed what now appears so obvious an idea." In this connection it is interesting to note how those general canons of evolutionary thought which were established by Spencer before the publication of the 'Origin' were brought into general recognition by scientific men only when the definite mathematical or statistical formula of natural selection was announced, and that, after the lapse of a whole generation, it is not this precise formula but the general conception of evolution which, according to many of the foremost naturalists, will obtain; the part which natural selection plays being uncertain and variously estimated by the many adherents of the theory of evolution. See, *inter alia*, the article on "Evolution in Biology" by Huxley in the 'Ency. Brit.,' 9th ed., vol. viii. p. 751: "How far natural selection suffices for the production of species remains to be seen. Few can doubt that, if not the whole cause, it is a very important factor in that operation. . . . The importance of natural selection will not be impaired even if further inquiries should prove that variability is definite and is determined in certain directions rather than in others by conditions inherent in that which varies." See also the Address of Lord Salisbury at the meeting of the Brit. Assoc. at Oxford in 1894, and the subsequent remarks of Huxley in seconding the vote of thanks ('Life of Huxley,' vol. ii. p. 378): "The essence of this great work (the 'Origin of Species') may be stated summarily thus: it affirms the mutability of species and the descent of living forms, separated by differences of more than varietal value, from one stock. . . . And yet it is also true that if all the conceptions promulgated in the 'Origin of Species' which are peculiarly Darwinian were swept away, the theory of the evolution of animals and plants would not be in the slightest degree shaken." In fact, the general principles of mechanical evolution, as first systematised by Mr Spencer, received recognition only through a definite formula, but may, after all, survive that special doctrine. It is further very evident how the parallel with Newton's formula of gravitation entirely breaks down if we look at matters in this light; every subsequent discovery having only tended to confirm that special mathematical relation, and proved the all-important part it plays in nature.

name which is so frequently associated with Darwin, especially in Germany, is that of Professor Haeckel, whose 'Generelle Morphologie' and 'History of Creation' have done much to introduce the spirit of Darwinism into German literature. These works also represent the

44. Haeckel.

first brilliant attempt to fill up conjecturally the broken lines of development and descent as the Darwinian conception of living nature postulates them.[1] As a first and daring approximation, they deserve to have assigned to them a prominent place in the history of the scientific thought of our age. In elaborating his pedigrees, Professor Haeckel has taken up and more clearly defined the analogy between the development of the embryo in the higher organisms and the supposed transition from lower to higher forms which is found in the classification of the genera or species of animals and plants. He has termed this analogy the great law of biogenesis, of the development of life in the individual (τὸ ὄν), and the species or tribe (τὸ φῦλον), expressed also as the parallelism of ontogenesis and phylogenesis. Long before Darwin and the appearance of the theory of descent this analogy[2] was pointed out in a restricted

[1] The later editions of the 'Origin of Species' contain the following reference to Haeckel (6th ed., p. 381): "Prof. Haeckel, in his 'Generelle Morphologie,' and in other works, has brought his great knowledge and abilities to bear on what he calls phylogeny, or the lines of descent of all organic beings. In drawing up the several series he trusts chiefly to embryological characters, but receives aid from homologous and rudimentary organs, as well as from the successive periods at which the various forms of life are believed to have first appeared in our geological formations. He has thus boldly made a great beginning, and shows us how classification will in the future be treated." And Huxley (art. "Evolution," p. 752) says: "Whatever hesitation may not unfrequently be felt by less daring minds in following Haeckel in many of his speculations, his attempt to systematise the doctrine of evolution, and to exhibit its influence as the central thought of modern biology, cannot fail to have a far-reaching influence on the progress of science."

[2] As to the early anticipations of this so-called "law of biogenesis," they are given with more or less completeness by many modern writers, such as Huxley in his article on Evolution (1878, 'Ency. Brit.'), P. Geddes (ibid., art. "Reproduction"), Yves Delage ('L'Hérédité,' &c., p. 159), J. A. Thomson ('The Science of Life,' p. 133, &c.) The most important earlier statement is that quoted by Huxley from Meckel's 'Entwurf einer Darstellung der zwischen dem Embryozustande der höheren Thiere und dem permanenten der niederen stattfindenden Parallele' (1811): "There is no good physiologist who has not been struck by the observation that the original form of all organisms is one and the same, and that out of this one form all, the lowest as well as the highest, are developed in such a manner that the latter pass through the permanent forms of the former as transitory stages. Aristotle, Haller, Harvey, Kielmeyer, Autenrieth, and many others, have either made this observation incidentally, or, especially the latter, have drawn particular attention to it, and drawn therefrom results of permanent importance for physiology." Louis Agassiz, in his celebrated "Essay on Classification" (1859), though rejecting the doctrine of descent, "insisted, nevertheless, on the correspondence between stages in embryonic development and the grades of differentiation expressed in the classification of living and extinct animals" (Thomson, 'The Science of Life,' p. 134).

sense by Meckel, von Baer, and Serres. It has sometimes been termed von Baer's law, though von Baer very carefully guarded himself against many popular versions of the analogy, applying it only within the limits of the four great groups or plans of organisation into which he divided the animal kingdom.[1] In his

[1] "A careful examination of von Baer's 'laws' shows that he did not accept the recapitulation without many saving clauses. He believed in it much less than many a modern embryologist, such as F. M. Balfour or A. Milnes Marshall" (Thomson, p. 133). Before the publication of Haeckel's 'Generelle Morphologie' the naturalist who seems to have most clearly expressed the recapitulation theory was Fritz Müller, who in 1864 published his famous tract 'Für Darwin,' which appeared in 1868 in an English translation by Dallas, with the title 'Facts and Arguments for Darwin.' The work of Fritz Müller, who for many years lived in the Brazils, isolated and secluded, and devoted to scientific observation, was welcomed by Darwin as one of the first and greatest supports to his doctrine: the author was singled out by him as the "prince of observers," and frequently referred to in the later editions of the 'Origin of Species.' Delage considers him to have first expressed the fundamental biogenetic law ('L'Hérédité,' pp. 159, 469), and this is in agreement with Haeckel's own declaration in the 13th chapter of the 'History of Creation.' It is, however, well to mention that the recapitulation theory has found little favour with botanists; that Haeckel himself admits that the parallelism between ontogenesis and phylogenesis is general and not exact; that there is a tendency to abbreviation; that recent adaptations (called by him "kainogenetic") may mask more ancient ("palingenetic") features, &c. See J. A. Thomson, 'The Science of Life,' p. 135. Ziegler, in his recent excellent review of the 'Present Position of the Doctrine of Descent' (Jena, 1902, p. 12), admits that the theory of parallelism has "perhaps not realised all the expectations" which were cherished thirty years ago.

time also no attempt was made to bring phytogenesis—the genesis of plant-life—into line and order with zoogenesis, the genetic arrangement of animals. It is Haeckel's undoubted merit to have attempted for the first time to carry out this general scheme on a large scale, and by means of detailed pedigrees, beginning with the undefined organisms in which as yet the peculiar characters of animal- and plant-life do not appear to be differentiated, and ascending in two great trunks into the vegetable and animal kingdom, and thence through many ramifications into the several classes, families, genera, species, and varieties of living things, to construct the supposed real natural system for which systematists had been unconsciously searching since the age of Ray and Linnæus. For the purpose of elaborating this great scheme he employs not only the great law of heredity, according to which ancestral characters are reproduced in development, but also the older law of adaptation to the existing environment, as pointed out by Lamarck. Haeckel, in fact, combines the views of Darwin and Lamarck, which other naturalists are more or less inclined to keep apart, whence has arisen the well-known division into the two great schools of the neo-Darwinians and neo-Lamarckians.[1] Though

45. Combines Darwin and Lamarck.

[1] Natural selection being an admitted fact among living things, like gravitation in the physical universe, three distinct problems arise: First, how far does it reach? the scope of the principle. The subsequent writings of Darwin were mainly occupied with this question, though—as we shall see later—he also ventured upon an important suggestion as to the underlying problem of inheritance. Secondly, the fact or principle itself requires to be traced to deeper-lying causes. We may say natural selection is a *vera causa*, but not a *prima causa*: it is a true but not a prime cause. The investigations regarding "variation" and "heredity" work along this line of research, and form the whole domain of modern post-Darwinian biology. The problem is far from being solved, though it is perhaps nearer a solution than the question as to the cause of gravitation. Thirdly, there is the ambitious attempt to construct a general philosophy of life by means of the new principle, or some modification or amplification of it. After Newton had discovered universal gravitation, the attempt was made by Boscovich and the French school of mathematical physics to use the idea of attraction at a distance as a general physical theory. Of those who, before or after Darwin, attempted the more ambitious task, we may take Herbert Spencer, Ernst Haeckel, and Nägeli as three distinct representatives. They, however, agree in one point—viz., in considering natural selection to be insufficient, and in admitting other agencies, which are largely drawn from the suggestive writings of Lamarck. The section of these philosophical writers who consider Lamarck's principles to be more fundamental than Darwin's, and who are largely represented by American naturalists (notably E. D. Cope and A. Hyatt), are called neo-Lamarckians. The best account of their views will be found in the last chapter of Professor Packard's book, 'Lamarck, the Founder of Evolution' (1901). The following passage quoted there (p. 391) from a much earlier memoir (1877) gives a very clear account of the reasoning of this school: "Darwin's phrase, 'natural selection,' or Herbert Spencer's term, 'survival of the fittest,' expresses simply the final result, while the process of the origination of the new forms which have survived, or been selected by nature, is to be explained by the action of the physical environments of the animals, coupled with inheritance-force. The phrases quoted have been misused to state the cause, when they simply express the result of the action of a chain of causes which we may, with Herbert Spencer, call the 'environment' of the organism undergoing modification; and therefore a form of Lamarckianism, greatly modified by recent scientific discoveries, seems to meet most of the difficulties which arise in accounting for the origination of species and higher groups of organisms." It is also well to note that Mr Wallace, though not a Lamarckian, considers the principle of natural selection insufficient especially to explain the higher developments of mental life. (See 'Darwinism,' p. 463, &c.)

Haeckel's work is, as he himself admits, highly conjectural,[1] it has done much to extend and popularise the

[1] "It is evident that our 'phylogeny' is and remains an edifice of hypotheses in the same way as her sister, historical geology. For she tries to gain a connected view of the course and causes of events long past, the direct investigation of which is impossible. Neither observation nor experiment can give us direct information regarding the endless processes of change through which the existing animal- and plant-forms have emerged out of lengthy ancestral stages. . . . The empirical documents of our history of descent will always remain largely incomplete, however much through continued discoveries our region of knowledge of individual things may increase." (Haeckel, 'Systematische Phylogenie,' 1894, vol. i. preface, p. vi.)

genetic view of nature, drawing likewise into this circle of ideas the great departments of anthropology and geography; in fact, it amounts to rewriting the 'Kosmos' of Humboldt on genetic instead of on purely descriptive lines. But in perusing these and similar writings of modern times, we feel on the one side that we are gradually getting out of the depths of science, not only into the domain of conjecture, without which a knowledge of the past cannot be gained, but also into the

46. Philosophical problems.

regions of philosophical thought, which proceeds on other lines than those prescribed to science, and which will claim our attention in a special portion of this work. On the other side, in using so confidently the ideas of descent and adaptation, we feel that we are appealing to two great empirical facts, the facts of heredity and of variation of living things, on which the genetic view of nature, when applied to the living portion of creation, rests, but which are scarcely even defined in clear terms, much less explained. In fact, we are face to face with the problem and definition of life itself. Neither the morphological nor the genetic view of nature is limited to the living world, although both views originated there, and were from thence extended to the larger domain of inorganic and cosmical phenomena. Into these larger views which try to grasp the forms of nature in their apparent rest or in their endless change and history, the phenomena of life have been fitted by the help of three definite con-

47. Problem of life.

ceptions — the conception of the cell as the morphological basis or unit of all life, and the two conceptions of inheritance and variation, by which living

forms are partially maintained and continuously altered.[1] These three conceptions deserve and have received special attention by a class of students who, since the beginning of the nineteenth century, have termed themselves biologists. On what lines of reasoning their studies have been conducted, and to what general results they have led, I propose to discuss in the following chapter, which might be appropriately entitled the "Biological view of Nature" in the narrower sense of the term. In order to distinguish the studies which I shall have to deal with in that chapter from those which have occupied us in this and the last chapter, which deal largely but not exclusively with living things, I have preferred to give to it the title, "On the Vitalistic[2]

[1] To these — according to some naturalists — might be added the factor of adaptation, so prominently put forward by Lamarck and his followers. But adaptation is one of the causes of variation, as natural selection is a consequence. The latter is a physical necessity wherever overcrowding exists; whereas the scope of adaptation, which is an undeniable fact so far as individuals are concerned, is, so far as it regards inheritance—*i.e.*, the development of the race—a much controverted question. It comes under the larger problem of the influence of environment, and will occupy us again in later chapters. Among the most valuable contributions to this subject are Mr Herbert Spencer's articles on the "Factors of Organic Evolution," published in the 'Nineteenth Century' in 1886, and separately, with additions, in 1887. In these essays he also shows how Darwin himself in his later writings includes the influence of environment as an important factor in development. (See p. 29 *sqq.* of the reprint.)

[2] As the two terms "biological" and "vitalistic" might, according to their etymology, mean the same thing, it may be appropriate to offer some explanation of the reasons which have induced me to adopt the latter term for the purpose indicated in the text. Biology means the science of life. This can only be studied in living things. Living things, however, are formed entirely of the same elementary substances as we find in inorganic or not living things, and are very largely formed through the same chemical and physical processes as we find among the latter. And as our scientific — *i.e.*, exact, accurate, and useful — knowledge has all begun with the study of inorganic phenomena, it is natural that biologists should have attacked the problems of living nature from the side of the similarity or sameness which they presented when compared with lifeless nature. The main progress in physiology

view of Nature." Clearly both the morphological and the genetic views of nature remain incomplete unless they embrace the forms and the processes of life. It is the problem from which both started and to which both lead. They, as it were, presuppose its possible solution. Let us see what has been done in the course of our century to effect it.

Before we do this it is well to draw attention to the great strengthening which the genetic or developmental view of nature has received, since the time of Darwin, from other quarters — notably from that of general physics and chemistry in their application to geology and astrophysics.[1]

and medicine during the last hundred years has come from that quarter. This large class of studies can be carried on without facing the problem of life at all; and thus it happens that we may have a very large biological literature in which the word life hardly occurs, and in which we seek in vain for a definition of life. We must, therefore, have a term which singles out from the enormous mass of biological literature that smaller portion which professedly deals with those properties and phenomena which are peculiar to the living as distinguished from the lifeless creation. I have chosen for this purpose the term vitalistic; but I may note that in using it I do not limit myself to that class of thinkers who are usually termed "Vitalists," because they are led to, or start with the assumption of, a special vital principle. Even those who, in studying the phenomena of life, arrive at or start from the denial of such a principle are included under the vitalistic view, just as Kant is rightly termed a metaphysician although the outcome of his philosophy may be considered to be the destruction of metaphysics in the sense which was current in his age.

[1] A general scheme of evolution, or of development as it was more frequently termed, which would embrace equally cosmical and terrestrial processes, the lifeless and living world, was clearly before the mind of Schelling and his followers, notably Oken and Steffens. The vagueness and extravagancies of this school brought the idea into discredit, and the remedy applied by Hegel, to put a logical process in the place of fantastic suggestions, ruined it utterly in the eyes of the cultivators of exact research. Only very few of the great students of organic development, but among them the greatest, von Baer, retained a just appreciation of the great aims of Schelling. The study of development abroad was almost entirely limited to embryology. In other sciences the "statical" aspect ruled supreme. In the face of this somewhat retrograde movement

48. Genetic view strengthened by physics and chemistry.

In the second chapter of this volume, which treated of the physical view of nature, and developed the various ideas which cluster around the term "energy," I showed how, in the middle of the century, through the introduction of these ideas, a new clue was gained wherewith to penetrate the connection of natural phenomena in time and space. Before that time the conservation of matter, the rule that matter can neither be lost nor created, guided research by trying to account for the apparent loss or gain of matter whenever and wherever changes take place in the material world. The science of chemistry with its instrument the balance was built on the foundation of this axiom. When, through the labours of Mayer, Helmholtz, and Joule, the further axiom became established that, besides matter, there exists in the material universe a second quantity (or substance) termed "energy," which, like matter, can be changed, but which, like matter, can neither be created nor annihilated, the questions began to be asked, "If we

abroad, the merit of Mr Spencer in urging the "dynamical" aspect long before the 'Origin of Species' put forward a definite mechanical agency is so much greater, and he himself says ('Factors of Organic Evolution,' p. 5): "Of the few . . . who, espousing the belief in a continuous evolution, had to account for this evolution, it must be said that though the cause assigned (viz., the modification of structures resulting from modification of functions) was a true cause, . . . it left unexplained the greater part of the facts. Having been myself one of these few, I look back with surprise at the way in which the facts which were congruous with the espoused view monopolised consciousness and kept out the facts which were incongruous with it— conspicuous though many of them were." Mr Spencer was also probably the first who defined in mechanical terms, applicable to cosmical, lifeless, and living phenomena alike, the process of development, adopting the term evolution. This fitting of the process of organic development into the general formula of evolution, and the subsequent announcement by Darwin of the mechanical agency of overcrowding and selection, has had the effect of strengthening immensely the genetic view of nature, but also of obscuring and pushing into the background the special problem of life.

receive energy, where does it come from? if we lose energy, where does it go to?" It was recognised that the great store of energy on which we at present depend is the heat of the sun, which is partly used or wasted by daily radiation, partly stored in the separated energies of chemical substances, such as were produced by the agency of solar heat in bygone ages; the deposits of coal in the bowels of the earth being a prominent and important example. Where does the heat of the sun come from, and how is it maintained? These were some of the questions which began to be asked. The genesis of the cosmos, as suggested by Laplace and fancifully elaborated by popular writers, had taken note only of the matter in the sun and in the planetary system, and had disregarded the heat[1] or energy which the sun supplied, and on which the historical changes on the surface of our globe have almost entirely depended. "But physical laws are for our mental vision," as Helmholtz says, "like telescopes which penetrate into the farthest night of the past and the future."[2] Shortly before the pioneers of the mechanical theory of heat published their

[1] "When Playfair (in his 'Illustrations of the Huttonian Theory') spoke of the planetary bodies as being perpetual in their motion, did it not occur to him to ask, What about the sun's heat? Is the sun a miraculous body ordered to give out heat and to shine for ever?" (Lord Kelvin in 1868, "On Geological Time," 'Popular Lectures and Addresses,' vol. ii. p. 45.) "The old nebular hypothesis supposes the solar system and other similar systems through the universe which we see at a distance as stars to have originated in the condensation of fiery nebulous matter. This hypothesis was invented before the discovery of thermodynamics, or the nebulæ would not have been supposed to be fiery; and the idea seems never to have occurred to any of its inventors or early supporters that the matter, the condensation of which they supposed to constitute the sun and stars, could have been other than fiery in the beginning" (id., 1871, ibid., vol. i. p. 184).

[2] See 'Vorträge und Reden,' 3 Aufl., vol. i. p. 57.

first theoretical and experimental essays, experiments had already been made by Sir John Herschel at the Cape, and independently by Pouillet in France, with the object of measuring the annual expenditure of heat by the sun. They had found it to be an enormous quantity.[1] 49. The heat of the sun.
They represented it popularly by the thickness of a crust of ice on the surface of the earth, which the heat radiated annually by the sun would be able to melt, and they found this to be about 30 metres or 100 feet. Mayer was the first who seems to have put the question definitely: How is this enormous expenditure of heat defrayed, which would, if not in some way compensated, have resulted, even in historical times, in a great lowering of the temperature of the sun, and hence also of that on the surface of our globe, such as is contradicted by all historical evidence? The answer which Mayer gave to this question was based upon an application of his conception of the equivalence of heat and the energy of mechanical motion. As the sun, according to the cosmogonic hypothesis[2] of Laplace, was originally formed by

[1] These measurements were made in 1837, and very nearly agreed. The resulting figures can, of course, only be considered as rough approximations: they have been considerably increased by more recent observations. See A. Berry, 'A Short History of Astronomy,' p. 397.

[2] It does not appear that Mayer brought his "meteoric" hypothesis of the generation and maintenance of the heat of the sun into connection with the nebular hypothesis of Kant and Laplace. In fact, in his first mention of it in his communication to the Paris Academy in 1846 he says simply: "En considérant le grand nombre que nous voyons, comme bolides ou étoiles tombantes, nous ne pouvons pas doubter qu'à tout moment des myriades d'astéroides semblables à une grêle épaisse se jettent dans tous les sens sur le soleil où ils perdent la force vive de leur mouvement" (Mayer's 'Schriften und Briefe,' p. 264); and M. Faye remarks that the fact that Mayer's ideas are opposed to Laplace's theory of the origin of the solar system explains how it came about that his theories were never reported on or explicitly mentioned. Leverrier also seems to have ridiculed the meteoric hypothesis, according to

the gathering up of cosmical matter which, under the force of gravitation, was in rapid motion—so the heat of the sun originated through the conversion of the energy of this arrested motion into heat. This process of gathering up of cosmical or meteoric matter is still going on, and it makes up for the loss or expenditure of solar heat through radiation. Helmholtz, in the sequel of his investigation into the conservation of energy, likewise takes up this problem, and while admitting to some extent Mayer's theory,[1] shows that even without the accession of cosmical matter, the mere contraction through gravitation of the gaseous substances of the sun would result in a continual production of heat. His calculations show that the amount of this contraction, resulting in a diminution of the sun's apparent diameter, would not be great enough to be perceptible during historic ages. The theory of Helmholtz has in general been accepted as

which the sun's heat was kept up by breakfasting and dining on meteorites. (See Wolf, 'Handbuch der Astronomie,' vol. ii. p. 433.) It is on the other side equally interesting to see how Herbert Spencer, for whom the nebular hypothesis was a principal example of cosmic evolution, failed to avail himself of the strengthening support it received through thermodynamics (see 'Essays,' vol. i., "On the Nebular Hypothesis," 1858). Had Mayer brought his ideas into connection with Laplace's cosmogony, he probably would have hit upon the correcter version, the contraction theory, which it was reserved for Helmholtz to propound in 1854.

[1] The subject was about the same time taken up by William Thomson (Lord Kelvin), first in a paper "On the Mechanical Energies of the Solar System" (Trans. Edin. Roy. Soc., 1854), and continued in a series of papers and addresses, reprinted in his mathematical, &c., papers (vol. ii.) in the 1st volume of his 'Popular Addresses,' and in an appendix to Thomson and Tait's 'Natural Philosophy.' He shows that the form of the meteoric theory propounded by Mayer, and independently by Waterston (Brit. Assoc., 1853), is as little able to explain the maintenance of the sun's heat through known historic ages as the chemical theory of combustion, which was already abandoned by Mayer in 1846, and finally adopts Helmholtz's form of the meteoric theory as the most likely. ('Pop. Lect.,' vol. i. p. 365, &c.; p. 373, &c.)

a valid explanation of the maintenance of solar heat. In fact, "as to the sun, we can now go both backwards and forwards in his history upon the principles of Newton and Joule."[1]

But further means for testing the correctness of these theories were afforded by the invention, in 1859, of Spectrum Analysis. It was found that the composition of the light of luminous bodies, as revealed by prismatic scattering in the spectrum, enabled us to tell a good deal about the nature of the body itself from which the light emanated. We can tell whether the body is shining with its own or with reflected light, what are the constituents of the incandescent body, whether it is an incandescent solid or an incandescent gas; also whether the body is in motion or not. The nebular hypothesis supposed that the planetary system owed its origin to incandescent, perhaps gaseous, matter, which, through the force of attraction, was collected in different centres: the discoveries of thermodynamics and of spectroscopy have enabled us to expand and correct some of the assumptions of this theory, and to add new features to its minuter elaboration. It is not necessary that the matter which was originally scattered through space and was gathered into attracting centres should be itself incandescent or gaseous; it may have been cold and solid like dust; rising in temperature and becoming incandescent only through the conversion of arrested motion into heat, which again was maintained for some time through accession of new matter or progressive shrinkage, but which must in a calculable time be radiated away, leaving a 50. Spectrum Analysis.

[1] Lord Kelvin, *loc. cit.*, vol. ii. p. 131.

cold, heavy, lifeless, and lightless body behind.[1] The action of attractive power would sometimes reveal the existence of cold bodies, with specific gravity much in excess of our earth, as in the case of the satellite of Sirius, and the spectroscope would reveal clusters of stars or nebulæ in the various stages of development, such as the nebular hypothesis suggested as making up the genetic process of our planetary system. Much uncertainty and much conjecture must of course exist in these chapters of science, which those who are in full possession of the accumulated and yet very imperfect facts may venture to elaborate in a more or less plausible or fanciful manner. Such attempts to

51. Genesis of the cosmos —Faye and Lockyer.

write the history of the universe have been made in an original fashion by M. Faye in France[2] and Sir Norman Lockyer[3] in this country. They have tried

[1] See Helmholtz, 'Vorträge und Reden,' vol. ii., 3rd ed., p. 88, &c.

[2] 'Sur l'Origine du Monde,' 2nd ed., Paris, 1885. The author, finding the celebrated cosmogonic hypothesis of Laplace in "full contradiction" with the actual state of science, takes up an original theory of Descartes, that of vortices, in order to characterise not the actual, but the initial, stage of the solar system (see Preface): "Autrefois, je veux dire il y a une vingtaine d'années, on avait les coudées franches pour imaginer un système cosmogonique: il suffisait de l'accommoder aux notions contemporaines d'Astronomie solaire et de mécanique céleste. Il n'en est plus de même aujourd'hui, car la thermodynamique assigne à notre Soleil une provision limitée de chaleur, l'Analyse spectrale nous révèle la constitution intime des astres les plus éloignés, et la paléontologie nous fait remonter à des époques où il n'y avait, sur notre globe, ni saisons, ni climats."

[3] Whereas M. Faye has ingeniously modified the original and older nebular hypothesis so as to account for the anomalies in the movement of some of the members of our planetary system, which were unknown or unexplained in Laplace's time, and has tried to account for the phenomena of loss and supply of heat which thermodynamical theory and palæontological records reveal, Sir Norman Lockyer has during more than thirty years been occupied with the elaboration of a special theory which tries to harmonise the revelations of the spectroscope as to the chemical constitution of the sun and other stars with the more recent developments of the atomic theory as suggested by chemical and electrical phenomena observed in our laboratories. His speculations, based upon his own observations as well as those of many other European and American authorities, such as Secchi, Dumas, Kayser and Runge, Rutherford, Rowland, Young, and, above all, of Sir W. Crookes and the late Professor Preston,—all of which, as well as many others, he generously quotes,—were given in three works 'The Chemistry of the Sun' (1887), 'The Meteoritic Hypothesis' (1890), and 'The Sun's Place in Nature' (1897). He has latterly collected the whole evidence in a brilliant and fascinating volume entitled 'Inorganic Evolution as studied by Spectrum Analysis' (1900). The central idea contained in these books, and elaborated with increasing detail and clearness, was suggested as early as 1873, when Sir N. Lockyer pointed out "that many of the difficulties would vanish if it were conceded that the 'atoms' of the chemist were broken up or dissociated into finer forms by the high temperatures employed in the new method of investigation" ('Inorg. Evol.,' p. 73). This "dissociation" hypothesis has been much criticised, and can only be firmly established by patient and prolonged research in that borderland which unites chemistry and astronomy. As the author says: "The chemist has little interest in an appeal to celestial phenomena, and astronomers do not generally concern themselves with chemistry. The region investigated by the chemist is a low temperature region, dominated by monatomic and polyatomic molecules. The region I have chiefly investigated is a high temperature region, in which mercury gives us the same phenomena as manganese. In short, the changes with which spectrum analysis has to do take place at a far higher temperature level than that employed in ordinary chemical work." It is well to note that during and since the time when the dissociation hypothesis was first prominently put forward researches conducted on entirely different lines have led to similar views—*i.e.*, to a further elaboration of the atomic hypothesis. M. Berthelot wrote in 1880: "L'étude approfondie des propriétés physiques et chimiques des masses élémentaires, qui constituent nos corps simples actuels, tend chaque jour d'avantage à les assimiler, non à des atomes indivisibles, homogènes et susceptibles d'éprouver seulement des mouvements d'ensemble . . . il est difficile d'imaginer un mot et une notion plus contraires à l'observation; mais à des édifices fort complexes, doués d'une architecture spécifique et animés de mouvements intestins très variés" (quoted in 'Inorg. Evol.,' p. 28). The first chemical confirmation of the dissociation hypothesis came in 1883 through the "beautiful researches on the rare earth Yttria," contained in Sir Wm. Crookes's Bakerian Lecture to the Royal Society. "In the lectures he gave a sketch of the train of reasoning by which he had been led to the opinion that . . . this stable molecular group had been (by a process termed 'fractionation') split up into its constituents" (ibid., p. 116); and already, in 1879, Sir Wm. Crookes had provisionally accepted the "dissociation" hypothesis (p. 74). Anomalies also in the periodic

to do what Professor Haeckel has done in the more restricted field of the history of the living creation. Whilst these attempts are by many scientific authorities con-

sidered to be premature,[1] they have contributed much to impress on the thought of our age the genetic or developmental view on a large as well as on a minute scale.

law of Mendeléef were explained by utilising this hypothesis (p. 165), and in the sequel other authorities, such as Brodie and Rydberg, expressed themselves in the same sense (p. 164). These, and quite recently the electrical researches of Prof. J. J. Thomson (referred to *supra*, p. 192), support the view, originally suggested in a cruder form by Prout, that what we call elements are really compounds or aggregations or complexes, built up "from similar particles associated with the presence of electricity" ('Inorg. Evol.,' pp. 167, 190; also J. J. Thomson, 'Discharge of Electricity through Gases,' p. 198 *sqq.*)

[1] It would be unfair not to state that many works on astronomy are still written in which all genetic hypotheses are left out, the "statical" view being still the predominant one. Especially in Germany, it seems as if "inorganic evolution" is not very popular; though a large amount of the best work in spectrum analysis of the stars has been done there by H. C. Vogel, Kayser and Runge, Scheiner, and many others. Dr Scheiner, in his valuable work (translated with the title 'A Treatise on Astronomical Spectroscopy,' by Prof. Frost of Dartmouth College, U.S.A., 1894), has some important criticisms on hypotheses and solar theories (see Preface, and the discussion of the Meteoritic Hypothesis in the German edition, Part II. chap. i.) In his 'Bau des Weltalls' (Leipzig, 1901) genetic views are not discussed. The older very valuable works of R. Wolf ('Gesch. d. Astronomie,' 1877, 'Handbuch der Astronomie,' 2 vols., 1890-92) give only slight attention to "genetics," and consider even the "statics" of the universe though a possible yet a difficult problem (see the last-named work, §§ 298, 299). The latest and excellent 'History of Astronomy,' by Mr A. Berry (1898), is likewise reticent about the evolution of the universe, admitting only a general, fairly well-founded presumption in favour of a modified nebular hypothesis (p. 409). It would, therefore, be doubtful whether a history of science should, at the end of the nineteenth century, give much room to these modern genetic theories in astronomy. It is different with a history of scientific thought. However premature and venturesome it may appear to purists in science to elaborate such hypotheses, there is no doubt that the genetic arguments and lines of reasoning have got a firm hold of many great thinkers in the physics of the universe as well as in biology, and that the genetic view of nature in general has received very strong support from the several trains of reasoning and the rapidly increasing revelations of spectrum analysis of cosmical and terrestrial objects, as set forth in Sir N. Lockyer's interesting volumes. Already thirty years ago Lord Kelvin said of the spectroscope: "It is not merely the chemistry of sun and stars, as first suggested, that is subjected to analysis by the spectroscope. Their whole laws of being are now subjects of direct investigation; and already we have glimpses of their evolutional history through the stupendous power of this most subtle and delicate test.

It is intelligible that these different lines in the genetic view of nature—the different trains of reasoning which, in the course of our century, have started independently in astronomy, in geology, and in natural history—should, as they develop and expand, come into contact, and in the event either support or invalidate each other. The former was the case when the geological record, the discoveries of palæontology, were brought in to throw light on the history and development of species; the stories of nature, as written from the point of view of the embryologist, the systematic zoologist and botanist, and the palæontologist, seemed more and more to confirm and support each other. The same cannot be said if we write the history of our earth from the point of view of the geological record on the one side and from that of the purely physical data afforded by thermodynamics on the other. Lord Kelvin has shown [1] that the untold

52. Palæontology and geophysics.

We had only solar and stellar chemistry; we now have solar and stellar physiology" (Presid. Address, Brit. Assoc., 1871. See 'Popular Lectures and Addresses,' vol. ii. p. 180).

[1] The literature of the subject begins with Lord Kelvin's Address to the Geological Society of Glasgow, February 27, 1868, which had been preceded by a paper read before the Royal Society of Edinburgh in 1865, briefly refuting the "Doctrine of Uniformity in Geology." The address began with the words: "A great reform in geological speculation seems now to have become necessary," and in the sequel stated: "It is quite certain that a great mistake has been made—that British popular geology at the present time is in direct opposition to the principles of natural philosophy." These papers are reprinted in the 2nd vol. of 'Popular Lectures and Addresses' (see pp. 10 and 44). The attack was taken up by Huxley in his Address to the Geological Society for 1869, reprinted in 'Lay Sermons,' &c., 1891, p. 198. In a rejoinder to this, delivered in the same year at Glasgow (*loc. cit.*, p. 73), Lord Kelvin shows how the current geology was in the habit of looking upon geological time as "an element to which we can set no bounds in the past any more than we know of its limits in the future" (quoted from Page's 'Text-book'), that Darwin's arguments themselves involve an almost unlimited duration of the conditions admitting of the operation of natural selection, since, in his view, "in all probability a far longer period than 300 million

ages with which geologists, since the time of Lyell, have been accustomed to reckon, are not supported by our present knowledge of the periods during which the so-called secular cooling of the earth has been going forward—the period which has elapsed since the "consistentior status" of Leibniz set in. He has thus put before natural philosophers a problem—the reconciliation of the geological and the thermophysical record—in which the genetic view of nature must be greatly interested. But even more important than all this is

53. Dissipation of energy.

the doctrine of the dissipation of energy, referred to in the second chapter of this volume—a doctrine of which

years has elapsed since the latter part of the secondary period" ('Origin of Species,' 1st ed., p. 287). He shows that Hutton and the uniformitarians were misled by a belief in the so-called stability of the solar system, which took no notice of the effect of tidal friction, nor of the phenomena of radiation and cooling in the past, still less of the law of dissipation of energy, and maintains that the modern ideas of evolution are in a sense a return to the older conceptions of Leibniz, Newton, and other more recent geologists (*loc. cit.*, p. 111). Since the subject was thus brought prominently forward, astronomers, physicists, and geologists have not only—as Huxley expected them to do (see 'American Addresses,' 1886, p. 93)—adduced arguments in order to arrive at an approximate idea how long the earth may have been able to maintain organic life, but biologists have been induced to revise the postulates of the extreme—almost infinite—slowness, and of the uniform continuity of organic changes, originally contained in the Darwinian theory. The influence of these researches upon biological and genetic reasoning has been to emphasise the sudden changes, the ruptures in the continuity of development. In England the great work of Mr William Bateson ('Materials for the Study of Variations,' 1894) has familiarised us with the idea of "Discontinuity" in the origin of species. On the Continent the rapid or even sudden appearance of variations is not a new idea, though the original suggestion of Maupertuis (1748), which was taken up and elaborated by Geoffroy St Hilaire (see Yves Delage, 'L'Hérédité,' p. 291), was forgotten. In quite recent years the reconciliation of the "persistence of species" with their "variability," and of the "geological" with the "biological" records, has been much furthered by the theory of "Mutation" of the celebrated Dutch botanist de Vries. His view is that "every species has its beginning and its end; it behaves in this way like an individual." He refers to the experiments on heredity and crossing of butterflies of Standfuss, who has been led to maintain the existence of sudden or "explosive" transformations; and he

the mechanical and cosmical importance was clearly foreseen by Lord Kelvin in 1852, but which is hardly assimilated yet by scientific, much less by popular, thought.

The two doctrines of the conservation of matter and of energy would lead to the idea that nature is a kind of *perpetuum mobile,* nothing in the way of matter or energy being lost; and that such a reversal of her processes is possible as we are accustomed to deal with in purely mechanical contrivances. But a closer examination of the processes of nature, as distinguished from those of artificial machines, revealed the fact that,

speaks of "periods of mutation"—*i.e.*, of rapid change of species, of which he gives various instances. He concludes that "as many steps as the organisation has taken since the beginning, so many periods of 'mutation' must have existed." He considers the vital processes to be built up out of "units." "Of such units there are probably in the higher plants several thousands, and their ancestors must have run through as many periods of mutation." He concludes with the following words: "Although such calculations are naturally exposed to much criticism, they nevertheless lead on very different roads to identical results. Lord Kelvin, who a few years ago collected and examined critically the various data referring to this subject, arrives at the conclusion that provisionally, and with all reservations, the duration of life on the earth can be placed at 24 millions of years. We accordingly take this figure for our biochronic equation. And as we can with great probability estimate the number of elementary properties in one of the higher plants at some thousands, it follows that the interval of time between two periods of mutation must have lasted several thousands of years." (See de Vries's Address to the German Assoc. of Science at Hamburg in 1891, 'Verhandelungen,' &c., p. 202, &c.; also Lord Kelvin (Phil. Mag. (5.) 47, p. 66). Mr Wallace has, from an entirely different point of view, been led to the conclusion that "certain definite portions of man's intellectual and moral nature could not have been developed by variation and natural selection alone, and that, therefore, some other influence, law, or agency is required to account for them." This would account for an apparent, though perhaps not an actual, break in the continuity of all natural processes, which, in the dictum *natura non facit saltum*, has received a very general expression and acceptance. This dictum—supported by the authority of Leibniz—is, however, by some modern thinkers denounced as a scholastic and antiquated aphorism. (See Yves Delage, 'L'Hérédité,' &c., p. 266.)

though matter and energy be indestructible, the succession of phenomena, the changes and processes which we call the genesis or history of things, are dependent on the condition in which energy exists; it being a general tendency for energy not to be lost, but to become unavailable; change and action, the life of things everywhere, depending on an equalisation of existing differences, say of level or temperature, or quicker and slower motions. This great property of natural, as distinguished from purely mechanical, processes, explains the fact that the processes of nature are irreversible, that the clock cannot be turned back, that everything moves in a certain direction. Various attempts have been made to explain mechanically this remarkable property of all natural processes, which seems to lead us to the conception of a definite beginning and to shadow forth a possible end—the interval, which contains the life or history of nature, being occupied with the slow but inevitable running down or degradation of the great store of energy from an active to an inactive or unavailable condition.

54. Mystery of the actual processes of nature.

This doctrine of the degradation or dissipation of energy leads us one step farther towards an understanding, or at least a description, of the processes of nature, but also of their mystery. It has been urged that, as we always only deal with a small portion of existing things, we have no right to apply conceptions which are based upon a restricted observation to the totality of things in the universe. For instance, we know nothing of what becomes of the energy radiated away into empty space. This is a reflection we should always bear in

mind. We have also been reminded that the theories of the so-called stability of the planetary system which were propounded in the earlier years of our century, and which have found their way into popular treatises on astronomy, are only approximations. On the other side, we have daily before our eyes the ever-recurring instances of the building up and running down of natural forces in smaller systems. These we term organisms, the living things of nature. It is from and through them that we first learnt to look upon the whole of nature as having a history and a life. Imperceptibly we have been led to study life, the genesis of things, on the large scale and in the abstract, and in doing so have lost sight of the life which goes on around and near us. Both the morphological and genetic views of nature started with a biological interest, but have gradually lost sight of it. It is time to come back to it and to see what real progress has been made during our century in the study of life itself—the truly biological view of nature. This will be the object of the next chapter.

CHAPTER X.

ON THE VITALISTIC VIEW OF NATURE.

In the foregoing chapters, where I have treated of the several distinct aspects of nature which have become helpful in science, I have always used the word nature in its widest sense as comprising everything which is revealed to us by our external senses, directly or indirectly.

The title of the present chapter may suggest to some of my readers that I am now narrowing down the meaning of the word,—the vitalistic view of nature being possible only where life is present. The astronomer might say, Life is only known to exist in an infinitesimally small portion of the universe, on the surface of our planet. This infinitesimal area has nevertheless for us the greatest importance, inasmuch as all that we know of the larger outlying world is only won by inference from observations made in this restricted portion. Independently of this, the conception of life itself has always fluctuated between the two extremes of considering it as a universal property of all matter, or on the other hand as quite a casual and accidental occurrence attached to conditions which, from a wider point of

1. The cosmical and the terrestrial views.

view, are extremely rare and exceptional. Between these two views, the cosmical and the terrestrial, the wider[1] and the narrower views of life, biological theories have fluctuated even in our century, and are still fluctuating.

[1] One of the foremost upholders of the wider conception of animation as a universal property of all matter is the celebrated German naturalist, Prof. Ernst Haeckel of Jena. See, *inter alia*, his Address "Ueber die heutige Entwickelungslehre im Verhältnisse zur Gesammtwissenschaft," 1876, reprinted in 'Gesammelte populäre Vorträge,' &c., part ii., Bonn, 1879, p. 119: "The recent controversies regarding the properties of the Atoms, which we must accept in some form or other as the ultimate elementary factors of all physical and chemical processes, seem to be most easily settled by the assumption that these smallest particles of mass, as centres of force, possess a permanent soul, that every atom is endowed with sensation and motion," &c., p. 109: "Arriving at this extreme psychological consequence of our monistic doctrine of development, we attach ourselves to those ancient conceptions as to the animation of all matter which, in the philosophy of Democritus, Spinoza, Bruno, Leibniz, Schopenhauer, have already found varied expression." The cosmical origin of life has also been put forward by such authorities as Helmholtz and Lord Kelvin, as long ago as 1871. (See Helmholtz's lecture "On the Origin of the Planetary System," 'Popul. Vorträge,' &c., vol. ii. p. 91, and Lord Kelvin's celebrated address to the Brit. Assoc. at Edinburgh in 1871, reprinted in 'Pop. Lects.,' &c., vol. ii. p. 199, &c.) This theory of "Panspermia," of the cosmical or ubiquitous nature of the germs of life, has also been proposed by biologists such as H. E. Richter (1865), and has been more fully elaborated by Prof. W. Preyer since the year 1880: it has received further support in the genetic theories of the chemical elements and compounds put forward by him in 1891 ('Die organischen Elemente und ihre Stellung im System,' Wiesbaden), and in 1893 ('Das genetische System der chemischen Elemente,' Berlin). Of the fourteen elements which are common to organic substances, he says (p. 49) "that they belong to the oldest elements"; that "they admit of more varied relations," and "agree with the assumption that, before being condensed as at present on the surface of the earth, they formed at higher temperatures more stable protoplasms which might be in other places the carriers of life"; and he has no doubt "that there existed before the present terrestrial phytoplasma and zooplasma another plasma, which ultimately came from the sun" (p. 50). In fact, Prof. Preyer asks whether, instead of living being evolved from dead matter, the latter is not rather a product of the former. See also the reference to organic evolution as a cosmical process in Sir N. Lockyer's 'Inorganic Evolution' (1900, p. 168). In many of the writings of the celebrated German physicist and philosopher, Gustav Theod. Fechner, the fact is emphasised that we never see the organic developed out of the inorganic, but that everywhere the living generates not only the living but more frequently the inanimate. See Lasswitz, 'G. T. Fechner,' Stuttgart, 1896, p. 130, &c.

No theory of the nature and origin of life has gained universal acceptance: the very alphabet of biology, or the science of life, has still to be written. We fancy we possess some knowledge of certain forms or processes which are common to all living matter, but the description of these has to be kept in the most general, not to say the vaguest, terms: quite unlike the rudiments of other scientific theories which deal with mathematically defined conceptions expressed in distinct language and formulæ.

2. Vagueness of biological theories.

For instance, if we take one of the best founded of modern biological theories — the cellular theory[1] of living matter — we notice that the pretty definite description which the early supporters of this theory—Schleiden and Schwann—gave of this morphological unit of vegetable and animal structure has been displaced by much vaguer descriptions. Schleiden and

[1] The history of the cellular theory has been written from various points of view in all the three languages. I give the titles of a few out of the great abundance of excellent treatises. Foremost stands the work of Prof. Oscar Hertwig of Berlin, 'The Cell: Outlines of General Anatomy and Physiology.' English transl. by Campbell (1895). Then there is the more recent book by Prof. Valentin Häcker of Freiburg, 'Praxis und Theorie der Zellen- und Befruchtungslehre' (Jena, 1899). In the French language we have the great compendium of biological theories by M. Yves Delage, 'La Structure du Protoplasma et les Théories sur l'Hérédité.' &c. (Paris, 1895). In English we have the valuable treatise of Prof. E. B. Wilson, 'The Cell in Development and Inheritance' (1896), and the excellent little work of Prof. James Arthur Thomson, 'The Science of Life' (1899). Of high importance are also the older works of the great master and brilliant expositor in biological science, Claude Bernard, notably his celebrated lectures entitled "Leçons sur les Phénomènes de la vie communs aux animaux et aux végétaux" (1878 and 1879), which every philosophical student of biology should read, as well as his excellent posthumously published little work, 'La science expérimentale,' 1890. Of him M. Dumas says that he has "épuisé ses forces à l'étude du grand mystère de la vie, sans prétendre à pénétrer toutefois son origine et son essence" ('Sci. Exper.,' p. 6).

Schwann defined the cell as "a small vesicle with a firm membrane enclosing fluid content."[1] But the cellular theory was gradually replaced by the protoplasmic theory of Max Schultze, the distinct membrane was found to be frequently absent, and there only remained "a small mass of protoplasm endowed with the attributes of life." The cell, which had once been compared to a crystal, became a very complicated and indefinite thing: it became, in the conception of biologists, an "organism."[2] Further, the nucleus or kernel to which Schleiden attached great importance in his cellular theory was, for a while, quite lost sight of—it being for a long time held that there exist non-nucleated cells. Elaborate theories, such as that of Haeckel,[3] were founded upon this view, till in more

[1] O. Hertwig, 'The Cell,' p. 5 n.

[2] Treatises on the subject now usually begin with an apology, the word cell being considered misleading. Thus Hertwig says (*loc. cit.*, p. 8), "It is evident that the term 'cell' is incorrect. That it has, nevertheless, been retained may be partly ascribed to a kind of loyalty to the vigorous combatants who conquered the whole field of histology under the banner of the cell-theory, and partly to the circumstance that the discoveries which brought about the new reform were only made by degrees, and were not generally accepted at a time when, in consequence of its having been used for several decades, the word cell had taken firm root in the literature of the subject."

[3] "Since, in consequence of the inadequacy of former methods, no nuclei had been discovered in many of the lower organisms, the existence of two kinds of elementary cells was assumed — more simple ones, consisting only of a mass of protoplasm, and more complex ones, which had developed in their interior a special organ, the nucleus. The former were called cytodes by Haeckel (1866), to the simplest solitary forms of which he gave the name of Monera; the latter he called cellulæ, or cytes. But since then the aspect of the question has been considerably changed. Thanks to the improvements in optical instruments and in staining methods, the existence of organisms without nuclei is now much questioned." (Hertwig, 'The Cell,' p. 54. See also Häcker, p. 239.) On the other side M. Delage says ('L'Hérédité,' p. 37), 'Après avoir découvert un noyau chez la plupart des monères et des cytodes et même chez les Bactéries, on a, par une induction à mon sens un peu hâtive, nié l'existence d'organismes sans noyau."

recent times, owing to improvements in the microscope, the existence of organisms without nuclei has become doubtful. To complicate matters still more, to the nucleus have been added the nucleolus, the vacuoles, the central or pole corpuscles of the cell, &c. It is quite evident from this short reference to the changes which the definition of the morphological unit of living matter has undergone, that no complete and accurate description lending itself to measurement and calculation could be based upon it. The conception, useful as it may be, has therefore not permitted of predictions, such as mechanical, physical, and even chemical science, abound in. "Has one ever," says Delage, "in a single instance divined in advance the least of those structures which the microscope has unveiled? Has one divined the transverse striation of muscles, the cilia of vibratile epithelia, the prolongations of nerve-cells, the action of the retina or the arcades of Corti, the chromosomes of the nucleus, the centrosome of the cytoplasma?"[1] Or, to take an example not from the morphology but from the physiology of organic cellular bodies. It is a very general and a very useful property of cells that they readily absorb substances; in fact, this property is one of the most valuable aids in microscopic exam-

3. Impossibility of prediction.

[1] 'L'Hérédité,' &c., p. 746. Prof. Weismann, in his celebrated 'Essays upon Heredity' (Engl. transl. by Poulton, &c., p. 255), claims for the theory of descent that "it has rendered possible the prediction of facts, not indeed with the absolute certainty of calculation, but still with a high degree of probability. It has been predicted that man, who, in the adult state, only possesses twelve pairs of ribs, would be found to have thirteen or fourteen in the embryonic state; it has been predicted that, at this early period of his existence, he would possess the insignificant remnant of a very small bone in the wrist, the so-called *os centrale*, which must have existed in the adult condition of his extremely remote ancestors."

ination, insomuch as the different behaviour of different parts of the cellular body towards organic staining solutions reveals to the observer differences of structure otherwise indistinguishable. Yet Professor Pfeffer,[1] who has studied the absorbing powers of cellular substances with much care, states that these cannot in the least be foretold, but can only be determined empirically; nor is the fact that cells require some substances for their life, while others are harmful, sufficient to enable us to predict that either will be absorbed or rejected. Again, hybridisation has been much studied by gardeners and breeders, and also, since the time of Darwin, by naturalists; nevertheless, the result of cross-fertilisation of individuals belonging "to different families or species, or even only to different varieties," cannot be theoretically foretold, but "can only be discovered by means of experiment."[2]

This ignorance in which we are still placed as to the forms as well as functions of living matter, has been a subject of much comment by biologists all through the

[1] See W. Pfeffer, 'Ueber Aufnahme von Anilinfarben in lebende Zellen.' Untersuchungen aus dem botanischen Institut zu Tübingen. Quoted by Hertwig, 'The Cell,' p. 136.

[2] Hertwig, 'The Cell,' p. 310. Another point, strongly urged by Claude Bernard, is, that a knowledge of structure in living beings—*i.e.*, anatomical knowledge—in no wise suffices to explain the functions, does not lead to physiological knowledge. See 'La Science Expérimentale, p. 105, "L'impuissance de l'anatomie à nous apprendre les fonctions organiques devient surtout évidente dans les cas particuliers où elle est réduite à elle-même. Pour les organes sur les usages desquels la physiologie expérimentale n'a encore rien dit, l'anatomie reste absolument muette. C'est ce qui a lieu par exemple pour la rate, les capsules surrénales, le corps thyroïde, &c., tous organes dont nous connaissons parfaitement la texture anatomique, mais dont nous ignorons complètement les fonctions. De même, quand sur un animal on découvre un tissu nouveau et sans analogue dans d'autres organismes, l'anatomie est incapable d'en dévoiler les propriétés vitales."

century, nor can it be stated that uniformity of opinion exists even yet as to the cause of this ignorance. The enormous progress which has been made in our knowledge of the different properties of living things has had an effect on the minds of those searchers to whom we are mostly indebted for it, similar to that produced on a wanderer who ascends an unexplored and distant peak. Ever and anon, after scaling the eminence just before him, he beholds a new and greater one rising into view, which he contemplates with mixed feelings of discouragement and of eager desire for advance. But whereas our wanderer must know that the very greatest height or distance is none the less a measurable and attainable quantity, what hope has the biologist to encourage him on his way? No other—as it appears to some—than the assurance that he is all the time exploring an unknown country, whereas the final achievement is impossible to him through the inaccessibility of the position or the limitation of his own powers. Others, indeed, from time to time have not taken this despondent view, but, elated by the triumphs which every new step has afforded them, have persistently maintained that some day the last step will be taken and the central peak really gained.

4. Oscillation of biological thought. The history of biological thought, as distinguished from biological knowledge, presents us with the spectacle of a repeated oscillation between these two extreme views: on the one side the continually recurring conviction that the problem of life is insoluble, and, on the other, the assertion that it is soluble, though

it is admittedly as yet unsolved. Biological knowledge itself has progressed on the same lines as chemical, physical, and mechanical knowledge; it registers the progressive conquest of new regions of phenomena exhibited by living matter through the methods which have been discovered in the abstract sciences: but it has generally been felt that this knowledge does not exhaust the subject; that there is some principle involved which we know not; and that we cannot think about the living portion of creation without consciously or unconsciously admitting the existence of this principle. The unknown—nay, possibly, the unknowable—element or factor must be admitted to exist, and it involuntarily governs our reflections on that which we know. To show the difference between reflections on biological and on other phenomena, which, though equally unknown, yet do not contain an admittedly unknown factor, it may be useful to refer to the scientific way of dealing with meteorological phenomena. The science of meteorology is probably as young as that of biology, if not younger. Prediction of the weather is probably even more uncertain than the prognosis of a physician at the bedside of a patient suffering from a malignant disease. Yet no one would suggest that there is a special meteorological principle involved, as in the case of the phenomena of life and death there is a special biological principle. We are quite satisfied that purely mechanical and physical and possibly chemical processes make up the whole of the weather problem, and that the difficulty of the latter is simply one of

5. The unknown factor.

complexity and intricacy. A similar[1] attitude has in the course of our century frequently been taken up with regard to the problem of life, but it has always been abandoned again.[2] We are still told that "in

[1] See, for instance, what Huxley, who, in his earlier writings, might be termed a vitalist (cf. his address "On the Educational Value of the Natural History Sciences," 1854, and his own criticism thereof in the preface, dd. 1870, in 'Lay Sermons and Addresses'), says in his article "Biology," 1875, in the 'Ency. Brit.,' vol. iii. p. 681: "A mass of living protoplasm is simply a molecular machine of great complexity, the total results of the working of which, or its vital phenomena, depend—on the one hand, upon its construction, and on the other, upon the energy supplied to it; and to speak of 'vitality' as anything but the name of a series of operations, is as if one should talk of the 'horology' of a clock." Similarly Claude Bernard, in his 'Leçons sur les phénomènes de la vie,' &c., vol. i. p. 379, says: "En un mot, le phénomène vital est pré-établi dans sa forme, non dans son apparition. . . . La nature est intentionelle dans son but, mais aveugle dans l'exécution." Both Huxley's comparison of an organism with a clock and the quotation from Claude Bernard suggest a parallel between the dictum of Archimedes: "δός μοι ποῦ στῶ καὶ τὸν κόσμον κινήσω," and a possible one of a biologist: "Give me an organism, and I will explain its action mechanically." In another place Claude Bernard says (*loc. cit.*, ii. p. 524): "L'élément ultime du phénomène est physique ; l'arrangement est vital."

[2] Examples of this could be multiplied indefinitely. I take one from an entirely different field. Prof. Kerner von Marilaun, the celebrated botanist, says ('The Natural History of Plants,' transl. by Dr Oliver, 1894, vol. i. p. 52): "In former times a special force was assumed—the force of life. More recently, when many phenomena of plant life had been successfully reduced to simple chemical and mechanical processes, this vital force was derided and effaced from the list of natural agencies. But by what name shall we now designate that force in nature which is liable to perish whilst the protoplasm suffers no physical alteration, and in the absence of any extrinsic cause; and which yet, so long as it is not extinct, causes the protoplasm to move, to inclose itself, to assimilate certain kinds of fresh matter coming within the sphere of its activity and to reject others, and which, when in full action, makes the protoplasm adapt its movements under external stimulation to existing conditions in the manner which is most expedient? This force in nature is not electricity nor magnetism; it is not identical with any other natural force, for it manifests a series of characteristic effects which differ from those of all other forms of energy. Therefore I do not hesitate again to designate as vital force this natural agency, not to be identified with any other, whose immediate instrument is the protoplasm, and whose peculiar effect we call life." Another example is that of Prof. Virchow, to whom we are indebted for the great revolution which the application of the novel conceptions of the cellular theory has worked in the

accepting a mechanical conception," we must not "fall into the very common mistake of trying to explain vital processes as being due directly to mechanical causes." It has been quite as impossible to banish the word life from the biological vocabulary as it has been to banish the word "ought" from the ethical. Biological knowledge has become purely chemical, physical, and mechanical, but not so biological thought. The question "What is life?" still haunts us. Let us see what position the foremost representatives of modern biological research have taken up to this question. We find that they can be divided into two classes.

6. The purely scientific aspect.

First, there are those who have studied the phenomena of living matter solely by the means which the advancing sciences of dynamics, physics, and chemistry have placed at their command. To them biology is an applied science. The question "What is life?" is, according to their view of method, only to be solved by degrees, by bringing the forms and processes manifested in the living world more and more under the sway of observation, measurement, and possibly calculation. The central problem as to the essence of life and the

field of pathology. After having assisted in banishing the older vitalism, he, to the dismay of many of his own school, reintroduced the conception of a vital principle in a well-known review entitled "Old and New Vitalism," in his own journal (vol. ix. p. 20). "Indeed, the living body consists, so far as we know, of substances of the same kind as we find in 'lifeless nature,' and these substances have not only no other properties and powers in the living body, but they do not even lose any of them. . . . Nevertheless, we cannot see how the phenomena of life can be understood simply as an assemblage of the natural forces inherent in those substances: rather do I consider it necessary to distinguish as an essential factor of life an impressed derived force in addition to the molecular forces. I see no objection to designating this force by the old name of vital force."

consensus of many mechanical, physical, and chemical processes in the living organism does exist, but it can only be answered by attacking it from all sides and reducing it to ever narrower issues. The stronghold in which life is intrenched is to be conquered by surrounding it on all sides by the attacking forces of dynamics, physics, and chemistry. It will have to yield some day, though that day may be far off. The number of those who treat biology in this way has increased very much ever since Descartes,[1] and still more Lavoisier, applied

[1] The claims of Descartes to be considered as one of the founders of modern physiology are put forward by Huxley in several of his addresses, notably in that of 'On Descartes' Discours,' &c., 1870 ('Lay Sermons,' &c., p. 279); and in that on 'The Connection of the Biological Sciences with Medicine,' 1881 ('Science and Culture,' p. 325). In the latter address he says: "Now the essence of modern, as contrasted with ancient, physiological science, appears to me to lie in its antagonism to animistic hypotheses and animistic phraseology. It offers physical explanations of vital phenomena, or frankly confesses that it has none to offer. And, so far as I know, the first person who gave expression to this modern view of physiology, who was bold enough to enunciate the proposition that vital phenomena, like all the other phenomena of the physical world, are, in ultimate analysis, resolvable into matter and motion, was René Descartes. . . . And as the course of his speculations led him to establish an absolute distinction of nature between the material and the mental worlds, he was logically compelled to seek for the explanation of the phenomena of the material world within itself" (p. 335). It is interesting to contrast with this announcement of the banishment of the animistic aspect from modern physiology what Prof. Bunge says in the introductory chapter to his well-known 'Text-book on Physiological and Pathological Chemistry' (Engl. transl. by Woolridge, 1890): "The mystery of life lies hidden in activity. But the idea of action has come to us, not as the result of sensory perception, but from self-observation, from the observation of the will as it occurs in our consciousness, and as it manifests itself to our internal sense" (p. 7). "Physiological inquiry must commence with the study of the most complicated organism, that of man. Apart from the requirements of practical medicine, this is justified by the following reason, which leads us back to the starting-point of our remarks: that in researches upon the human organism we are not limited to our physical senses, but also possess the advantage afforded by the 'internal sense' or self-observation" (p. 11). "The essence of vitalism does not lie in being content with a term and abandoning reflection, but in adopting the only right path of obtaining knowledge, which is possible, in starting from what we know, the internal world, to explain what we do not know, the external world" (p. 12).

the purely scientific or exact method to the study of the organism.

7. Influence of medicine.

But biology is not only a subject of purely scientific interest. There is a second and larger class of students —those who study biology as the basis of the art of healing, the medical profession. To them the question of life and death, of the normal or abnormal co-operation of many processes in the preservation of health or the phenomena of disease, is of prime interest: the knowledge of the mechanical, physical, and chemical properties and reactions of living matter, of the construction of the organs and their functions, is only the means to an end. Before the time of Lavoisier, with the solitary exception of Descartes, biology was studied only by medical men; indeed to them both the existence and the progress of the science were entirely due. For them the paramount questions must always be, "What is life? What is its origin? What is death? What are its causes? What is disease?" To this class of students we are indebted for again and again bringing forward and trying to answer these fundamental, these central questions.[1]

By the other, the smaller yet increasing class of purely scientific biologists, we are being continually told that these questions are premature or metaphysical,[2] and

[1] See, for example, the two very interesting and suggestive addresses by Prof. Ed. von Rindfleisch of Würzburg, 'Ärztlich Philosophie' (Würzburg, 1888), and 'Neo-Vitalismus' (Verhandl. d. Ges. deutscher Naturforscher und Ärzte zu Lübeck, 1895, vol. i. p. 111).

[2] See Claude Bernard, 'La Science Expérimentale,' 3me ed., p. 211: "La vie est l'idée directrice ou la force évolutive de l'être; . . . mais l'erreur serait de croire que cette force métaphysique est active à la façon d'une force physique. . . . La force métaphysique évolutive par laquelle nous pouvons caracteriser la vie est inutile à la science, parce qu'étant en dehors des forces

that the answer which we may give to them is of no scientific importance and of no scientific value. The question, "What is electricity? What is the ether?" cannot yet be answered; nevertheless the sciences which deal with the properties of the ether or of electrical bodies are advancing daily. So also—we are told—does the science of biology progress, even though we leave the question "What is life?" unanswered. This would be a tenable position if the living organism were like an electrical or an optical apparatus, constructed by man himself with the modicum of knowledge which he possesses. But the living organism, the eye that can see or the nervous system that is in action, or even the smallest "autonomous" cell, visible only with the microscope, are each an apparatus constructed by nature with the employment of all the intricate agencies which are at her command. In dealing with such an apparatus, we are again and again tempted to ask, "What is life? On what does the normal and healthy co-operation of all parts in the living organism depend? In what does it consist?" Fragmentary knowledge may be well enough so far as it goes, but every medical practitioner must painfully feel it to be altogether insufficient. Where practical interests are involved we cannot indefinitely postpone our answers. Science can wait and

physiques elle ne peut exercer aucune influence sur elles. Il faut donc ici séparer le monde métaphysique du monde physique phénoménal qui lui sert de base, mais qui n'a rien à lui emprunter. . . . En résumé, si nous pouvons définir la vie à l'aide d'une conception métaphysique spéciale, il n'en reste pas moins vrai que les forces mécaniques, physiques, et chimiques, sont seules les agents effectifs de l'organisme vivant, et que la physiologiste ne peut avoir à tenir compte que de leur action. Nous dirons avec Descartes: on pense métaphysiquement, mais on vit et on agit physiquement."

content itself with the known and the knowable. Practice is placed face to face with the unknown and the unknowable.[1] Thus the question will again and again be asked, "What is life?" And for the benefit or injury of mankind theories will exist which profess to handle this delicate problem successfully, even as weather-prophets will always exist though the necessary knowledge for accurate prediction is still wanting.

8. Practice urges the question: What is Life?

One of the first in time and eminence in the course of the nineteenth century to whom we are indebted, not

9. Bichat.

[1] See what Theod. Bischoff, one of the first and foremost German anatomists of the new school, says in his Éloge of Liebig (München, 1874), p. 60. "Inorganic science is not any way induced and is much less obliged to forsake the road from the known to the unknown. But what would have been the result, what would still be the result, if, in all our researches into organised nature, and yet more in all our actions which have reference to our state of health or ill-health, we had proceeded, or were now to proceed, only from data firmly established as to cause and connection? Could we then so much as take a morsel into our mouths or treat a cold otherwise than with fear and trembling? Physiologists and doctors have surely always been ready to proceed according to the methods of exact science so far as this was developed. But so long as this gave but a stone instead of bread, acceptance could not be thought of; necessity compelled us to make some attempt towards the solution of questions, to invent some language in order to gain an understanding; and through this frequently an erroneous procedure has arisen which outlives the means for its correction." "Physiology," says Du Bois-Reymond (Éloge of Joh. Müller, 'Reden,' vol. ii. p. 199), "is the only science in which one is obliged to speak about things which one does not know. Chemistry need not treat of unknown compounds, nor physics of undiscovered forces; botany and zoology do not mind what kind of animals may still move about unknown among unknown vegetation in unexplored regions. But in physiology, even if we confine ourselves to man, a definite number of things is given which must be dealt with. The spleen, the thyroid gland, the thymus, the suprarenal capsules; many parts of the brain, ganglia, nerves, the labyrinth of the ear—all these are there, and must, according to the customary view, be there for something. Manifold suppositions as to the functions of these parts, seemingly supported or invalidated by pathological experience, have put in the place of absolute darkness a twilight which is richer in delusions though not in certainty. The expounder of our science is obliged to lead his pupils through this twilight on an anxious path, and then meet in return with that discouragement which really is owing to the subject itself."

indeed for the name, but for the modern science and direction of biology, was Xavier Bichat, who during the short period of his remarkable career (1771 to 1802) remodelled biological studies. He approached the subject from the side of medicine and in a philosophical spirit. In 1800 there appeared two treatises, one on the membranes and tissues, and another entitled "Recherches physiologiques sur la vie et la mort." These by their titles already reveal the twofold aspect of biological science which drew the attention of Bichat and his school. First, the attempt to reform biological and medical knowledge by a close anatomical examination of organic tissues in their normal and diseased states, for the purpose of which he, within a very short time, examined six hundred corpses. The fuller account of his researches is given in the four volumes of the 'Anatomie Générale' (1801) and in the posthumous five volumes of the 'Anatomie Descriptive,' completed by some of his numerous pupils and followers after his death. In these works Bichat created the science of histology without resorting to the microscope, which was to do such good service in the hands of those who came after him, and without that application of physical and chemical principles which during his time (notably by Lavoisier and his school) had been applied with much success in the theory of animal combustion and in the foundation of another new science—that of organic chemistry. The reasons which inclined Bichat to distrust the microscope were the delusive nature of the revelations of the imperfect instruments then in use. They disappeared when, in the course of the next thirty

years, the instrument was gradually improved. The reasons which prevented Bichat from treating biology as an application of physics and chemistry lay deeper, and were rooted in the second great idea which governed him and his school—his "Vitalism." As stated above, those who have studied the phenomena of life can be divided into two classes. There are those who have been struck by the resemblance of the processes and phenomena in living matter with those in dead or unorganised matter: their attention has been directed more and more to establishing a parallelism between organic and inorganic nature, and they have frequently ended in the conviction that their parallelism warrants us in asserting their ultimate identity. There have been others who have been impressed with the essential and fundamental difference between organic and inorganic processes and phenomena. To them, all attempts to reduce the living process to a mechanism seem to have failed, and however much they have appreciated the insight gained by the other class of students, they have deemed it equally important to emphasise the essential difference—the independence, originality, and incommensurability of the phenomena of life. The latter can be called Vitalists in the broadest sense of the term. Bichat belonged to them. As the former class of students have frequently arrived at the thesis that organic and inorganic processes are ultimately identical, so the latter have frequently arrived at the thesis that they are fundamentally opposed and antagonistic. Bichat gives expression to this view in his celebrated definition of life, as the totality of those

10. His vitalism.

11. His definition of life.

functions which resist death. He adopts, on the one side, the method of looking for the explanation of the phenomena of matter in the properties of matter. In the introduction to the 'Anatomie Générale,' he says:[1] "The connection of the properties as causes with the phenomena as effects is an axiom which has become almost tiresome to repeat nowadays in physics and chemistry: if my book establishes an analogous axiom in the physiological sciences, it will have fulfilled its purpose." But being convinced of the essential difference of the object with which the physiologist is concerned,

[1] Claude Bernard (1813-78), from whose various writings the passages of Bichat are mostly taken, has very fully analysed the theoretical views of his eminent predecessor. The following books belong to the best, in substance and notably in style, that have been written on the subject: 'La Science Expérimentale,' 3me ed., 1890; especially: 'Definition de la vie,' p. 149, &c.: 'Leçons sur les Phénomènes de la vie communs aux animaux et aux végétaux,' 1878, especially vol. i. p. 57, &c.; 'Rapport sur les progrès et la marche de la Physiologie générale en France,' 1867. Introduction. Although Bichat was a vitalist, he took a first and important step in the direction of getting out of the vitalistic conceptions which he inherited from Haller, and which had assumed a special form in the Montpellier school. Through his foundation of physiological research upon an anatomical study of tissues, he localised the problem of physiology. Had he proceeded further on the lines he himself started, he would have thrown off, like his successors, notably Magendie, the hypothetical distinction between physical, chemical, and vital properties, and become a pure experimentalist. The founder of this purely experimental school in France was Magendie (1783-1855). It is interesting to note that prior to Magendie in France, Charles Bell in London had led up to experimental physiology in England by his famous distinction between sensory and motor nerves (1811). But, according to Claude Bernard, this anatomical distinction required experimental verification in a living animal. Magendie furnished this in 1822, and, together with this corner-stone of modern physiology, laid the foundations of the art of vivisection, with all its wonderful discoveries and its disfavour in certain quarters. There is no doubt that for many years Paris became, through this innovation, the centre of medical teaching on the Continent. As to the distinctive merits of Bell and Magendie, see Claude Bernard's exhaustive examination ('Physiol. gén.,' p. 11, &c.), but also Du Bois-Reymond's Eloge of Johannes Müller ('Reden,' vol. ii. p. 176, &c.) According to him the "Thesis" of Bell was not generally considered to be proved till after Müller's experiments in 1831.

he does not advance to the position that the same method will lead to parallel results. "There are," he says, "in nature two classes of things, two classes of properties, two classes of sciences. Beings (things) are organic or inorganic, their properties are vital or non-vital, the sciences are physical or physiological." He did not anticipate that a faithful examination of the properties of organised matter, of membranes and tissues —which should not be limited to lifeless corpses—would more and more reveal that their properties, the forces acting on and in them, could be analysed into the same forces as those we find in the inorganic world.[1]

[1] According to Claude Bernard ('Physiol. gén.,' p. 5, &c.), three things were wanting at the beginning of the nineteenth century to place physiology on a satisfactory basis. The first—anatomical knowledge of the structure of living matter—was brilliantly established by Bichat. But Bichat was not a physiologist: he neglected the second requisite, the study of the continual conflict between the living organism and the mechanical influences of the "milieux," the environment. "Il faudra"—says Bernard—"tenir compte de deux ordres de conditions: 1°, des conditions anatomiques de la matière organisée qui donnent la nature ou la forme des phénomènes physiologiques; 2°, des conditions physico-chimiques ambiantes qui déterminent et reglent les manifestations vitales." A third impulse was wanted in physiology: "il fallait la ramener définitivement à la méthode des sciences expérimentales; il fallait la pousser avec vigueur dans la direction des expériences sur les organismes vivants, afin de la détourner de la voie des hypothèses et des explications prématurées dans laquelle elle s'était si souvent égarée. Un grand physiologiste français, Magendie, mon maître, est venu, au commencement de ce siècle, exercer cette action générale sur la science physiologique, en même temps qu'il l'enrichissait par ses propres découvertes. Magendie fut élevé dans l'école anatomique de Paris, mais il n'était point disposé à suivre les successeurs de Bichat dans leurs explications hypothétiques. Doué d'un esprit précis et pénétrant, sceptique et indépendant, il fut lié de bonne heure avec Laplace, qui le patronna. Par cette influence il se trouva encore fortifié dans son antipathie innée pour les explications physiologiques dans lesquelles on ne se payait que de mots. Puis, par une tendance spontanée de reaction qui, à cette époque, fut très utile à la physiologie, il s'arrêta à l'expérimentation empirique, c'est-à-dire au résultat brut de l'expérience considérée en dehors de toute interprétation et de tout raisonnement."

Bichat, as Claude Bernard has told us,[1] thus clearly and eloquently found the expression or "formula for the fleeting ideas of his age. All the ideas of his contemporaries regarding life, all their attempts to define it, are, in a way, only the echo and paraphrase of his doctrine." We find it repeated by surgeons like Pelletan, who practised in the Hôtel Dieu, and by great naturalists like Cuvier, who founded comparative anatomy. To both of these life was a contest, a struggle, as it is at the end of the century to the Darwinians; but it was a struggle of the living forces against the dead, whereas nowadays it is the struggle of the living for supremacy amongst each other or a process of adaptation to external conditions. Whilst there is this great difference between
12. Vitalism and Darwinism. these two views characterising respectively the beginning and the end of our century, they have one point in common—they both emphasise the unrest, the continued change, the extreme mobility which distinguishes living matter. But even this distinction has ceased during the course of the century to impress us so much as it did Bichat; since the stability of the solar system proclaimed by Laplace has ceased to charm astronomers, and the dictum of ancient science has been refuted: "materiam cœli esse inalterabilem."[2]

[1] 'La Science Expérimentale,' p. 164.

[2] Claude Bernard (*loc. cit.*, p. 172, &c.) dwells on this point with great eloquence. "Aujourd'hui l'esprit des astronomes est familiarisé avec l'idée d'une mobilité et d'une évolution continuelle du monde sidéral. Les astres n'ont pas toujours existé, dit M. Faye; ils ont eu une période de formation; ils auront pareillement une période de déclin, suivie d'une extinction finale. . . . Les astronomes, avant de connaître les lois des mouvements des corps célestes, avaient imaginé de puissances, des forces sidérales, comme les physiologistes reconnaissaient des forces et des puissances vitales. Kepler lui-même admettait un esprit recteur sidéral par l'influence duquel les planètes suivent dans l'espace des courbes savantes sans heurter les astres qui fournissent d'autres carrières, sans troubler l'harmonie réglée par le divin géomètre." Another property which was once thought peculiar to and characteristic of living organisms, that of regeneration after mutilation, of "redintegration," is now known to exist also in lifeless structures: "M. Pasteur a signalé des faits de cicatrisation, de rédintégration cristalline, qui méritent toute notre attention. . . . Ces faits . . . se rapprochent complètement de ceux que présentent les êtres vivants lorsqu'on leur fait une plaie plus ou moins profonde" (ibid., p. 173).

After the age of Bichat, and largely through his influence,—*i.e.*, through the cultivation of anatomical researches,—the pendulum swung in the direction of proving more and more the parallelism of organic and inorganic processes. It reached its maximum swing in that direction about the second third of the century. Since then it appears to have again returned in the opposite direction. Let us follow this movement somewhat more closely, and see how the stronghold in which the innermost secret of life is intrenched has been attacked from all sides by all the processes and methods of the mechanical, physical, and chemical sciences, and how it has persistently refused to surrender.[1] There was a time when the leading repre-

[1] Bischoff, in his Éloge of Liebig, who remained all his life a vitalist, says (p. 57): "We must, indeed, as in the exact sciences, guard against letting a mere word step in as an explanation, wherever our insight into the conditioning causes has been insufficient, as was indeed repeatedly done formerly, when a word was considered to be a sufficient reason. We must consider it to be the continual duty of organic science to wage, as it were, a constant war against this organic force, and to dispute its territory wheresoever possible. If, for example, a talent like his succeeds in deducing many morphological traits of the higher animal organisms from the mechanical conditions of growth in the embryo, &c., we shall gratefully accept the proof; but we must all the while not forget to ask the further question, by whom these mechanical conditions have been brought together. If it be further true that the cells of the embryo perform the most extraordinary wanderings, in order to arrange themselves into the various tissues and organs of the animal body, we shall welcome this as a very interesting and remarkable phenomenon in the obscure region of development; but we have received no light on the question who acts as guide to the wandering cells. Similarly, if chemistry should some day succeed in forming albumen ar-

13. The extreme vitalism.

sentatives of the medical profession considered it unworthy and degrading to treat the human frame as a mechanism, and to approach it by the methods used in other sciences. "For the vitalist physician," says Helmholtz,[1] "the essential part of the vital processes did not depend on natural forces which act according to fixed laws. What these could do appeared of secondary importance, and a study of them hardly worth the trouble. He thought to be face to face with something soul-like,"—the anima of Stahl, the vital force of the vitalists,—"which had to be met by a thinker, a philosopher, a man of spirit. . . . Auscultation and percussion were practised in the hospitals,[2] but I have heard it said that these were crude

tificially, we shall probably be able to date from that day an entirely new period in natural science, but this artificial production of albumen will never be feasible through the simple affinities of the elements, but only by producing a new arrangement in organic substances already formed by the plant. We shall gratefully receive all such increase of our knowledge: we do not require wonders and belief in miracles for the vital force, but only a name for the effects of which we do not know the causes. . . . Neither the ancient primæval ooze nor the modern Bathybius, neither the remote monads nor the recent monera, neither protoplasm, nor nucleus and cell and their development, confessedly so simple and easily understood up to self-conscious man, give us the smallest clue to the forces at work and their origin. This induces us to ascribe them to a force, regarding the essence of which we indeed know no more than we know of any cause that cannot be further analysed. But we admit in doing so the imperfection of our knowledge, and do not deceive others by suggesting that mechanical science could solve the secret of organised nature."

[1] 'Vorträge und Reden,' vol. ii. p. 179.

[2] Chr. Fried. Nasse (1778-1851), since 1822 professor at Bonn, where, together with Walther, Joh. Müller, and others, he cultivated the physiological method in medicine, "was, as it seems, the first German doctor in whose clinical institute physical diagnosis was introduced. From 1820 onward percussion was practised; since 1821 the stethoscope was regarded as an indispensable instrument" (Haeser, 'Geschichte der Medizin,' 3rd ed., Jena, 1881, p. 912). "The thermometer was first used extensively at the bedside by James Currie (1756-1805). His 'Medical Reports on the effect of water, cold and warm, as a remedy in fever and other diseases,' London, 1797, "contains observations on the variations of the body-temperature. . . . But these attempts had little success. Not till the middle of the nineteenth century was the importance of medical thermometry recognised, first through the classical work of von Bärensprung (1851), then through that of Traube, but mainly through Wunderlich" (ibid., p. 930).

mechanical devices which a physician with a clear mental vision did not require: moreover, the patient would thereby be degraded and treated as a machine. Feeling of the pulse was the most direct method of ascertaining the reactive power of the vital forces, and was delicately practised as the most important process. Elderly practitioners considered counting with a second-watch as hardly good taste: taking the temperature was not thought of. As to the eye-mirror, a highly celebrated surgical colleague told me he would never use the instrument, it being dangerous to throw brilliant light into suffering eyes: another declared the mirror might do well for oculists with poor sight; he himself possessed very good eyes and did not need it. . . . A celebrated professor of physiology had an argument with his colleague in physics regarding the images in the eye. The professor of physics invited him of physiology to come and see the experiment. This was indignantly refused: a physiologist should have nothing to do with experiments, which might do well enough for a physicist."

The first great attack upon the organic system of forces, upon the citadel of life, was made by chemistry, and was led by Lavoisier and the great school of chemists which continued his work. It consisted in the application of the theory of combustion, in which oxygen played such an important part, to the processes of respiration, 14. Attack from the side of chemistry.

nutrition, and the generation of animal heat.[1] Already in 1783 Lavoisier and Laplace had presented a memoir to the Paris Academy of Sciences, in which they attributed the generation of animal heat mainly to a process of combustion which took place by the conversion of oxygen into fixed air during the process of respiration. Lavoisier continued his researches on these and other similar physiological processes, such as perspiration, along with Séguin. They presented a joint memoir on the subject in 1790. It is also known, through the posthumous publication of Lavoisier's scientific papers in 1862, long after Liebig had brought out his series of researches on this matter, that the former had entertained very correct views on the economy of organic life as it exists in the balance of the animal and vegetable creations. After Lavoisier, the application of the new science of chemistry to questions of the individual and collective life of organisms was extended in a series

[1] The two great discoveries of oxygen and of the electric current at the close of the eighteenth century were not long in being applied to the reform of medical doctrine. In both instances exaggerated theories were not wanting. Fourcroy, himself a medical student by profession and one of the most ardent followers and promoters of the new chemistry, who, moreover, edited a journal with the title 'La médecine éclairée par les sciences physiques' (1790-92), found it nevertheless necessary to give warning against the premature introduction into medical teaching of the new ideas of chemistry. Of this many instances existed, both in France and Germany, such as the 'Essai d'un système chimique de la science de l'homme' (1798), by J. P. T. Baumes of Montpellier, against which Fourcroy aimed his criticisms in a letter to Humboldt. On these extravagances see Haeser, 'Geschichte der Medicin,' vol. ii. p. 737, &c.; also Dr A. Hirsch, 'Gesch. d. medicin. Wissenschaften in Deutschland' (München, 1893, p. 567). There is no doubt that opposition to this one-sided application of some chemical or physical theory, or of some special therapeutic method, which might be valuable to a limited and restricted degree, partly accounted for the fact that the more thinking members of the profession clung to the notion of a vital force or principle, as yet undefined but nevertheless existent.

of very valuable but unconnected researches in all the different countries where chemistry was cultivated. Priestley, in England, had noticed the purifying effect of plants on air; De Saussure, in a series of remarkable experiments, carried on in the last years of the eighteenth century at Geneva, established the fact that in sunlight plants increase the quantity of carbon and other constituents in their tissues. Ingenhousz in Holland and Senebier in France had shown that in the presence of sunlight bubbles of oxygen gas are given off by plants when plunged under water, and had traced this oxygen to its source, the carbonic acid in the atmosphere. Sir Humphry Davy had applied chemistry to agriculture; and, much later, German physiologists like Tiedemann and Johannes Müller had recognised the necessity of explaining the processes in the living body chemically. All these labours, however, were detached, and their value was little known. It was therefore a very timely proposal which issued from the British Association in 1839, that a report on the present state of organic chemistry should be drawn up. For this task no less a person than Justus Liebig was selected.[1] The event

[1] The sources of information on Liebig's great work in revolutionising the science of life through his application of organic chemistry to agriculture and physiology are numerous. In particular there are two addresses by Vogel and von Bischoff, delivered in the Munich Academy in 1874, Hofmann's "Faraday" lecture, delivered in the Royal Institution in 1875, and a very able summary, drawn mainly from these sources by Mr W. A. Shenstone, in Cassell's 'Century Science' Series (1895), entitled "Justus von Liebig, his Life and Work." Bischoff's address contains a very full discussion of Liebig's vitalistic sympathies. His great influence was established as much by his special scientific discoveries as by his method of teaching,—by his early attempts to popularise science and make it an educational power through his well-known 'Familiar Letters.' He was in this respect a pioneer, as after him Helmholtz and Du Bois-Reymond were pioneers in spreading scientific ideas by means of popular lectures and addresses.

marks an epoch equally in the science of organic chemistry proper and in the life-work of Liebig. The necessity of collecting and systematising the scattered labours of chemists and physiologists in this department was simultaneously felt in France, where Liebig's friend and rival, Dumas, published his 'Essai de Statique chimique des Êtres organisés' as a conclusion to his course of

Liebig broke through the barriers which in his age separated science in Germany from general culture, and the university professor from the man of the world. From France he learnt the merit of a clear style, and from England the higher art of popularisation. His fame did not grow slowly and surely like that of Helmholtz, spreading almost imperceptibly from narrower into ever wider circles: he took the world by surprise, and stirred up everywhere inquiry, opposition, and controversy. He ventured on great and sweeping generalisations and on daring experiments and prophecies, with the result that in the final establishment of truth his opponents had frequently as great a share as himself. Notable instances are his so-called "mineral theory" of manuring and his theory of fermentation. Through the former the great division which separated the processes in the living from those which obtained in the inanimate (mineral) world was broken down; and through the latter the modern notions of the ubiquity and continuity of life were to a large extent established, as will be seen in the sequel of this chapter. The correct notions which he entertained as to the necessity of the mineral ingredients (phosphoric acid, lime, potash, &c.) in plant-manures, which he started in opposition to the older "humus" or "vegetable mould" theory, was on the point of being refuted by his insistence on making his chemical fertilisers insoluble, ignorant as he then was of the absorbing and retaining function of mould; but, a generation after, the prevailing predilection for soluble manures was again much modified by the introduction of the "Thomas slag," and the enormous improvements in the process of extreme pulverisation. Prof. Vogel in his above-mentioned address gives many extracts from Liebig's writings, referring to the final and corrected expression of the chemical theory of fertilisation. These are so characteristic of Liebig's habit of thought and his whole mental attitude, that I transcribe them: "When I knew the reason why my fertilisers would not act, I felt like a man who had received a new life, for through this all processes of agriculture were explained, and now that the law is known and lies clearly before our eyes, there remains only the wonder that we did not see it long ago: but the human mind is a queer thing,—what does not fit into the circle of ideas once given, does not exist for it. . . . I had sinned against the wisdom of the Creator, and for this had received merited punishment. I wanted to improve His work, and in my blindness I thought that in the wonderful chain of laws which bind life to the surface of the earth, one link was missing which I, a helpless worm, could supply" (*loc. cit.*, p. 34).

chemistry at the Medical School of Paris in 1841. With him was associated Boussingault, the man who, next to Liebig, did most for the elaboration of the true principles of agricultural chemistry.

To Liebig, organic chemistry did not mean the chemistry of the carbon compounds as it is defined nowadays, and has largely become since Dumas himself introduced into science the fruitful method and idea of substitution. This idea extended the facilities of the laboratory chemist enormously,[1] but also marks the altered view which has 15. Change in organic chemistry.
since taken hold of organic chemistry, the alliance with arts and industries rather than with an understanding of the economy and the phenomena of living organisms. From the moment of that alliance dates the division of organic chemistry into the two great branches of the chemistry of carbon compounds and the chemistry

[1] It is well known that organic chemistry during Liebig's lifetime outgrew the canons and the circle of ideas in which he moved, and that he complained of not being able to understand the papers in his own periodical, the 'Annalen,' &c. Liebig originally opposed Dumas' ideas on substitution, but in the end admitted himself defeated, when, through Hofmann, he became convinced "that the character of a chemical substance does not depend so much as he had supposed on the nature of its constituent atoms, and depends very largely also on the manner in which these atoms are arranged. Some years afterwards, at a dinner given by the French chemists to chemical visitors to the Exhibition of 1867, Liebig made his defeat on this occasion the source of a happy retort to Dumas, who had asked him why of late years he had devoted himself exclusively to agricultural chemistry. "I have withdrawn from organic chemistry," said Liebig, "for with the theory of substitution as a foundation, the edifice of chemical science may be built up by workmen: masters are no longer needed" (Shenstone, 'J. von Liebig,' 1895, p. 61). Already, in 1838, Liebig and Wöhler, in their investigation on uric acid and its derivatives, prophetically suggested the twofold development which organic chemistry was destined to take: "From these researches the philosophy of chemistry must draw the conclusion that the synthesis of all *organic* compounds which are not *organised* must be looked upon not merely as probable, but as certain of ultimate achievement" ('Annalen,' &c., vol. xxvi. p. 242). In fact, we have now a chemistry of organic and one of organised substances.

of organised nature. From this organic chemistry of the modern school Liebig turned away—continuing to lead research in the older and less fashionable direction. This choice is explained by the peculiarity of his great mind, which, while investigating details, never lost sight of the organic whole of natural processes, and which allowed itself many a flight of imagination into unexplored regions. In fact, if we review the work of Liebig from the side of the history of thought rather than from that of science, we must assign to it a very great and lasting influence. He was probably the first man of science who conceived the twofold meaning which belongs to the words, life and organism, a meaning which was known and appreciated by practical men, but which had, at that time, hardly received scientific recognition.[1] Life is not only defined, as Bichat put it, by the contrast with death; it is just as much defined by the idea of co-operation or solidarity: life is not only the property of individual beings, but also of the collection or society of several individuals in a larger organism. As such, political economy had conceived it long before Liebig's time, but Liebig was probably the first scientific thinker who studied the economy of nature, who fully realised the interdependence of animal and plant life, and tried to reduce this larger life of living things to scientific data and laws. Through him and his school two terms have become current in scientific and popular literature which, especially in the

16. Influence of Liebig.

[1] The idea of the dependence of living things on the environment, on the "milieu," was indeed fully recognised and emphasised by Lamarck (see p. 314 *supra*); but the philosophical ideas of that great thinker were then unknown and disregarded.

German tongue, have characterised the new ideas then introduced into science, and have brought them home to the intelligence of the educated classes. These two terms have only been inadequately rendered[1] in any other modern language: they are the words "Stoffwechsel" and "Kreislauf des Lebens." The former denotes the continual change of matter connected with maintenance of form in all living things; the latter denotes the continual interchange which exists between the separate members and the different provinces of the living creation, the circulation of living matter and living processes. Liebig looked upon nature on the large and on the small scale as an economy, as a household, and he applied himself to study the conditions of its existence, of its normal and abnormal states. Through Liebig chemistry entered into close alliance with political economy, or, as it is termed abroad, national economics.

17. "Stoffwechsel" and "Kreislauf des Lebens."

We shall see immediately how the progress of science has, in the further course of the century, tended to emphasise this twofold aspect and define it more clearly; how the individual organism, the bearer of life, has been traced to smaller and smaller dimensions and units, and how, correspondingly, life as we see it on the larger scale has more and more revealed itself as consisting in co-operation, in the collective action of societies made up of individuals. We have on the one side the doctrine of the "Autonomy of the Cell," so eloquently proclaimed by Professor Virchow; on the other side the doctrine of

18. "Autonomy of the Cell."

[1] We shall see farther on how the word "Metabolism," with its two subordinate terms "Anabolism" and "Catabolism," is even more expressive than the German term "Stoffwechsel."

“Division of Physiological Labour.” the “Physiological Division of Labour,” the happy expression invented by the great French zoologist, Henri Milne-Edwards.

Whilst Liebig was working at the great problems of the economy of life, and making chemistry subservient to the interests of agriculture, physiology, and pathology, another influence was exerted—mainly in Germany—on the study of the processes which take place in the living organism. This influence had its source in an application of the principles of dynamics and the more modern teachings of physics.[1] It emanated from two distinct centres—from Leipzig, where the brothers Weber[2] taught how to

[1] In many passages of his interesting and brilliant “Addresses” Du Bois-Reymond has dwelt on the great revolution which came over physiological studies about the middle of the century, characterising it as a special German achievement. Claude Bernard has given us an interesting account of a corresponding, but not identical, change of ideas in the great medical schools of Paris. Quite recently Sir Michael Foster has created in this country an interest in the history of medicine, notably of physiology, and has on various occasions given us masterly summaries of the results of his historical research. I may refer specially to his very lucid and fascinating monograph on Claude Bernard (London, 1899, in Fisher Unwin's ‘Masters of Medicine’ Series). Another authority in modern physiology, Prof. M‘Kendrick of Glasgow, has treated in a companion volume of Helmholtz, dwelling mainly on his physiological labours, based upon his brilliant application of physics and mathematics. The two monographs exhibit very clearly two distinct influences which have been at work in remodelling the science of physiology and the conceptions of the phenomena of life.

[2] Regarding the position and influence of the three brothers Weber, I may refer to former passages of this history (vol. i. p. 196; vol. ii. chap. vi. *passim*). The greatest of the three—Ernst Heinrich Weber (1795-1878)—occupies a unique position in the development of the “science of life” in Germany. He seems never to have come under the influence of the then prevalent “philosophy of nature,” and he had accordingly, unlike Liebig and Johannes Müller, nothing to unlearn. See on this point Du Bois-Reymond's Éloge of Müller in ‘Reden’ (vol. ii. p. 216), also Ludwig's Éloge of Weber (Leipzig, 1878, p. 10). Weber represents in the purest form the influence which physics, based upon experiment and measurement, had upon the development of the study of organic form and function, as Liebig represents in the purest form the influence of chemical research and reasoning. In this respect Liebig was more nearly related to the Paris school, Weber to the Berlin school, which he greatly influenced.

apply strict experimental research, combined with actual measurements, to physical, organic, and psychical phenomena, which had so far escaped all exact treatment; and from Berlin, where in the person, and still more in the school, of Johannes Müller, the great and complex 19. Johannes Müller.
phenomenon of life in the higher organisms was analysed into various mechanical and physical processes, each connected with some well-defined organ which was more and more recognised as possessing the properties of a physical apparatus. A great deal of the work of the numerous members of this school consisted in unravelling with the microscope the structure of such organic apparatus, and studying its action by physical measurements and experiments. As examples and models of this kind of work we have Du Bois-Reymond's ‘Researches in Animal Electricity’ (1848), and Helmholtz's ‘Physiological Optics’ (1867, second edition, much enlarged, 1896), and ‘Physiological Acoustics’ (1862). In the course of these labours it was found that the older ideas of “Stoffwechsel,” and the conception of the circulation of matter as it was taught in the school of Liebig, required to be corrected and extended. I have referred in an earlier chapter[1] to the interesting circumstances under which our modern notions of the conservation of energy first dawned independently upon Mayer and Helmholtz whilst studying the phenomena of heat in the animal organism. In the school of Liebig we meet with an occasional attempt to extend the idea of “Stoffwechsel,” the exchange of material or of elementary matter in the living body of animals and

[1] See *supra*, chap vii.

plants, so as to embrace likewise the imponderables—heat, light, electricity, &c. We find Mohr treating of heat and animal energy as substances which must be counted among the elements or prime materials known to chemists—just as the French chemists of Lavoisier's school enumerated the imponderable along with the ponderable elements of nature: even Liebig's first edition of the 'Chemical Letters' is not quite averse to such an interpretation. The ideas on this matter were, however, vague, and needed defining. When Mayer attempted a first step in this direction, Liebig did not see the value of it. The subject was only cleared up when Helmholtz, in 1847, showed that all so-called living forces were the different manifestations of a certain quantity of power to do work — later termed energy — and that this power could show itself in actual change and motion, or be stored up in tensions in the system, later called "potential energy." After this, "Stoffwechsel" appeared not only as an exchange of material, but also as a change in the form of energy, whereby potential or latent energy could be accumulated in the organism and let loose, as the latent power of an explosive substance is let loose by the pulling of a trigger.

One of the immediate consequences of these varied researches—all tending to show how the conception formerly established in chemistry, physics, and dynamics could be utilised in the description of the phenomena of living matter, how the complex phenomenon of life could be split up into a number of separate chemical and physical processes, which could be imitated in

the laboratory, and how the living organism could be analysed into a complex of separate apparatus or machines, acting on intelligible mechanical and physical principles—was a radical change of the conception of vital force and the vital principle. It ceased in the opinion of many to be opposed to other non-living forces, as it was with Bichat; according to others it was non-existent, or at all events useless; others again reduced it to a purely regulative function, or even a mere idea. A popular philosophy founded upon the unknown principle of matter, and the equally unknown and even less clear principle of force, promulgated the notion that science had succeeded in banishing all spiritual entities, and was able to explain everything on purely mechanical principles. Vitalism and animism were at an end; there only remained mechanism and materialism. It is well to note that none of the great men to whom we are indebted for the real extension of our knowledge of biological phenomena favoured or embraced this view. The reasons which kept them from drawing what seemed to some the inevitable consequences of their discoveries were manifold.

20. Influence of doctrine of energy.

21. Mechanism.

As I stated before, there are two ways of approaching the problems of nature, and two interests by which our researches can be guided. The one is the abstract mathematical method, which begins with the simplest definable and measurable elementary processes, and tries to imitate the complicated phenomena of nature by more and more intricate combinations of these elementary processes. The other is the more concrete method inspired by practical interests. The mechanical, physical, and

chemical methods of analysis and synthesis follow the former way, and they generally arrive at satisfactory explanations of isolated parts of the actually existing phenomena, or of special and simple cases. Notably, they create the artificial world of manufactured things, such as instruments, machines, chemical and mechanical compounds. They may at times make it appear as if this process of putting together, continued indefinitely, would ultimately reach the real things which we behold in inorganic, organised, and even in animated nature. At all events no other way, it might seem, is open to science, and the only thing that delays our progress is the bewildering intricacy and complexity of things natural. At the beginning of our century, when, through Laplace and his school, many seemingly complicated phenomena of nature, notably those of physical astronomy, yielded to the processes of analysis just described, there seemed for the moment a possibility of building up a complete philosophy of nature on such a groundwork. Laplace himself indulged in a frequently quoted prophetic vision of this kind. When, in the middle of the century, some molecular phenomena, notably those of light, had likewise yielded to the calculus, and when correcter views as to the nature of forces had further brought another and different world of phenomena into a calculable form, it seemed likely that even the mysterious processes of living organisms might be subjected to similar reasoning. It seemed time to abandon the familiar conception of a special vital force, and to hand over physiological problems likewise to the physicist, the chemist, and the microscopist. A regular

crusade was accordingly started in Germany by philosophers, as well as by naturalists and biologists, against the vitalists—those who believed in a special principle of life; and an impression was created in the minds of thinking outsiders that a purely mechanical explanation of life and mind was finally decided on, and within possible reach. Among those who assisted in bringing about this impression, I need only single out two names—those of Hermann Lotze,[1] the philosopher of Göttingen, and of 22. Lotze and Du Bois-Reymond.

[1] The position which Lotze occupies in the history of the conceptions of life or of vitalism is peculiar. If we read works dealing specially with the history of medicine, such as those of Haeser or Hirsch, we do not come across the name of Lotze at all, and it is only in quite recent times, fifty years after the appearance of Lotze's writings dealing with vitalism, that experts in physiology have reverted to his discussion of the subject. See notably the following: 1. Rauber, "Formbildung und Formstörung in der Entwickelung von Wirbelthieren" ('Morphol. Jahrbuch,' Band vi.), 1880. 2. Wilhelm Roux, "Einleitung zu den Beiträgen zur Entwickelungsmechanik des Embryo," 1885 (reprinted in 'Gesammelte Abhandlungen,' vol. ii. p. 11, Leipzig, 1895). 3. O. Hertwig, 'Zeit und Streitfragen zur Biologie' (Heft 2, Jena, 1897), pp. 23-29. 4. Carl Hauptmann, 'Die Metaphysik in der modernen Physiologie' (Jena, 1894), p. 3. These and many other recent references go back to Lotze's article, "Leben und Lebenskraft," in Rud. Wagner's 'Handwörterbuch der Physiologie,' 1842; and to his larger publications, 'Allgemeine Pathologie und Therapie als mechanische Naturwissenschaften' (Leipzig, 1842), and 'Allgemeine Physiologie des Körperlichen Lebens' (Leipzig, 1867). The reasons why Lotze's expositions were so little regarded at the time were probably twofold. He taught that the phenomena of life constituted a mechanical problem. This was enough to dismiss in the eyes of many empirical naturalists the further, but not easily comprehended, statement of Lotze that life was not merely a mechanical problem. The definition and solution of the second part of the problem was much more difficult, aud Lotze delayed his expositions on this side of the question for ten years, when he published his 'Medicinische Psychologie oder Physiologie der Seele' (1852), which contained a metaphysical introduction apparently little in harmony with the supposed purely mechanical or even materialistic standpoint of his earlier writings. In the meantime several important works had appeared which carried out in wider or narrower regions the purely mechanical or inductive and experimental treatment, and quite revolutionised physiological and medical studies. I need only mention such works as Jacob Henle's 'Allgemeine Anatomie' (1840), and his 'Handbuch der rationellen Pathologie' (1846-53). Henle, as von Kölliker

Du Bois-Reymond, the eminent physiologist of Berlin. The former owed much of his scientific training to the school of Ernst Heinrich Weber in Leipzig, the latter to that of Johannes Müller in Berlin. Both agreed in denouncing the conception of a vital force—as it was then called—as illogical, and moreover as scientifically useless. But whilst Lotze distinctly stated that his criticisms on this subject were only addresssd to scientific thinkers, and promised a further philosophical

says, "correctly saw that the work of Bichat had to be remodelled on the foundations laid by Schleiden and Schwann," an undertaking in which von Kölliker himself laboured with the greatest success. But above all must be mentioned the appearance of Rud. Virchow's 'Cellular Pathology' (1858, Engl. transl. by Chance, 1860), "in which he himself explains that he does not give a system but a general biological principle," and in so doing lays the foundation for the entire exact treatment of pathological cases. It is, however, well to note that Virchow does not regard life as a purely mechanical problem. The works of such authorities as Henle and Virchow give as much or as little philosophy and discussion of general principles as physiologists of the exact school required for about thirty years. Those masters, indeed, had themselves grappled with the philosophical problem, and had arrived at a formulation which sufficed to lead research into fruitful paths for a new generation of experts who themselves were not philosophically educated. The term vital force disappeared, and in the specialist medical literature of a lengthy period even life itself was hardly any longer discussed. Thus a firm basis was laid on which mechanics, physics, and chemistry could be usefully applied. A similar silence as to general problems reigns in the great school which for two centuries built on the principles laid down by Newton in natural philosophy. Similarly in chemistry, the foundations laid by the atomic theory sufficed for the greater portion of the century following its enunciation. We have seen in earlier chapters of this work how, even in these much more firmly established mechanical sciences, our century has witnessed before its end discussions again arising as to fundamental questions and leading principles. A similar fate has come over biological science, and with it a renewed interest in the writings which stand at the entrance of that epoch which was so rich in the unravelling of definite and special problems. Authorities like Prof. O. Hertwig warn us now of that "other extreme which sees in vital processes nothing but chemico-physical and mechanical problems, and thinks it finds the true science of nature only in so far as it is possible to reduce phenomena to the motions of attracting and repelling atoms, and to submit them to calculation" ('Die Lehre vom Organismus,' an Address, Jena, 1899, p. 8).

investigation of the question, Du Bois-Reymond [1] gave the impression, in his earliest deliverance, that the

[1] Du Bois-Reymond's position in the vitalistic controversy is interesting and instructive, inasmuch as he considerably modified his opinions in course of time. His first deliverance on the subject is to be found in the preface to his celebrated 'Untersuchungen über Thierische Elektricität' (March 1848). This discussion of the subject followed soon after the deliverances of men like Berzelius (1839), Schwann (1839), Schleiden (1842), Lotze (1842), on the same subject, which are stated to have been "ineffectual." After the lapse of twenty-four years Du Bois-Reymond approached the subject again in his celebrated address at the German Association of Sciences at Leipzig, 1872, entitled "Ueber die Grenzen des Naturerkennens." This deliverance created a great sensation: the pamphlet appeared in many editions and translations, and only in this country failed to get adequately noticed. A further explanation of the views expounded in it was given by the author (1880) in an oration at the meeting held annually in honour of Leibniz in the Berlin Academy on the 8th of July. It bears the characteristic title "Die sieben Welträthsel." These documents together contain the author's "philosophical creed," which ends in "Pyrrhonism," out of which there seems no escape except through "Supernaturalism," which, however, begins where science ends. (See note 1 to the last-mentioned address.) All three documents are reprinted in the two volumes of 'Reden' (Leipsic, 1886-87), from which I quote. In the interval of a quarter of a century which lay between the first and second deliverance great changes had come over scientific thought. The mechanical view, which had been put forward in an extreme form in 1848, when it was prophesied that "physiology, giving up its particularistic interest, would disappear in the great united estate of natural philosophy, would be entirely dissolved in organic physics and chemistry" (vol. ii. p. 23), had had time and opportunity to show its power and its limits. It had gained through greater facility of application (such as Ludwig's automatic curve-plotting), through the larger conception of "Stoffwechsel" as denoting "metabolism" of matter and energy. The author himself had introduced a new definition of life as a "dynamical equilibrium" in the place of older descriptions (vol. ii. p. 25); and, above all, Darwin had shown the possibility of a mechanical explanation of so-called "final causes" in nature. The author himself was one of that great school, emanating from Johannes Müller, but now represented by the still greater Helmholtz, which had pushed the mechanical or exact treatment to its furthest limits, to the analysis of the phenomena of the nervous system in its highest activity, those of sensation and perception. It is therefore highly significant that, instead of confirming the earlier dictum, that the exact treatment would halt only at the most advanced point—viz., the manifestation of "free will,"—the author is now forced to admit that not only is the "origin" of all motion quite obscure, but likewise the lowest forms of animation or consciousness are not to be explained mechanically,

question was definitely settled and the road quite clear for research. To those—and they comprised the second class of thinkers referred to above—who were unwilling or unable to follow Lotze and Du Bois-Reymond into the details of their criticism of the illogical conception of force as employed in the term "vital force," but who looked at the great facts of economy, design, and recurrent order which are exhibited in the living creation, these criticisms had little that was convincing. If the term "vital force" was illogical, some other term such as "vital principle" might be substituted. The enormous difference between the phenomena of living and of dead matter remained and impressed itself on them. Liebig, and many naturalists in France and Germany, had approached the study of nature from the practical side. Their methods were not mathematical but rather experimental, and very frequently not limited to the laboratory and dissecting-room, but carried out in the workshop of nature itself. In spite of his successful attempts to establish clearer views regarding the economic processes of living nature and the application of chemical analysis, Liebig[1] to the end

the mystery which attaches to all beginnings as well as to the great transitions in the ascending scale of natural phenomena being further emphasised and forcibly driven home in the last-named address, which, as has been said, bears the title "The Seven Enigmas." The three deliverances of Du Bois-Reymond, together with the copious notes and references which he gives in the latest reprint, serve as a very good and lucid exposition of the inherent difficulties of the problem, and should be studied by every one who desires to be at home in the ancient and modern literature of the subject. The position of the author has been many times criticised. See, *inter alia*, Kaufmann, 'Die Metaphysik in der modernen Biologie' (Jena 1894), *passim*.

[1] Lord Kelvin in his essay "On the Dissipation of Energy" (reprinted in 'Popular Lectures,' &c., vol. iii. p. 464) has the following interesting note: "The influence of animal or vegetable life on matter is infinitely beyond the range of any scientific

of his life never satisfied himself that the phenomena of life can be mechanically explained: he remained, in the face of much criticism, a Vitalist. So did Wöhler in Germany—so did most of the eminent physiologists in France and in England. The crusade against Vitalism, which was started in Germany, seems to have had little influence on them. In 1854, six years after Du Bois-Reymond's essay on Vital Force, and twelve years after that of Lotze, Huxley[1] could still, in the first of his 'Lay Sermons' "On the educational value of the natural history sciences," express opinions on the difference between living and not-living bodies which were distinctly vitalistic, maintaining, much in the same way as Liebig did in the later editions of his chemical letters, that "the phenomena of life are dependent neither on physical nor on chemical, but on vital forces"; and if, in 1870, he could himself state that he had long since grown out of this view, it is interesting to discover what were the arguments which brought about this remarkable change. I will at once state what seems to me to be the great influence which combated Vitalism in this country, which greatly strengthened the anti-vitalistic or mechanical views in Germany, but which, as little as the methematical and philosophical criticism of Lotze and Du Bois-Reymond, ever took real hold of biological thought

23. Liebig's vitalism.

inquiry hitherto entered on. About twenty-five years ago I asked Liebig if he believed that a leaf or a flower could be formed or could grow by chemical forces. He answered, I would more readily believe that a book on chemistry or on botany could grow out of dead matter by chemical processes."

[1] The address referred to was reprinted in 1870 in the well-known volume, entitled 'Lay Sermons, Addresses, and Reviews,' with a "prefatory letter" to Tyndall, in which the following passage occurs: "The oldest essay of the whole contains a view of the nature of the differences between living and not-living bodies, out of which I have long since grown."

in France, where a modified kind of vitalism still prevails.[1] It is the far-reaching influence of the reasoning
24. Darwin. which sprang out of Darwin's theory of descent.

[1] The older ideas of vital forces have in all the three countries been combated by authorities of the very first order, but, characteristically, in a very different manner — the phenomena of living bodies having been attacked from different sides. In Germany the mechanico-physical school was for a time the dominant one. In France the dominant school was the so-called experimental, also termed the vivisectional, school, founded by Magendie. Between these two extreme positions, both equally opposed to the older vitalism, there stood in the middle, with a less strongly pronounced antagonism to earlier conceptions, those who, like Liebig in Germany, Dumas and Boussingault in France, approached the phenomena of life mainly by the methods and reasoning of the new science of chemistry. This school had a profoundly modifying influence on the extreme views of the experimental school in France. It made itself felt mainly through Claude Bernard. In Germany this influence was felt later, after that of Darwinism had somewhat subsided. In England it was the doctrine of descent pure and simple which combated the older vitalism: the question became one of origins, and vitalism, as such, could be temporarily ignored. The facts of variation, overcrowding, natural selection, and inheritance, presented such a mass of material, waiting to be sifted and arranged by exact methods, that the problem of the essence of life and its beginnings was set aside. Accordingly, the attempts both of Darwin and Huxley to grapple with the central and final problem of vitalism are very few; the latter only repeating what had been said long before him by thinkers of a very different school. The question was not answered, because, for the progress of the sciences and for their successful application in medicine, it did not require to be answered. It became a purely philosophical question, and the only English writer of authority who seriously grappled with it was Mr Herbert Spencer in his 'Principles of Biology.' Darwin in 1863 wrote to Hooker ('Life,' vol. iii. p. 18): "It is mere rubbish thinking at present of the origin of life; one might as well think of the origin of matter." Huxley, in a letter from the year 1884 ('Life,' vol. ii. p. 67), compares life with a whirlpool, a favourite simile of Cuvier's (see *supra*, vol. i. p. 129), but is doubtful as to comparing it with a machine. M. Delage names Chevreul ('Considérations générales sur l'analyse organique et ses applications,' 1824): "Il a eu le mérite d'écrire que la Force vitale n'explique rien, qu'elle aurait besoin elle-même d'être expliquée avant de prétendre expliquer autre chose, et que les phénomènes de la vie ont leur cause directe dans les principes immédiats constitutifs de la matière organisée. Il n'établit cependant sur cette donnée une théorie de la vie, car il conclut, au contraire, que, eût-on ramené les phénomènes vitaux à leurs causes prochaines et aux forces qui régissent la matière inorganique, on ne serait pas encore en état de comprendre comment l'être organisé en se reproduisant répète avec une constance si remarquable les caractères de son espèce." Even François Magendie, the great founder of the purely experimental school of physiology, says of Bichat's celebrated 'Recherches,' &c.: "Les esprits sévères et amis des progrès des sciences . . . ont regretté que l'auteur opposât sans cesse la vie aux lois physiques, comme si les êtres vivans n'étaient pas de corps, avant d'ètre des végétaux ou des animaux" ("avertissement" to the 4th ed. of Bichat's 'Recherches,' &c., 1822).

In order to enable my readers to comprehend clearly the great change which has come over biological thought through Darwin's writings and reasonings, I must now introduce an idea which I have so far intentionally avoided in discussing the various scientific views of nature. This is the idea of final causes, the apparent existence of a purpose (in German *Zweck*), or an end (in German *Ziel*) in all processes of nature, but pre-eminently in those of the living portion of creation. In all writings prior to Darwin a great deal is made of final causes in nature, of the teleology of living processes. The phenomena of life seemed safely intrenched in the citadel of final causes: no mechanism could explain them away. The very fact that organisms were compared with machines, admitted the existence of a definite end and purpose; for it is the peculiarity of every humanly constructed machine or instrument that it serves a definite purpose which, in the mind of the inventor or maker, suggested the peculiar arrangement or organisation which we behold. The criticisms of Lotze[1]

[1] The lengthy discussions of Lotze contained in the writings quoted above are not easy to understand, and it is not surprising that, beyond the elimination of the conception of vital force as useless to the purely scientific student, his real meaning was at the time not grasped at all. In fact, we may say that Lotze, though ceasing to be a vitalist, remained an animist. Discarding vital force, he retained the conception of a soul in a manner which drew upon him the ridicule of those whom, like Carl Vogt, he had converted to pure materialism. This has had the consequence, that in more recent times his whole philosophy has been stigmatised as dualistic, and that he has been accused of having halted half-way. His real meaning can be gathered more easily from his later and more mature writings: for his contemporaries it must have remained to a great extent enigmatical. See C. Haupt-

and Du Bois-Reymond[1] did not do away with this very evident property of living things, but only maintained

mann ('Die Metaphysik in der Physiologie,' 1894, p. 7): "However convincingly Lotze destroyed the conception of a vital force, he had no desire to criticise in a similarly destructive manner the principle of a soul, though both have grown up in the same climate, in the fertile country where substances blossom, &c. . . . And although he emphatically, and in many ways, urged that all organism is a definite form and arrangement of mechanism, he nevertheless accorded to the principle of inherent disturbances (soul, will) a partial control over the functions of the animal body," &c. Accordingly this view set only the physiology of plant-life quite free for a purely mechanical treatment, which it received — after the suggestive beginnings made by Schleiden — mainly at the hands of Julius Sachs, from whose 'Lectures on Plant Physiology' (1887) Kaufmann gives the following very characteristic extract: "The organism is only a machine put together out of different parts; . . . in a machine, even if only made by human hands, there lies the result of deepest and most careful thought, and of high intelligence, so far as its structure is concerned," &c. (p. 623).

[1] The two great facts which stare every unbiassed student of nature in the widest sense in the face, and which always upset a purely mechanical view, are Purpose and Will. Lotze recognises both, and in all his writings never forgets or ignores them. Naturalists, who for the nonce are deeply interested and fully absorbed in the analysis of some definite organ, or some special chemical power in the organism, may usefully ignore these two facts, of which the first only intrudes itself if we rise to a general, a comprehensive aspect; the second is a result of individual experience. Nor did Du Bois-Reymond ignore these facts. It is interesting to see how he deals with them in his earlier and later writings. In the earlier period he eliminates the problem of free will as not a scientific problem at all, and gets over the question of purpose by a reference to the evident existence of purpose in inanimate nature also, — an idea which really comes ultimately back to an assumption of a general animation of the whole of nature, such as has been maintained by many philosophers and naturalists in very various forms. See, for instance, the further remarks of Julius Sachs in the passage quoted above. But there is no doubt that this method of viewing the teleology of nature did not really satisfy Du Bois-Reymond, for in the reprint of his paper on vital force he refers to it as superficial ('Reden,' vol. ii. p. 26), having in the meantime adopted the explanation of Darwin, whose "highest title to glory" will, "so long as philosophy of nature exists," be this, that he to "some extent allayed the agony of the intellect that ponders over the problems of existence" ('Reden,' vol. i. p. 216). In 1887 he holds that what he wrote as late as 1859, before the appearance of the 'Origin of Species' — for instance his celebrated Éloge of Johannes Müller — is antiquated, though it still gives a valuable picture of the "tormenting confusion of those who could not free themselves from the embarrassing fetters of the fixity of species, the incompleteness of the palæontological records, and, more than all,

25. Lotze and Claude Bernard.

that this end or purpose was attained by purely mechanical processes, that no new force, called vital force, need be assumed to exist, that it was the adequate and sole object of science to disclose the mechanism by which the various ends of life were attained. The very idea of life, the vitalistic element or factor, was chased away beyond the region of the knowable, and remained merely an idea in the realm of thought, as it was for Descartes and Leibniz, and as it has remained, up to recent times, for von Baer and for Claude Bernard, and for all those who do not accept the Darwinian explanation. For Lotze, Du Bois-Reymond, and Claude Bernard[1]

of final causes; in one word, of all pre-Darwinian Darwinians" (vol. ii. p. 299).

[1] Du Bois-Reymond ('Reden,' vol. ii. p. 557) claims that the greater part of the progress in modern physiology belongs to Germany, in spite of the great talent and originality of Claude Bernard. He thus describes the different position of the three countries: "One branch of physiology especially emanated from Germany — general physics of muscle and nerves. Whereas in England experimental physiology lay fallow, while it moved in France in vivisection and zoochemistry, being held down in both countries by vitalism, German science was the first to advance to the investigation of the surviving organs, especially of the frog, looking upon them as apparatus built up by nature, extremely complicated, yet conceivably only machines." This was spoken in 1880. Since that time a certain change has come over physiological reasoning, notably even in the very centre of the physico-chemical school at Berlin. In 1899 Prof. O. Hertwig warns us of the other extreme, opposed to the older vitalism, "which would lead us to a one-sided and equally inadequate conception of the vital process . . . which would see in it merely a chemico-physical and mechanical problem, and would recognise the genuine science of nature only so far as it is possible to reduce phenomena to motions, . . . and to subject them to mathematical calculation" ('Die Lehre vom Organismus,' an Address, Jena, p. 8). How far Du Bois-Reymond in later years modified his earlier notions, we can to some extent see from his published addresses. We know that the French school, with Claude Bernard as its most illustrious representative, never fell into the mistake of looking at the living organism as a physico-chemical machine, and we may be inclined to attribute this to a large extent to those experiments on the living organism which were first instituted by Magendie, which, under the hands of Claude Bernard, led to the discovery of the action of the pancreatic juice, of the glycogenic function of the liver, of vaso-motor nerves, and of the effects of poisons:

purpose exists in nature, notably in living nature; it is its very characteristic, its definition—the very "quid proprium" of life,[1] but it is useless as a scientific conception. It remains a problem for the philosopher, but the

all of them epoch-making discoveries which revolutionised physiological science, and which it is difficult to conceive of as having been made without vivisectional methods. We have also a remark from the pen of the late Prof. Georg Wiedemann, that Helmholtz himself, that greatest master in the line of mechanico-physical reasoning on the processes and organs of the higher senses and the nervous system, foresaw the necessity of resorting for further progress to vivisectional research, to which he had a personal dislike. (See Wiedemann's Introduction to the third volume of Helmholtz's 'Wissenschaftliche Abhandlungen,' p. xxiv.)

[1] In many passages of his later writings Claude Bernard has dealt with the definition of life: most fully in the posthumously published volume entitled 'La Science Expérimentale' (3rd ed., 1890). He there arrives at the final statements (p. 207): "Je pense, quant à moi . . . que les phénomènes chimiques dans l'organisme sont exécutés par des agents ou des procédés spéciaux; mais cela ne change rien à la nature purement chimique des phénomènes, &c. . . . Les agents des phénomènes chimiques dans les corps vivants ne se bornent pas à produire des synthèses chimiques, . . . mais ils les organisent. . . . Parmi ces agents . . . le plus puissant et le plus merveilleux est sans contredit l'œuf, la cellule primordiale qui contient le germe, principe organisateur de tout le corps. Nous n'assistons pas à la création de l'œuf *ex nihilo*, il vient des parents, et l'origine de sa virtualité évolutive nous est cachée. . . . Il y a comme un dessin vital qui trace le plan de chaque être et de chaque organe; . . . ils semblent dirigés par quelque condition invisible dans la route qu'ils suivent, dans l'ordre qui les enchaîne. . . . C'est cette puissance ou propriété évolutive que nous nous bornons à énoncer ici qui seule constituerait le *quid proprium* de la vie, car il est clair que cette propriété évolutive de l'œuf, qui produira un mammifère, un oiseau ou un poisson, n'est ni de la physique, ni de la chimie. . . . En disant que la vie est l'idée directrice ou la *force évolutive de l'être*, nous exprimons simplement l'idée d'une unité dans la succession de tous les changements morphologiques et chimiques accomplis par le germe depuis l'origine jusqu'à la fin de la vie. . . . La forcela métaphysique évolutive par laquelle nous pouvons caractériser la vie est inutile à la science, parce qu'étant en dehors des forces physiques elle ne peut exercer aucune influence sur elles. Il faut donc ici séparer le monde métaphysique du monde physique phénoménal qui lui sert de base mais qui n'a rien à lui emprunter. Leibniz a exprimé cette délimitation dans les paroles: 'Le corps se développe mécaniquement, et les lois mécaniques ne sont jamais violées dans les mouvements naturels; tout se fait dans les âmes comme s'il n'y avait pas de corps, et tout se fait dans le corps, comme s'il n'y avait pas d'âmes.' . . . Nous dirons avec Descartes: *on pense métaphysiquement mais on vit et on agit physiquement.*"

naturalist may neglect it, or at best can only use it as an "heuristic" help, as an indication where to look for the special mechanical contrivances which he is trying to unravel. It seems to me that the position which such thinkers take up towards the objects or individuals of living nature is similar to that of a mathematical student who clearly comprehends the solution of an algebraical problem, but who himself would be unable to find it. He may all his life remain in this attitude without being able to find any solution himself: he has got complete hold of the mechanism, but not of the idea, of mathematical reasoning. The student of nature could thus hope eventually to understand the mechanism of life, but the idea is beyond his comprehension. This can be expressed by saying: the mechanism of life is ultimately comprehensible, though highly intricate; the idea of life is transcendental, incomprehensible. Let us not trouble ourselves about the manner in which life first originated, but let us study the mechanical processes by which it is maintained, by which its various ends are accomplished. Let us study the mechanism of the clock, though this may not tell us the story of its maker nor the process of its manufacture. Those who cling to the conception of a vital force or principle would probably not even admit as much as this. It is doubtful whether Liebig to the end, whether Huxley in his earlier period, and Du Bois-Reymond in his later writings, would have admitted even this position.

26. Darwinism and final causes.

We are now prepared to understand the novel position which the Darwinian conception of natural processes introduced so far as the teleology of nature is concerned,

—how it dealt with final causes, with the apparent existence of a purpose, an end in the processes of nature, notably of the living organism.

It must here be remembered that the question how living things come to exhibit traces of design and purpose has really nothing to do with the nature and processes of life: it is not necessarily a biological question. Every machine shows the same marks of design, but is not therefore alive. The influence of Darwin's principle of natural selection, of overcrowding and consequent struggle for existence and survival of the fittest specimens, has therefore not been in the direction of explaining any of the vital processes which are at work in the individual organism. It is at best merely a statistical relation, a peculiar phenomenon occurring only in a large or congested group of living and self-multiplying beings: it presupposes the facts of reproduction, heredity, and variation; it does not explain them. Hence I dealt with Darwin's ideas in the last chapter, and did not introduce them under the present heading of Biological Thought. As we shall see later on, Darwin did recognise the necessity of attempting also a biological explanation.

The possibility of explaining the marks of design as merely apparent depends on the conception of the genetic process acting on a large, a gigantic scale: individual things put forth ever new developments by which they eventually overtop their neighbours, ultimately advancing to such a degree of excellence and individual perfection that to an outside beholder the few surviving specimens give the impression of having been originally designed. In fact, they only exist because those numberless individuals which could not grow in a sufficient degree perished in the struggle. Only those individual specimens survived in whom, in one or a few directions, something specially excellent was produced at the expense of development in other directions. In the mass, the crowd are sacrificed—*i.e.*, automatically crushed, in favour of the few: in the individual, one special growth is automatically pursued at the expense of a general but less enduring—*i.e.*, self-assertive—development. The end—the seeming purpose—is produced in the process of production, it being merely something more enduring—*i.e.*, something better. It conveys the impression to an outside beholder of having been consciously set at the term of the process of development; in reality it was produced simultaneously. The mountain peak which towers above its neighbours, and gives a distinctive rounding off and finish to a landscape, may be conceived as having been built up by the selective action of the natural artist who brought together the best materials and placed them in their most enduring positions: in reality it owes its existence only to one out of the numberless throes of nature which happened to take place with stronger materials and in more stable forms of arrangement and grouping, or it is due to the denudation of the strata surrounding it. The end and purpose of any natural development is that which it can itself automatically produce and endow with most distinctive and enduring characters, for this only survives at the expense of weaker productions: there is a natural result in development, but there need not be a purpose. The

27. "Natural result" against "purpose."

contemplation of the result may permit us to trace backward the process by which it was brought about; but we are not warranted in assuming that it existed independently, like the plan of a building or the purpose of an instrument. In the place of a growth according to a prearranged plan, Darwin put the conception of an automatic adjustment called "natural selection"; in the place of a conscious end or purpose he put the conception of a mere result, a product, the "surviving fittest."[1]

The development and proof of Darwin's ideas gave a new impetus to biological research, as it did also to the science of the history and economy of nature. The fact that the phenomenon of selection, or rather of automatic crowding out, presupposes intimate relations and contact of every living thing with numberless other similar and dissimilar beings, led naturalists into the open air, to

[1] A very full appreciation of the great change that has come over the sciences of nature through the influence of Darwin will be found in the various writings and addresses of Prof. Haeckel, notably in his address to the German Association in 1877 at Munich, "Ueber Entwickelungslehre" (reprinted in 'Gesammelte populäre Vorträge,' vol. ii. p. 97). A more critical examination, referring specially to the central biological problems, is the address by Du Bois-Reymond, delivered in 1876 in the Berlin Academy, and reprinted in 'Reden,' vol. i. p. 211, with valuable literary notes. He there discusses how far the principle of natural selection, in addition to the general doctrine of descent, has been adopted or opposed, and refers to the outstanding difficulties. "One of the greatest difficulties," he says (p. 226), "presents itself in physiology in the so-called regenerative power, and—what is allied to it—the natural power of healing: this may now be seen in the healing of wounds, in the delimitation and compensation of morbid processes, or, at the farthest end of the series, in the re-formation of an entire fresh-water polyp out of one of the two halves into which it had been divided. This artifice could surely not have been learnt by natural selection, and here it appears impossible to avoid the assumption of formative laws acting for a purpose. They do not become more intelligible by the fact that the regeneration of mutilated crystals, observed by Pasteur and others, points to similar processes in inanimate nature. Also the ability of organisms to perfect themselves by exercise has not found sufficient appreciation with regard to natural selection."

outdoor research, into the arena of real life. On this I dwelt in the last chapter. Ideas of a cognate kind had already emanated from other schools, such as that of Liebig,—the circulation of life in the different provinces of nature, the interdependence of different species of living things. Isolated investigations, like those of Gärtner and Sprengel, of Huber and Lubbock, on insect life, or of bacteriologists like Pasteur and Boussingault on fermentation and fertilisation, received a fitting place as important chapters in the economics of nature. The problem of life became twofold—the life of the community and the life of the individual: organisation and individuation. Two great questions presented themselves: What is an individual? what is a society of individuals? Physiologists were from of old accustomed to ask the former; economists like Rousseau and Adam Smith had asked the latter question. Both now became questions for the biologist. Physiology and economics joined hands. In isolated instances, as in those of Liebig and von Baer, these two interests had already been united. The real meaning and reason of this union now became clear to every one: it revealed itself as founded on the two characteristic features of life—individuality and co-operation. With the exception of the strong emphasis put by Liebig on the latter side of natural, notably organic processes, biologists before Darwin had mainly studied the phenomena of individual life. In two special directions—in embryology and in the cellular theory—they had made great progress. I have already treated of these advances in their bearing upon morphology, the study of forms, and upon genesis, the study of change

28. Organisation and individuation.

29. Biology and economics.

and development. Let us see how they affected biology proper—the study of life.

The early propounders of the cellular theory were evidently much influenced by the then existing theories which explained the constitution of inorganic chemical substances by atoms and by the processes of crystallisation. The progress of science, however, was in the direction of showing more and more that these borrowed conceptions are quite inadequate. Reasoning or thinking on organised matter is quite different from that which refers to unorganised substance. Chemists and physicists deal with atoms as imaginary units, which form the ideal groundwork for constant arithmetical proportions or for the action of calculable mechanical forces measured by observable movements. Biologists, whether dealing with plants or animals, approach the cells which they regard as the units of living matter with the microscope—an instrument which, till quite recently, has only been sparingly used in chemical research. The units of the chemist far transcend our powers of vision; the units of the biologist are to some extent accessible to our senses. All organisms have been found to be analysable by the aid of the microscope into similar morphological constituents called cells, which present very similar forms and functions. This has had the advantage of permitting the phenomena of life to be analysed into a few fundamental processes common to all living things; the great diversity of the larger organisms, and the more conspicuous phenomena of life, being conceived as put together in various ways out of these elementary units, which exhibit in varying degrees

of intensity the living processes common to all. Just so a state or human society is made up of a large number of individuals, all having the same human nature, who carry on the different functions peculiar to each with varying degrees of efficiency. The conception of the cell as the unit or type of all living matter, and the further discovery that there exist unicellular beings which are not essentially different from the constituent living elements of the most complicated organisms, has brought physiological research to a focus. The difficulties in the study of biological phenomena have vanished as those of the organic chemist did on the introduction of the conception of valency, of the saturating powers of chemical substances. Accordingly, if we compare a text-book of these subjects written in the middle of the century with one belonging to the latter part of it, we find an enormous difference of treatment. It is instructive to contrast the introduction given in Johannes Müller's 'Elements of Physiology' and that of Professor Michael Foster's 'Text-book.' The former represents the most advanced knowledge obtainable at the end of the thirties—the latter that of a generation later. The former contains a lengthy introduction on general physiology—the latter a short one on the physiological properties of a living amœba,[1] a

30. The cellular theory.

[1] Already, in 1835, K. E. von Baer pointed out how the study of one small animal can revolutionise our entire reasoning. "Ninety years ago a naturalist discovers the hydropolyp, an insignificant slimy animal, not larger than a peppercorn, and how, without head, sense-organs, muscles, nerves, blood, and sexual organs it nevertheless is nourished, grows, feels, moves, and multiplies,—how it can even be divided, each part forming a whole: he observes it with much wonder for nine years with untiring perseverance. At that time many would, no doubt, consider such an occupation childish and unworthy, yet these diligent observations have slowly but ma-

unicellular organism which is taken as a type, a model of all the phenomena of life. The former consists of philosophical and abstract generalisations, gathered from many sources; it treats of life in general, of the vital force, of the difference between animal and plant life, &c.: the latter sums up the whole matter of the treatise under a few heads, taken from the life of the simplest living thing. The generalisation has become an actual observable example. This transition from the abstract to the concrete, from the idea to the thing itself, is owing mainly to those definite conceptions which in Müller's time were being slowly elaborated: these were the cellular theory, the larger conception of "Stoffwechsel" as contained in the term "metabolism," and the conception of "differentiation of tissue" connected with division of labour. The two former are already very clearly foreshadowed in Theodor Schwann's microscopical researches; the latter takes us back to K. E. von Baer's embryological researches, to which the Darwinian idea of a struggle for existence, and the consequent tendency to one-sided development of form and function, have given an additional importance. Of the first and third of these definite modern conceptions I have treated above. The cell is the morphological unit of living matter. The process of differentiation was recognised

terially influenced physiology, the basis of medicine, and hence also the latter; and it is incalculable what many of those here present have gained through such influence in days of sickness or may still gain. Whoever carefully studies the development of physiology, will be convinced that it is mainly Trembly's observations of the hydro-polyp that have changed the former aspect of things, and that the transformation of the general views of life has altered the theory of sensation, circulation, &c., very materially, and is still active" ("Blicke auf die Entwickelung der Wissenschaft," an address, reprinted in 'Reden,' vol. i. p. 109).

in the examination of dead embryos in various stages of development, and the idea of the division of labour is one flowing from the premises of the Darwinian theory—the facts of variability and overcrowding. The second conception, that of "metabolism," touches immediately upon the processes of life, and demands special treatment in the present chapter which deals with biological Thought.

The conception of a continuous exchange or circulation of matter and of energy in every living organism, and the study of this elementary typical form of the living process in the morphological unit of all living organisms, in the cell, seems to have originated with
Theodor Schwann,[1] and is laid down in his 'Microscopical Researches,' published in 1839. On it is based 31. Schwann.
the whole simplification and unification of biological thought which distinguishes the second from the first half of our century. The study of the cell—its

[1] On the change which came over general physiology about 1840, and the part he himself played, Theodor Schwann has expressed himself in a letter addressed to Du Bois-Reymond, which is given in the notes to the latter's Éloge of Müller, reprinted in the second volume of his 'Reden,' pp. 143-334. It forms one of the most important historical documents. The Éloge itself should be read together with Claude Bernard's 'Rapport,' &c., mentioned above (p. 384 n.), which gives the history of the great change from a more exclusively French point of view. In the letter mentioned above, from which also the quotations given in the text are taken, Schwann claims that the first instance in which an "evidently vital phenomenon was submitted to mathematical, numerical" rule, was his measurement of the carrying power of a muscle in relation to its contraction in 1836. The purely physical view of vital phenomena exhibited in this example was not adopted by Müller, nor yet the quickly following general principle of the cellular theory. Schwann refers to the third section of his 'Microscopical Researches,' in which he discards "vitalism," but admits in man ("on account of his freedom") an immaterial principle, and claims that this assumption divides him distinctly from the materialists.

formation, growth, division, and maintenance of form amidst change of matter and alternation of function—constitutes the "prolegomena" of physiology, and a comparison of Prof. O. Hertwig's recent publication on the "cell" with the introduction to Johannes Müller's 'Physiology' marks well the change of ideas which half a century has produced. And we must so much the more admire the clear anticipation of Schwann, as he was not in possession of the full conception of energy in its two interchangeable forms of kinetic and potential energy, which was developed in the course of the two decades following his publication. Schwann not only conceived the cell to be the morphological unit of all living matter, but he also saw that "cell formation must be the general principle of organic development, and that there can be only one such principle." In the third section of his 'Microscopical Researches' he founds on this "his theory of organisms, and rejects distinctly therein all teleological explanations based upon a vital force acting according to final purposes." He thus showed "that the only essential property of all living matter—viz., growth—is not inaccessible to a physical explanation," and he did this at a time "when Liebig had not yet taught physiologists the chemical changes which take place in living tissues." These ideas were only partially adopted by Johannes Müller and other leading physiologists of the day. Schwann's view could only be consistently elaborated in proportion as
32. Circulation of matter and energy. to the older conception of a "Stoffwechsel" (a chemical process) there was added that of a "Kraft"- or

"Energie-wechsel"[1] (a physical process). Bio-chemistry had to be supplemented by bio-physics. With a clear anticipation of the correcter and fuller view, Schwann introduced the Greek term "metabolê." It is the merit of Prof. Michael Foster to have domiciled this useful and all-comprising technical term in English physiological

[1] Du Bois-Reymond ('Reden,' vol. ii. p. 220) mentions this as the third important gain which physiological science had to register since the appearance of Müller's book; the first and second being the cellular theory and the mechanico-physical method, both largely owing to Schwann. This was written just before the great influence of Darwin began to be felt. In the ideas introduced by Helmholtz, which clarified the conception of force, he sees the "key which opens a comprehension of the 'Stoffwechsel' in plants and animals." The term "Stoffwechsel," also "Stoffumsatz," or simply "Umsatz," has been quite familiar in German physiological literature during the whole of the century. I cannot find any generally accepted term in English literature before the introduction of Schwann's term "metabolic phenomena," which, I believe, was first adopted by Sir M. Foster, and is now quite domiciled in English text-books and translations. The passage in Schwann's 'Microscopical Researches' is as follows ('Sydenham Society's Translation,' p. 193): "The phenomena attending the formation of cells may be arranged in two natural groups: first, those which relate to the combination of the molecules to form a cell, and which may be denominated the *plastic* phenomena of the cell; secondly, those which result from chemical changes, either in the component particles of the cell itself or in the surrounding cytoblastema, and which may be called *metabolic* phenomena (τὸ μεταβολικόν, implying that which is liable to occasion or to suffer change)." It will be seen later on that the term metabolism is a peculiarly happy one, as it lends itself by a slight change in the prefix to denote the twofold process of building up and of running or falling down, which constitutes the changes peculiar to protoplasm as the constituent element of all organised substance. It is, accordingly, somewhat surprising that the term has found so little favour abroad. In France, where this twofold movement has long ago been recognised as one of the characters of the living process, the terms "composition et décomposition" (de Blainville), "organisation et désorganisation" (Claude Bernard), "assimilation et désassimilation," have been variously adopted (see Claude Bernard, 'Phénomènes de le vie,' vol. i. p. 36, &c.) M. Yves Delage ('L'Hérédité,' p. 53) says: "Les Anglais ont substitué à ces expressions si significatives: *nutrition*, *assimilation*, *désassimilation*, une terminologie qui a dû leur paraître bien belle, car ils l'ont tous adoptée avec un empressement remarquable; c'est celle de métabolisme," &c.

literature, to have placed it at the entrance of his text-book of physiology, and thus to have given the student a somewhat more detailed description of the elementary functions of living matter than was afforded by the older term "vortex," employed by Cuvier.

33. "Metabolism." These merits of Schwann, which attach more to the conception of "metabolism" than to that of the cell, are not reduced by our having to state that the latter conception has been entirely changed since his time. The cell of to-day is not the cell as Schwann conceived it. Of the pretty clearly defined structure or organisation of that biologist, with its wall (membrane), its kernel (nucleus), and its fluid contents (cell sap), nothing has remained but the cell contents, termed protoplasm by von Mohl; and the living process can no longer be considered as the function of a well-defined organ or machine. It is rather the fundamental property of an almost homogeneous substance, the mass of protoplasm, in which the kernel is the only recognisable differentiated portion. The immediate effect of this destructive analysis of the early conception of the cell was to destroy the idea that the living processes carried on in any special cell or organ are a result of its organisation, as the function of an apparatus is dependent upon the arrangement and combination of its parts. It has promoted the view that—for our understanding at least—the first thing to learn is the nature of the processes themselves. We have to look upon the visible structure of special cells and organs merely as "mechanical contrivances, serving only to modify in special ways the results of the exercise of these fundamental activities,

and in no sense determining their initial development."[1]

It seems, then, that we can date back to Schwann's 'Researches' the origin of two distinct courses of Thought which in the second half of our century obtain in biological science. The first we may call the morphological or structural school of biology. It is based on the theory of the cell or some modified conception, and attempts to explain the fundamental processes which go on in living organisms from the structure of the elementary parts. As the most minute particles of

34. Structural analysis of morphological elements.

[1] See Sir Michael Foster's excellent article on "General Physiology" in the 19th vol. of the 'Ency. Brit.,' 9th ed., p. 12. In this connection a passage from an early review of Huxley's, "On the Cell Theory," has been frequently quoted, according to which cells may be "no more the producers of the vital phenomena than the shells scattered in orderly lines along the sea beach are the instruments by which the gravitative force of the moon acts upon the ocean. Like these the cells mark only where the vital tides have been and how they have acted" (1853, in the 'Brit. and For. Med. Chirurg. Review,' reprinted in the first volume of 'Scientific Memoirs,' p. 277). According to this view, which has been further developed in more recent times, the cells would be "indications," not instruments, of the vital phenomena, which "are not necessarily preceded by organisation, nor are in any way the result or effect of formed parts, the faculty of manifesting them residing in the matter of which living bodies are composed, as such—or, to use the language of the day, the 'vital forces' are molecular forces." It is interesting to quote together with this passage from Huxley, what was said forty years later by an eminent living physiologist, Prof. Max Verworn of Jena: "The fact has been established that a fundamental contrast between living organisms and inorganic bodies does not exist. In contradistinction to all inorganic nature, however, organisms are characterised solely by the possession of certain highly complex chemical compounds, especially proteids" ('General Physiology,' transl. by F. S. Lee, 1899, p. 126). "We can summarise our considerations and give simple expression to the problem of all physiology. *The life-process consists in the metabolism of proteids.* If this be true, all physiological research is an experiment in this field: it consists in following the metabolism of proteids into its details, and recognising the various vital phenomena as an expression of this metabolism which must result from it with the same inevitable necessity as the phenomena of inorganic nature result from the chemical and physical changes of inorganic bodies" (ibid., p. 136).

living matter, revealed by the most powerful microscope aided by all the elaborate processes of staining, still appear to be endowed with the fundamental properties of life, such as irritability, contractility, and metabolism, *i.e.*, change in form and chemical constitution, the object of this line of research, viz., the investigation of the initial structure of the elements of living matter, can only be reached by indirect means or by conjecture. Structural chemistry and stereo-chemistry proceed by similar methods of investigation, and have succeeded by means of the atomic, molecular, and kinetic theories of matter in bringing order and unity into a very large portion of our knowledge of chemical combinations. The morphological or structural biologist pictures to himself very much more complicated arrangements of molecules than the carbon tetrahedron of van 't Hoff or the benzine ring of Kekulé, yet formed on similar principles; and by continuing in his mind these combinations which, as they become more complex, also become more unstable, he arrives ultimately at a very complex and continually changing chemical structure, which he imagines might be the beginning of the living process, the element of organisation. This conception, so far as I can find, was first introduced into biological literature by Herbert Spencer. He has termed this element of living matter "the physiological unit." The conception has been varied in endless ways by many subsequent biologists, all of whom have invented special names for these elementary units of life out of which they hope to put together the many observable protoplasmic and cellular tissues of the plant and animal

organism as Haüy attempted to build up crystals out of his "molécules intégrantes." The most elaborate analysis of this conception is put forward in the 'Micellar Theory' of the celebrated botanist Nägeli, which in Germany has found favour with many eminent biologists as a provisional programme of the various problems involved. It is clear that the conception of the physiological unit opens out two distinct lines of research. We can approach it on the one side by artificially producing in the chemical laboratory more and more of those chemically stable compounds which we find in the living organism. After Wöhler had produced urea artificially in 1828, the number of these artificial syntheses greatly increased, and we are specially indebted to M. Berthelot for having shown how all the simpler chemical compounds contained in the organism can be put together by inorganic processes. Some of the more complex substances have likewise subsequently yielded to this synthetic method. "It is possible," we are told, "that after a time our knowledge of chemistry may have advanced sufficiently to enable us to produce albuminous bodies artificially by synthesis."[1] "We are already able artificially to build up, atom for atom, out of their elements a series of organic compounds, some of a very complicated character. We no longer doubt that all the rest, even the most complex, will be thus produced; it is only a question of time."[2] But the ways in which the

35. Synthesis of organic substances.

[1] See O. Hertwig, 'The Cell,' p. 16.

[2] See G. Bunge, 'Physiological Chemistry,' transl. by Wooldridge, p. 313.

chemist puts together these substances in the laboratory are rarely the methods adopted by nature in the living organism, and in many cases the product itself, though apparently the same, is yet essentially different.[1]

[1] This touches on a very important point, which is much emphasised in all the best modern treatises on the subject. Claude Bernard in all his writings insisted on the fundamental difference between the processes going on in the organism and those that go on in the laboratory of the organic chemist, though the two produce frequently the same apparent result. "Si les forces que l'être vivant met en jeu dans ses manifestations vitales ne lui appartiennent pas et rentrent toutes dans les lois de la physico-chimie générale, les instruments et les procédés à l'aide desquels il les fait apparaître lui sont certainement spéciaux. En effet, l'organisme manifeste ses phénomènes physico-chimiques ou mécaniques à l'aide des éléments histologiques cellulaires, épithéliaux, musculaires, nerveux, &c. Il emploie donc de procédés, c'est-à-dire des outils organiques qui n'appartiennent qu'à lui. C'est pourquoi le chimiste, qui peut refaire, dans son laboratoire, les produits de la nature vivante, ne saurait jamais imiter ses procédés, parce qui il ne peut pas créer les instruments organiques élémentaires qui les exécutent. Cela revient à dire que tous les appareils des êtres organisés ont une morphologie qui leur est propre" ('Rapport,' &c., 1867, p. 135). Quite recently Bunge (*loc. cit.*, p. 313) has said, "All our artificial syntheses can only be achieved by the application of forces and agents which can never play a part in vital processes, such as extreme pressure, high temperature, concentrated mineral acids, free chlorine—factors which are immediately fatal to the living cell. . . . It follows that the animal body has command of ways and means of a totally different character, by which the same object is gained." A very interesting speculation, referring specially to this point, was put forward by the eminent physiologist, Prof. E. Pflüger of Bonn, in the year 1875. It is fully discussed in Verworn's frequently quoted work on General Physiology (pp. 304, 311, 482). The theory is based upon the remarkable part which the compound radicle cyanogen seems to play in the organism. Pflüger starts from the fundamental characteristics of the substance called proteid, with which life is inseparably connected. Proteid is known to exist in a stable form in food-stuffs, for instance in egg albumen. But this is not the same as the proteid contained in living matter. In the latter it is not stable, but is being continually decomposed. The decomposition was found to be due to the oxygen that occurs in the living proteid molecule. This oxygen, which is intramolecular, being continually received from outside by respiration, transforms the more stable molecule into an unstable labile molecule. In further following the clue afforded by this property, and comparing the decomposition products of living proteid with those obtained by artificial oxidation of dead proteid, Pflüger is led to the conclusion that the presence of the radicle cyanogen in the living proteid will explain the difference. "In the formation of cell-substance—*i.e.*, of living proteid—out of food proteid, a change of the latter takes place, the atoms of

Another way of inquiry is to analyse the existing organic tissues still further by microscopic and chemical methods, in order to find out how they are built up. As the result of such inquiries we have a framework theory of protoplasm, a foam theory, a filament theory, a granular theory; and the attempt has been made to define living protoplasm as a colony of still smaller structural units termed "bioblasts." By this twofold method—by synthesis and by analysis—the biologist may attempt to approach the physiological unit, the seat and stronghold of the vital process.[1]

nitrogen entering into a cyanogen-like relation with the atoms of carbon, probably with the absorption of considerable heat." Cyanogen being a radicle possessing a great quantity of internal energy, the addition of it to the living molecule "introduces into the living matter energetic internal motion." The interest which attaches to the theory lies in this, that it allows us to form some conception how living matter originated. This problem is indentified with the problem, How does cyanogen arise? This we know is formed at an incandescent heat. "Accordingly, nothing is clearer than the possibility of the formation of cyanogen compounds when the earth was wholly or partially in a fiery or heated state. . . . If, now, we consider the immeasurably long time during which the cooling of the earth's surface dragged slowly along, cyanogen, and the compounds that contain cyanogen and hydrocarbon substances, had time and opportunity to indulge extensively their great tendency towards transformation, . . . and to pass over, with the aid of oxygen, and later of water and salts, into that self-destructive proteid, living matter. . . . The first proteid to arise was living matter, endowed in all its radicles with the property of vigorously attracting similar constituents, adding them chemically to its molecule, and thus growing *ad infinitum*." This theory is interesting, as it is, so far as I know, the only attempt to reconcile the existence of living matter with the fact of the high temperature which once existed on the earth.

[1] A description of these several theories on the structure of protoplasm, among which the micellar theory of Nägeli, the foam theory of Bütschli, and the "bioblasts" of Altmann, have been elaborately developed, will be found in Prof. O. Hertwig's work on 'The Cell' (Engl. transl., p. 19, &c.), as also in M. Yves Delage's great work, 'L'Hérédité' (pp. 299-310). Verworn (*loc. cit.*, p. 87) draws special attention to the "alveolar" or "foam" theory, which, built upon investigations of Prof. Quincke, has "completely clarified our ideas upon the real nature of the protoplasmic structures so much observed. . . . As a result of these recent investigations the following picture can be formed of the finer morphological structure of proto-

36. The "physical" method.

There is, however, a second way open to the student of the phenomena of life, and this may be termed the "physical method," as opposed to the "structural." Thus chemists and physicists first establish the general laws of motion and change in dynamics and energetics, and subsequently apply them to special problems, such as those of physical astronomy or the chemistry of electrolysis and solution. Similarly the physiologist may study the processes common to all living matter, and look upon the action of a definite cell, tissue, or organ merely as an application of these general processes. From this point of view structural biology, like structural chemistry, only furnishes illustrations, not an explanation, of the vital processes: the special structure or organ is a result of the process or function—not its cause. As Prof. Michael Foster says, "We may throw overboard altogether all conceptions of life as the outcome of organisation, as the mechanical result of structural conditions, and attempt to put physiology on the same footing as physics and chemistry, and regard all vital phenomena as the complex products of certain fundamental properties exhibited by matter, which, either from its intrinsic nature or from

plasm. Protoplasm consists of a ground mass in many cases completely homogeneous, in most cases very finely foam-like or honeycomb-like, in which lies embedded a greater or less quantity of very various solid elements or granules. In the foam-like protoplasm the granules always lie at the corners and angles where the foam-vacuoles come together, never in the liquid of the bubbles themselves." Some physiologists think that none of the descriptions of protoplasmic architecture help us much, and "hold to the fundamental principle that living matter acts by virtue of its structure, *provided* the term structure be used in a sense which carries it beyond the limits of anatomical investigation—*i.e.*, beyond the knowledge which can be attained either by the scalpel or the microscope" (Burdon Sanderson, 'Address,' Brit. Assoc., 1889, p. 607).

its existing in peculiar conditions, is known as living matter."[1]

Thus, instead of trying to penetrate to the physiological units and construct them through a process of imagination, this latter class of biological thinkers confine themselves to the task of describing in the simplest manner and as completely as possible the various properties of the living substance—*i.e.*, its functions.[2] And

37. Properties of the living substance.

[1] 'Ency. Brit.,' article "Physiology," vol. xix. p. 12. See also an address delivered by Prof. Burdon Sanderson at the meeting of the Brit. Assoc. at Newcastle in 1889 ('Report,' p. 604): "During the last ten or fifteen years histology has carried her methods of research to such a degree of perfection that further improvement scarcely seems possible. As compared with these subtle refinements, the 'minute anatomy' of thirty years ago seems coarse—the skill for which we once took credit seems but clumsiness. Notwithstanding, the problems of the future from their very nature lie as completely out of reach of the one as the other. It is by different methods of investigation that our better-equipped successors must gain insight of those vital processes of which even the ultimate results of microscopical analysis will ever be as they are now, only the outward and visible signs" (p. 608).

[2] As Prof. Burdon Sanderson puts it in his 'Address,' it is a reversion to a position which is not new in the history of physiology. "The departure from the traditions of our science which this change of direction seems to imply is indeed more apparent than real. In tracing the history of some of the greatest advances, we find that the recognition of function has preceded the knowledge of structure. Haller's discovery of irritability was known and bore fruit long before anything was known of the structure of muscle" (p. 607). ". . . In much more recent times the investigation of the function of gland-cells, which has been carried on with such remarkable results by Prof. Heidenhain in Germany, and with equal success by Mr Langley in this country, has led to the discovery of the structural changes which they undergo in passing from the state of repose to that of activity; nor could I mention a better example than that afforded by Dr Gaskell's recent and very important discovery of the anatomical difference between cerebro-spinal nerves of different functions" (ibid.) What has to a great extent worked this important change in the methods and reasoning in physiology is the recognition of "plurality of function with unity of structure," a principle urged strongly by the experimental school of medicine, with Claude Bernard as its greatest representative. Notably this was the effect of his "demonstration that the liver had other things to do in the animal economy besides secreting bile. This, at one blow, destroyed the then dominant conception that the animal body was to be regarded as a bundle of organs, each with its appropriate function—a conception which did much to narrow inquiry, since when a suitable function had once been assigned to an organ

38. Environment.

here we meet first of all with the great fact that a living thing cannot be conceived to exist alone; it is dependent upon its environment, and upon other living things of similar, never quite identical, and mostly very different nature. As a consequence of the conception which guided Lamarck in contemplating the living world—especially the crowd of living things which before him had remained unobserved — the influence of environment plays a greater and greater part in the study of every form of life. The further fundamental property of all living matter—that it absorbs through intussusception other matter which surrounds it, that it grows and multiplies by division, casting off some portions of its own substance as useful secretions or cumbrous and useless excretions—has the twofold result that every living thing modifies its own surroundings and that it creates a society of its like which, through an automatic process of crowding-out, exercises a kind of selection among its members, they being forced to accommodate themselves to circumstances and to each other.[1] The process suggested by Darwin as the rationale

there seemed no need for further investigation. Physiology, expounded as it often was at that time in the light of such a conception, was apt to leave in the mind of the hearer the view that what remained to be done consisted chiefly in determining the use of organs such as the spleen, to which as yet no definite function had been allotted. The discovery of the glycogenic function of the liver struck a heavy blow at the whole theory of functions." (Sir M. Foster in 'Claude Bernard,' p. 90.) On the necessary condition of the experimental as distinguished from the anatomical method, namely, that it deals with the organism whilst it is alive, see the concluding remarks in Sir M. Foster's article on "General Physiology" in the 'Ency. Brit.,' vol. xix.

[1] The relations of living things to each other and to their environment admit of being contemplated in two ways, which may be best distinguished by a reference to human society, exhibiting as it does the two phenomena of co-operation and of competition. The former is based upon harmony, the latter upon conflict. The former aspect is more particularly emphasised by the French school of Lamarck, de Blainville, and Claude Bernard; the latter more by the English school of Malthus and Darwin; each starting apparently without any reference to the other. Claude Bernard in particular says ('Phénomènes de la vie,' vol. i. p. 67): "Pour nous la vie résulte d'une conflit, d'une relation étroite et harmonique entre les conditions extérieures et la constitution pré-établie de l'organisme. Ce n'est point par une lutte contre les conditions cosmiques que l'organisme se développe et se maintient; c'est, tout au contraire, par une adaptation, un accord avec celles-ci. . . . L'être vivant ne constitue pas une exception à la grande harmonie naturelle qui fait que les choses s'adaptent les unes aux autres; il ne rompt aucun accord; il n'est en contradiction ni en lutte avec les forces cosmiques générales; bien loin de là, il fait partie du concert universel des choses, et la vie de l'animal, par exemple, n'est qu'un fragment de la vie totale de l'univers."

of variation and development is more and more coming to be recognised as an inevitable property of all growing and multiplying living things. So far as the influence on the environment, the medium in which it lives, is concerned, we owe to the great French biologist, Claude Bernard, the helpful conception of the inner medium,[1] as

[1] Although the biology of Claude Bernard does not contain the principle of descent and evolution which so powerfully influenced the contemporary writings of English and German naturalists, one is nevertheless reminded of the ideas of Lamarck in reading the second of his lectures on the 'Phenomena of Life' (vol. i. pp. 65-124). Lamarck had expressed the idea that in the graduated scale of living things we recognise an increasing independence with regard to the external environment. (See *supra*, chap. vii. p. 315.) Claude Bernard says (p. 67): "Le mode des relations entre l'être vivant et les conditions cosmiques ambiantes nous permet de considérer trois formes de la vie, suivant qu'elle est dans une dépendance tout à fait étroite des conditions extérieures, dans ude dépendance moindre, ou dans une indépendance relative. Ces trois formes de la vie sont: 1°, La *vie latente;* vie non manifestée. 2°, La *vie oscillante;* vie à manifestations variables et dépendantes du milieu extérieur. 3°, La *vie constante;* vie à manifestations libres et indépendantes du milieu extérieur." Examples of the "vie latente" are to be found in the vegetable and animal creation alike. Grains of seed, desiccated animals, germs, eggs, ferments, yeast, &c., are examples. All vegetables belong to the class of the *vie oscillante,* also among animals all invertebrates, and among the vertebrates those with cold blood. These depend on cosmic conditions, the cold of winter, and the reviving heat of summer, &c. The higher animals with warm blood whose temperature is constant are not in the same way subject to the influence of the external medium. They

it were the inner atmosphere which any large assembly of living units must necessarily create for itself. All larger organisms are complex societies of living units which depend not only on the external but also on the internal medium which bathes them. It was one of Claude Bernard's happiest generalisations to look upon the blood, not as a living tissue but as a means of communication of the living tissues of the organism, as an internal medium which bears the same relation to the constituent tissues that the external medium, the atmosphere, does to the whole body.

39. The "internal medium."

One of the principal functions of this artificial medium or atmosphere which the larger organisms possess, create and maintain for themselves during their life, is to enable a particular elementary substance to get access to every living cell or tissue of the organism. This substance is oxygen, without which the continuance of life in the higher organisms seems impossible. That life is a process of combustion is accordingly a

possess "un *milieu intérieur* perfectionné" (p. 104). But they begin their existence as beings with an oscillating life when they are in the form of eggs. Of the *vie constante ou libre* Claude Bernard says: "Je crois avoir le premier insisté sur cette idée qu'il y a pour l'animal réellement deux milieux; un *milieu extérieur* dans lequel est placé l'organisme, et un *milieu intérieur* dans lequel vivent les éléments des tissus. L'existence de l'être se passe, non pas dans le milieu extérieur, air atmosphérique pour l'étre aérien, eau douce ou salée pour les animaux aquatiques, mais dans le *milieu liquide intérieur* formé par le liquide organique circulant qui entoure et baigne tous les éléments anatomiques des tissus; c'est la lymphe ou le plasma, la partie liquide du sang qui chez les animaux supérieurs, pénètre les tissus et constitue l'ensemble de tous les liquides interstitials, expression de toutes les nutritions locales, source et confluent de tous les échanges élémentaires. Un organisme complexe doit être considéré comme une réunion *d'êtres simples* qui sont les éléments anatomiques et qui vivent dans le milieu liquide intérieur. *La fixité du milieu intérieur est la condition de la vie libre indépendante*" (p. 113).

definition which has been put forth in various ways ever since Lavoisier's time, when he and Laplace tried to explain the existence of animal heat in this manner. The progress of science in the course of the century which followed Lavoisier has more and more confirmed the importance of the *rôle* which oxygen plays, but has also shown how very complex are the products of oxygenation in the living organism,—how the living processes are indeed chemical processes, but are quite different from those of the chemical laboratory. As Claude Bernard says, "The chemistry of the laboratory is carried on by means of reagents and apparatus which the chemist has prepared, and the chemistry of the living being is carried on by means of reagents and apparatus which the organism has prepared."[1] One of the great performances of living matter is the production, another is the storing up and distribution, of oxygen. But though we know that the chlorophyll-containing cells of green plants, under the influence of sunlight, are able to decompose that very inert body, carbonic acid, breathed out by both animals and plants, into free oxygen and carbon, allowing the carbon to be retained or utilised in the form of more or less complex carbohydrates, and though

[1] See especially the extensive explanations in the 'Rapport sur les progrès de la Physiol. gén.' (1867, p. 133 *sqq.*): "Les phénomènes physico-chimiques qui se passent dans les corps vivants sont exactement les mêmes, quant à leur nature, quant aux lois qui les régissent et quant à leurs produits, que ceux qui se passent dans les corps bruts; ce qui diffère, ce sont seulement les procédés et les appareils à l'aide desquels ils sont manifestés. . . . Il est déjà prouvé qu'un grand nombre de phénomènes qui s'accomplissent dans les corps vivants peuvent être reproduits artificiellement, en dehors de l'organisme, dans le monde minéral. Mais ce que l'on ne peut pas reproduire, ce sont les procédés et les outils spéciaux de l'organisme vivant" (p. 222).

we also know that the red blood corpuscles in vertebrate animals convey oxygen in a concentrated form[1] through all the organs, giving it up wherever it may be wanted, the real chemical process concerned in the action of chlorophyll is not cleared up;[2] and "no one has been able hitherto to explain, by a reference to physical laws, the active functions of the heart and muscular wall," by which the circulation of the blood is effected.[3]

In the explanation of many physiological phenomena no idea has proved more fruitful than the conception of natural selection, introduced by Darwin to explain the growing diversity and the purposefulness of organisms. Coupled with the cellular theory, which looks upon every living organism as a society of self-accommodating individual units or cells, forced by circumstances into differentiation of form and into divided labour or function, it relieved biologists of that spectre of vitalism which still survived after Lotze and Du Bois-Reymond had placed the creative and formative influence outside of the mechanism—as the watchmaker lives outside of the watch, which exhibits only mechanical contrivances. That which puzzles the spectator of the watch, as it does the spectator of every

[1] See Bunge, 'Physiological Chemistry,' p. 275.

[2] "Iron plays an important part in vegetable life: we know that chlorophyll granules cannot be formed without it. If plants are allowed to grow in nutritive solutions free from iron, the leaves are colourless, but become green as soon as an iron salt is added to the fluid in which the roots are immersed. It is even sufficient merely to brush the surface of the colourless leaf with a solution of an iron salt to cause the appearance of the green colour in the part thus painted. Chlorophyll itself contains no iron, and we do not know in what way the iron is concerned in its production" (Bunge, *loc. cit.*, p. 25). See also Hertwig, 'The Cell,' p. 153.

[3] Bunge, p. 7; cf. also p. 275.

organism, is the apparent design and purpose, without which neither could be conceived to have been formed.[1] Here, then, the idea that it was a process of natural choice, of automatic adjustment, which produced the apparent end and purpose at the moment when the structure itself was produced, came as a great relief.[2] It explained how it comes about that nature, even with unloaded dice, so often—yet not always—throws doublets. It permitted naturalists and physiologists to use purpose and final cause, not as an explanation, but as an indication where to look for causal—that is, for mechanical—connections. Accordingly the first systematic attempt to use natural selection in the explanation of the adjustment of the internal parts of an organism, which is due to Prof. Wilhelm

40. Natural selection within the organism.

[1] "The main problem which the organic world offers for our solution is the purposefulness seen in organisms. That species are from time to time transformed into new ones might perhaps be understood by means of an internal transforming force, but that they are so changed as to become better adapted to the new conditions under which they have to live is left entirely unintelligible" (Weismann on Nägeli's "Mechanisch-Physiologische Theorie der Abstammungslehre" in 'Essays upon Heredity,' Engl. transl., p. 257).

[2] See Du Bois-Reymond's Address, "Darwin *versus* Galiani" ('Reden,' vol. i. p. 211, &c.): "Here is the knot, here the great difficulty that tortures the intellect which would understand the world. Whoever does not place all activity wholesale under the sway of Epicurean chance, whoever gives only his little finger to teleology, will inevitably arrive at Paley's discarded 'Natural Theology,' and so much the more necessarily, the more clearly he thinks and the more independent his judgment. . . . The physiologist may define his science as the doctrine of the changes which take place in organisms from internal causes. . . . No sooner has he, so to speak, turned his back on himself than he discovers himself talking again of functions, performances, actions, and purposes of the organs. The possibility, ever so distant, of banishing from nature its seeming purpose, and putting a blind necessity everywhere in the place of final causes, appears therefore as one of the greatest advances in the world of thought, from which a new era will be dated in the treatment of these problems. To have somewhat eased the torture of the intellect which ponders over the world-problem will, as long as philosophical naturalists exist, be Charles Darwin's greatest title to glory" (p. 216).

Roux in his work on the 'Struggle of the Parts in the Organism,' was hailed by Darwin as "the most important book on development that has appeared for some time." [1] In modern books on physiology the process of selection is a familiar conception; but if in natural history, in the life of plants and insects, there still remain many extraordinary instances of selection

[1] The work appeared in 1880, and is referred to by Darwin in a letter to Romanes ('Life and Letters,' vol. iii. p. 244; 16th April 1881), where he suggests also a similar consideration of plant life and structure. It has been republished in Roux's 'Gesammelte Abhandlungen zur Entwickelungsmechanik der Organismen' (Leipzig, 1895, 2 vols.), with an interesting preface (vol. i. p. 139, &c.), and many historical and critical digressions. It originally emanated from the earliest school of Darwinism in Germany, represented by Haeckel, Gegenbaur, and Preyer, at Jena. It has been found very suggestive, and has been the beginning of a very large controversial literature in Germany, in which the fundamental problems of biology have been discussed, and have received new formulations. The idea of the struggle of individuals for survival, suggested by Darwin, is applied by Roux to the different parts and organs within the developing organism. Du Bois-Reymond almost contemporaneously published his brilliant and celebrated address on "Exercise" ("Ueber die Uebung," 'Reden,' vol. ii. p. 404). In England Roux's suggestive treatise does not seem to have been much noticed, and Prof. Roux himself attributes this to the inadequate notice of the book by Romanes in 'Nature' (vol. xxiv. p. 505), in which his doctrine was erroneously compared with Spencer's ideas of "direct equilibration." Prof. J. A. Thomson, in 'The Science of Life,' refers to the importance of Roux's work (pp. 138, 229), and of his 'Archiv für Entwickelungsmechanik.' Roux has been classed by some of his critics among the "organicists," a school represented in France chiefly by Claude Bernard. The main thesis of this view seems to be that the phenomena of life consist in the play of two factors—the organisation and the environment of the living thing. Roux applies the process of natural selection and consequent adaptation, which Darwin sees at work in a crowd of living things, to the organisation of the individuals themselves, each of which is a microcosm, a society of autonomous units, say of cells. He has accordingly gone a step farther back than the older "organicists," studying the development, the genesis of the organism on Darwinian lines. M. Delage accordingly dates from him a new school of "organicism." "L'organicisme commence, à mon sens, avec Descartes (1642), se continue avec Bichat, Claude Bernard, et arrive avec Roux (1881) à une théorie si profondément modifiée, bien qu'elle dérive du même principe, qu'elle peut être considérée comme toute moderne" ('L'Hérédité,' p. 408).

for which no teleological mechanism has been invented, still more are we baffled by the apparent "autonomy of the living cell," in consequence of which it is, *e.g.*, "able to select its food, retaining what is useful and rejecting what is harmful." [1] And what shall we say of the so-called "wandering cells, which are actually sent out by the organism in order to absorb in the alimentary canal food-stuffs, notably fat, returning with it into the blood, or to receive into themselves malignant bacteria, making them innocuous by a process of digestion?" [2] No mechanical physico-chemical explanation of this process is imaginable, and the word "selection," with which Darwin charmed away so many mysteries, has revealed new ones in their place.[3]

[1] See the very interesting and frequently quoted address by Prof. G. E. Rindfleisch (Würzburg, 1888), entitled 'Ärztliche Philosophie,' p. 13.

[2] Rindfleisch, *loc. cit.*, p. 15.

[3] In this connection it is interesting to refer to a discussion which was raised by the suggestive address of Prof. F. R. Japp, entitled, "Stereochemistry and Vitalism" ('Brit. Assoc. Report,' 1898, p. 813). It refers to the discovery by Pasteur of "chirality" in solutions of certain crystallised organic salts, on which I reported in vol. i. p. 450. "Pasteur regarded the formation of asymmetric organic compounds as the special prerogative of the living organism. Most of the substances of which the animal and vegetable tissues are built up—the proteids, cellulose—are asymmetric organic compounds." Now, in his experiments on fermentation Pasteur found that "the asymmetric living organism selected for its nutriment that particular asymmetric form" out of a mixture of two enantiomorphous compounds held in solution—"which suited its needs—and left the opposite form either wholly or, for the most part, untouched" (p. 817). Prof. Japp proceeds to consider the opinion then formed by Pasteur, "that compounds exhibiting optical activity have never been obtained without the intervention of life" (p. 818). This view, to which Pasteur adhered, and which he defended against eminent opponents, has been frequently challenged, and seemed definitely set aside by the explanation of Prof. Emil Fischer of Berlin, and by Jungfleisch's synthesis of racemic acid and its resolution into dextro- and lævo-tartaric acids. . . . "Consequently, the overwhelming marjority of chemists hold that the foregoing synthesis and separation of optically active compounds have been effected without the intervention of life, either directly

41. Mobility of living matter.

Another property of all living matter which has been seized upon to furnish a definition of life is its extreme mobility. It has been stated that the great difference between living and non-living matter is this—that the former is in a state of movable or dynamical equilibrium, whereas the latter tends always to a condition of rest or of statical equilibrium. This was especially urged by the late celebrated Du Bois-Reymond of Berlin, to whom we owe the greater part of our knowledge of the physical and chemical changes exhibited in the active nervous system. In comparison with this property of a dynamical equilibrium, explained by the analogy of a fountain of water or a vortex which change their substance whilst maintaining their form, other older distinctions which had been drawn between organised and unorganised bodies sank into insignificance.[1]

or indirectly" (p. 824). Prof. Japp and Prof. Crum Brown of Edinburgh are of the opposite opinion, inasmuch as in the view of the former "the action of life, which has been excluded during the previous stages of the process, is introduced the moment the operator begins to pick out the two enantiomorphs," as was done by Jungfleisch.

[1] Among the older discussions of the best way of defining life which belong to the second third of the century, we have in Germany the various writings of Du Bois-Reymond ('Reden,' notably vol. ii. p. 25); in France those of Claude Bernard ('Phénomènes de la vie,' notably vol. i. p. 21, &c.); in England the 'Biology' of Mr Herbert Spencer. The two last-named authors examine with some care the definitions of earlier writers. All three should be read and re-read by any one who desires to arrive at a clear understanding of the subject. Du Bois-Reymond's definition shows the preponderating influence of the ideas which governed the Berlin school of physiology, and which centred in Helmholtz's tract on the Conservation of Energy. Claude Bernard defines life by the words "La vie, c'est la création." Organisation and disorganisation are the two sides of this process, organisation and environment the two factors. The doctrine of evolution goes a step farther back, and attempts to analyse "organisation." The process of creation is to Mr Herbert Spencer a process of development. The word creation in the older sense ceases to have a meaning. Of more recent date are the discussions of the subject in the very interesting work of Carl Hauptmann, 'Die Metaphysik in der modernen Physiologie' (Jena, 1894, neue Aufl.), especially the last chapter. Still more recent is the very careful analysis contained in the new edition of Mr Spencer's 'Biology,' notably vol. i. p. 111 *sqq.* The final conclusion arrived at by these two latest philosophers has much in common. Both strive after a dynamic conception of life; both confess that such is at present unattainable—a desideratum, not an achievement. Hauptmann says (p. 386): "The most primitive life, from which alone the living world on this earth can have sprung, can only be assumed to be a species the members of which varied in manifold ways and propagated themselves. Here we have to do already with an eminently complex interaction of elementary processes. . . . We still absolutely lack every conception of such a dynamical system. . . . Likewise the origin of the simplest living substance is mechanically quite unknown and uncomprehended. . . . The individual forms of life stand in the midst of a yet unintelligible higher order of the material world." Similarly Mr Spencer (*loc. cit.*, p. 120): "We are obliged to confess that life in its essence cannot be conceived in physico-chemical terms. The required principle of activity, which we found cannot be represented as an independent vital principle, we now find cannot be represented as a principle inherent in living matter. If, by assuming its inherence, we think the facts are accounted for, we do but cheat ourselves with pseudo-ideas. . . . It needs but to observe how even simple forms of existence are in their ultimate nature incomprehensible, to see that this most complex form of existence is in a sense doubly incomprehensible. . . . While the phenomena (of life) are accessible to thought, the implied noumenon is inaccessible, ... only the manifestations come within the range of our intelligence, while that which is manifested lies beyond it" (p. 122). There seems ample evidence that under different forms of words Claude Bernard and Du Bois-Reymond, in his later writings, arrived at similar conclusions. See 'La Science Expérimentale,' p. 210, and "Die sieben Welträthsel" ('Reden,' vol. i. p. 381). "The mystery is the more profound the more it is brought into contrast with the exact knowledge we possess of surrounding conditions" (Prof. Burdon Sanderson, 'Brit. Assoc. Report,' 1889, p. 614).

It is true that not all parts of a higher organism are subject to this continued change, but those that are not —such as the skeleton of an animal or the trunk of a tree—are automatically deposited by the living organism for the purpose of external or internal support, protection, or communication. They are the permanent mechanism by which the economy and administration of the society of living units or cells are kept up. These it has been possible, in many instances, to analyse into stable chemical compounds, which have been reproduced in

the chemical laboratory by processes which were like or unlike those going on in the organism itself. But such stable compounds are not the bearers, they are merely the collateral products, the accompaniments, of the living process. The artificial production of organic compounds, beginning with Wöhler's production of urea, and ending with the production of albumen, do not approach the problem of the production of living matter. Could the chemist produce protoplasm, it would not be living; or were he fortunate enough to hit upon one of its many metamorphoses, it would die the next moment, not having the inner structure or the external and internal environment necessary for its self-conservation and activity. Nor do we seem to get any nearer the real secret by analysing more closely the chemical and physical changes, the metabolism, the rhythmical processes which constitute this activity. We call it nutrition or respiration, assimilation and disassimilation, oxidation and reduction—storing up and letting loose of energy. We picture to ourselves the building up of more and more complicated chemical molecules, containing thousands of atoms, in a temporary and easily disturbed equilibrium, and the subsequent breaking down again of these complex structures by gradual decomposition or by sudden explosions due to external stimuli, or by the still more mysterious directive action of conscious will: we liken them to the pulling of a trigger, or the gathering up and letting loose of a destructive avalanche by the motion of a flake of snow on the top of a peak. We see how this metabolism, this "Stoff- und Kraft-wechsel," goes on in the smallest amœba in rhythmical movements, and how, in higher

organisms, it is divided into many stages, allocated to special cells or to quite distinct classes of beings, some of which, like plants, take upon themselves the first important steps of the anabolism, so that others—the animals—may carry it a stage higher, preparing a discharge, or catabolism, which becomes more and more effective, till it reaches the unique nervous function which accompanies the highest phenomenon of animal activity—the mental process. Claude Bernard[1] has put into classical words the rationale of this process. "If, in the language of a mechanic, the vital phenomena —namely, the construction and destruction of organic substance—may be compared to the rise and fall of a weight, then we may say that the rise and fall are accomplished in all cells, both plant and animal, but with this difference, that the animal element finds its weight[2] already raised up to a certain level, and that hence it has to be raised less than it subsequently falls.

[1] 'Phénomènes de la vie,' &c., vol. ii. p. 513. It is one of Claude Bernard's greatest merits to have corrected the earlier formula in which the circulation of matter had been expressed. Dumas and Boussingault had said: "L'oxygène enlevé par les animaux est restitué par les végétaux. Les premiers consomment de l'oxygène; les seconds produisent de l'oxygène. Les premiers brûlent du carbone; les seconds produisent du carbone. Les premiers exhalent de l'acide carbonique; les seconds fixent de l'acide carbonique." On this passage Claude Bernard has the following comment: "Cette loi qui sous la forme précédente exprime avec vérité le mécanisme d'une des plus grandes harmonies de la nature est une loi cosmique et non une loi physiologique. Appliquée en physiologie, elle n'explique pas les phénomènes individuels: elle exprime comment l'ensemble des animaux et l'ensemble des plantes se comportent en définitive par rapport au milieu ambiant. La loi établit la balance entre la somme de tous les phénomènes de la vie animale et de la vie végétale: elle n'est point l'expression de ce qui se passe en particulier dans un animai ou une plante donnés" (p. 512). This false direction, which had been introduced into physiology a generation earlier, Claude Bernard corrected by the view that the circulation of matter takes place not only between the two kingdoms of nature but in every elementary organism.

[2] Or its potential.

The reverse occurs in the green plant cells. In a word, of the two movements, that of descent is preponderant in the animal, that of ascent in the vegetable." No one has done greater service to the fixing of our ideas on this subject than Dr Gaskell when he analysed the whole process, called "Metabolism" by Professor Michael Foster after Schwann, into the two complementary pro-

42. Anabolism and Catabolism.

cesses of Anabolism the upward, and Catabolism the downward, movement — the winding up and running down of the clock, the preparation and loading of the explosive and the discharge of the gun.[1]

[1] The introduction of these terms is, however, connected with a special view — differing somewhat from that suggested by the formula of Claude Bernard—which is now very generally adopted in text-books of physiology. Prof. Burdon Sanderson has given a lucid statement of this difference in his Address, entitled "Elementary Problems of Physiology," before the Brit. Assoc. in 1889 ('Report,' p. 613). He there says: "A characteristic of living process . . . is that it is a constantly recurring alternation of opposite and complementary states, that of activity or discharge, that of rest or restitution. Is it so or is it not? In the minds of most physiologists the distinction between the phenomena of discharge and the phenomena of restitution (*Erholung*) is fundamental, but beyond this unanimity ceases. Two distinguished men—Prof. Hering and Dr Gaskell—have taken, upon independent grounds, a different view to the one above suggested, according to which life consists not of alternations between rest and activity, charge and discharge, loading and exploding, but between two kinds of activity, two kinds of explosion, which differ only in the direction in which they act, in the circumstance that they are antagonistic to each other. Now, when we compare the two processes of rest . . . and discharge . . . with each other, they may further be distinguished in this respect, that whereas restitution is automatic, the other is occasional—*i.e.*, takes place only at the suggestion of external influences. . . . It is in accordance with the analogy between the alternation of waking and sleeping of the whole organism, and the corresponding alternation of restitution and discharge, of every kind of living substance, that physiologists by common consent use the word stimulus (*Reiz*), meaning thereby nothing more than that it is by external disturbing or interfering influence of some kind that energies stored in living material are discharged. Now, if I were to maintain that restitution is not autonomic, but determined, as waking is, by an external stimulus, that it differed from waking only in the direction in which the stimulus acts—*i.e.*, in the tendency towards construction on the one hand, towards destruction on the other—I should fairly and as clearly as possible express the doctrine which Dr Gaskell and Prof. Hering have embodied in words which have now become familiar to every student. The words in question—'anabolism,' which, being interpreted, means winding-up, and 'catabolism,' running down—are the creation of Dr Gaskell."

The modern theories of the cell, of metabolism, and selection, have also greatly influenced and modified our conceptions concerning the last and most important property of all living matter—viz., that it is self-reproductive. Older text-books on physiology treated of the great problem of generation—*i.e.*, the origin of a new individual 43. Reproduction.
—as a phenomenon of organised life which stood quite isolated; and although the sexual difference in plants and animals had early led to certain analogies, to similar terminology, and to vague inferences, the mysterious phenomena of generation, and especially of sexual generation, were not brought into line with the general properties of all living matter till about fifty years ago. Even Johannes Müller in his great text-book on Physiology, which takes a much wider view of the subject than any work before it, treats of the reproduction of tissues and of generation in quite separate, seemingly disconnected, parts of his work. Into this uncertainty only little light was thrown by the original propounders of the Cellular theory, who, misled by the supposed analogy of cells and crystals, imagined that cells originated out of the surrounding cell sap, as crystals solidify out of the solution or mother liquor. Correcter views were gradually elaborated by botanists. Mohl emphasised the important part which protoplasm plays in the formation of cells. Nägeli established the process of intussusception as against external accretion; anatomists like Max Schulze and Brücke joined hands,

and the year 1863 is usually given as that in which the protoplasmic theory was established. According to this view protoplasm is the element or unit of all living substance: it grows through assimilation (intussusception and excretion), and multiplies (*i.e.*, gives rise to other living units) by subsequent division. This process was found to be fundamental: it describes the growth of the simplest and the most complicated organisms as beginning alike with a unit cell, which may or may not grow by division; it is the formula of growth, restitution, and generation (whether sexual orasexual); and, what is equally important, it prevails also in pathological cases—*i.e.*, in the formation of diseased tissues. In fact, the great generalisation which followed Harvey's celebrated dictum, "omne vivum ex ovo," was put forth by the late Professor Rudolf Virchow, the eminent founder of cellular pathology, in his formula, "omnis cellula e cellula." The formula has in more recent times been further elaborated on the same lines of thought in proportion as the importance of the nucleus or cell kernel has been recognised, or as the granular structure of protoplasm has been maintained; leading to analogous formulæ, such as "omnis nucleus e nucleo," "omne granulum e granulo." These formulæ[1] are the

44. The protoplasmic theory.

[1] See Roux ('Gesammelte Abhandlungen,' vol. i. p. 393): "Uninterrupted durability is the indispensable condition of all that is organic, although this does not involve a distinction from inorganic processes. This fact is expressed by the fundamental theses: Omne vivum ex ovo (Harvey), Omnis cellula e cellula (Virchow), Omnis nucleus e nucleo (Flemming)." Hauptmann ('Die Metaphysik,' &c., p. 334) says: "Altmann formulates for himself in analogy with these biological principles the further principle, 'Omne granulum e granulo.'" On Altmann's theory of the "bioblasts" as elementary organisms, see Yves Delage, 'L'Hérédité,' p. 498, &c., Hertwig, 'The Cell,' p. 24.

expression of anatomical observations and theories representing an enormous amount of research, labour, and ingenuity, but they involve no new line of reasoning, and they belong, accordingly, more to the history of Science than to that of Thought.

The first to attempt a mechanical explanation of the process of cellular division was Mr Herbert Spencer,[1] who, in his 'Principles of Biology' (1863), pointed out that there exists a limit of growth through assimilation or intussusception, inasmuch as volume and mass increase at a greater rate than the surrounding surface through which communication with the environment is afforded. A resultant tension brings about an increase of surface through rupture, and restores the balance between the contained mass and the surface. In his analysis of this process of readjustment, Spencer has given mechanical

45. Spencer's law of limit of growth.

[1] The principle here referred to sometimes goes under the name of the Leuckart-Spencer principle, it having been suggested independently by Rudolf Leuckart, Herbert Spencer, and Alexander James. It requires, of course, a great many qualifications. See the 'Principles of Biology,' vol. i. part 2, chap. i. But "it follows from these considerations that the cell can never surpass a certain size; for if the disturbance of metabolism that arises because of the increasing disproportion between the more superficial and the deeper layers has reached a certain extent, the cell can no longer continue living in its existing form. Thus the remarkable fact is explained very simply, that no cells of constant form are known that are larger than a few millimetres in diameter, and thus we are made to understand why the development of large organisms is only possible by the arrangement of the living substance into an aggregate of small cells instead of into a single cell, for example, of the size of a man. . . . If, therefore, the living substance of such a cell is not to perish by growth, at some period in its growth a correction of this disproportion between mass and surface and of the disturbance of metabolism conditioned by it must come in: such a correction is realised in the reproduction of the cell by division. The reproduction of the cell by division is accordingly to be considered merely as a result of growth, and the morphologists for a long time have rightly termed reproduction a continuation of growth, 'a growth beyond the measure of the individual'" (Verworn, 'General Physiology,' Engl. transl., p. 530, &c.)

biologists a formula which, like his physiological units, has helped to give precision and direction to reasoning on these subjects. But as growth has a natural limit and leads to division, so reproduction through division appears to have a limit also. "Only the very lowest organisms, such as fission fungi, appear to be able to multiply indefinitely by repeated divisions: for the greater part of the animal and vegetable kingdoms the general law may be laid down that, after a period of increase of mass through cell division, a time arrives when two cells of different origin must fuse together, producing by their coalescence an elementary organism which affords the starting-point for a new series of multiplications by division."[1] Fertilisation is now known to be a cellular problem. As such it has been studied in favourable cases which permitted of direct observation, and what has been ascertained in those cases—exhibiting in general the same common features and phases of development—has by inference under the great generalisations of the cellular theory been extended to all living things in which sexual differentiation exists, be they animals or plants.[2] The male and the female

46. Fusion of two elements.

[1] Hertwig, 'The Cell,' p. 252. The process may be looked at as an instance of the cyclical order of change. "The multiplication of the elementary organism, and with it life itself, resolves itself into a cyclic process. . . . Such cycles are termed generation cycles. They occur in the whole organic kingdom in the most various forms." Similarly Sir M. Foster ('Text-book of Physiology,' 5th ed., p. 1555), as quoted, *supra*, p. 289. We may add that from a still broader standpoint, which we may call that of bionomics — in distinction from biology — the cycle never repeats itself, but, owing to overcrowding and selection, something different, more complex—*i.e.*, externally or internally better endowed—is produced. Philosophically we call this progress.

[2] There exists no more remarkable instance of the extension of natural knowledge by a process of very incomplete induction than the gradual firm establishment of the now universally adopted doctrine of fertilisation, no more brilliant refu-

elements concerned have both been recognised to be cells, both have been found to undergo, before what is termed the stage of maturity, similar preparatory changes. The changes represent, as it were, the last stages of their independent existence as living cells. After these changes have taken place they can only enter into a new cycle of existence, exhibiting new powers of growth and division by a process of fusion where each supplies what in the other is wanting to start on a new cycle of life—*i.e.*, of differentiation and development.

Thus the vague theories of former times, which reach far into the nineteenth century, the speculations of the Spermatists and the Ovists, have during the last thirty years, beginning with Pringsheim's observation in 1869 of the pairing of the swarm-spores of certain algæ,

tation of the purely enumerative, or all-case method. The number of instances in which the process of fertilisation, with its various preparatory stages and its consequences, can be actually observed is infinitesimally small compared to the number of different species and varieties in which it is endlessly repeated on lines which no biologist doubts to be essentially the same. M. Yves Delage says: "C'est une chose remarquable combien certains êtres, par des particularités en apparence sans intérêt ont facilité la solution de certains problèmes presque insolubles en dehors d'eux. *L'Ascaris megalocephala* [the round-worm of the horse, first observed by van Beneden in 1883], par le petit nombre de ses chromosomes, les Echinodermes [sea urchins, &c.] par la facilité avec laquelle ils acceptent la fécondation artificielle, ont fait faire, en dix ans, plus de progrès aux questions relatives à la fécondation que n'ont fait avant ou depuis tous les autres animaux réunis. Dans l'Ascaride, le testicule forme un long tube et les diverses phases de la spermatogénèse s'accomplissent dans les régions différentes de l'organe: il y a une zone à spermatogonies, une zone à spermatocytes en voie d'accroissement, une zone où se font les divisions réductrices et une enfin où les spermatides se transforment en spermatozoïdes" (L'Hérédité,' p. 133). See on the variety of objects which have lent themselves to the gradual unravelling of the processes of cell division, nuclear division, fusion of nuclei, cleavage and embryonic development, notably the volume of Prof. Val. Haecker, 'Praxis und Theorie der Zellen- und Befruchtungslehre' (Jena, 1899). A very lucid summary is contained in J. A. Thomson's 'The Science of Life' (1899).

and centring in van Beneden's discovery,[1] been replaced by definite conceptions capable of typical description. This typical process consists in the fusion of certain parts of the male and female cells,—the nuclei or kernels playing an important if not the essential part. Many biologists of the foremost rank, notably in Germany and France, have contributed to make clearer the various lines in this typical picture of the most mysterious process in the physical organism, whilst every new discovery has brought with it new and unanswered questions or given a novel aspect to older problems.

47. New problems. Of these problems, those of heredity and variation are at present by far the most important. Both the cellular theory of living matter and the theory of natural selection, including the principles of differentiation and of the division of physiological labour, converge upon these two great facts of modern biology. The theory of natural selection pre-

[1] See last note. "Since the researches of O. Hertwig and others in 1875, it had been clear that each parent contributes a single germ-cell to the formation of the offspring; but the masterly researches of E. van Beneden (1883) showed that every nucleus of the offspring may contain nuclear substance derived from each of the parents, a conclusion which is visibly demonstrable for a few of the first steps in cleavage. In fact, van Beneden to some extent *proved* what Huxley had foreseen when he said, in 1878, 'It is conceivable, and indeed probable, that every part of the adult contains molecules, derived both from the male and from the female parent; and that, regarded as a mass of molecules, the entire organism may be compared to a web, of which the warp is derived from the female, and the woof from the male'" (J. Arth. Thomson, 'The Science of Life,' p. 129). Another theoretical anticipation is, according to Haecker (*loc. cit.*, p. 133), the "Idioplasma" of Nägeli: "The heritable substance, organised, possessing a complex structure, transmitted from one generation to another," which was "about the same time identified by Strassburger, O. Hertwig, von Kölliker, and Weismann, with the chromatin substance of the nucleus."

supposes the fact of heredity—that is, the transmission of characters peculiar to the parents (be they acquired by them or not), and the fact of variation, but it does not explain them. It does not give any intelligible description of the means which nature uses to secure that continuity of change which is marked on the one side by a faithfulness to certain typical forms, and on the other by a gradual development. The cellular theory permits us to comprise, under the general categories of cell-growth, cell-division, and cell-fusion, the great facts of the history of all living matter, but it does not explain how that apparent sameness of structure which the ultimate morphological unit, the cell, presents to our view, develops into that variety of recurrent forms which make up the wealth and the order in the world of natural objects. The older naturalists were divided into two distinct schools: one believed in pre-formation with development—the older meaning of "evolution"; the other in after-formation, or "epigenesis." The former foundered on the difficulty of explaining or making plausible how all the germs of hundreds of succeeding generations could be contained in the first ancestor; the latter failed to explain how nature was able to build up by mechanical forces out of unorganised matter a structure resembling the parent structures. The suggestion of a "nisus formativus," which we owe to the celebrated Blumenbach, is only a definition of the difficulty, not an explanation.

The three distinct ideas represented by these historic terms occur again in modern biology, though altered to suit the vast extension of actual knowledge of facts, and

the three great generalisations mentioned above. Out of the three ideas of pre-formation, after-formation, and the directive principle, the three generalisations, namely, the cellular theory, natural selection, and metabolism, and the enormous number of facts collected by microscopists and naturalists of all kinds, many more or less ingenious theories of life have been put together. None of them has obtained, though some have had a very marked influence on biological science, and even on popular thought. Of these Prof. Weismann's theories of heredity are probably the best known. Without entering upon the enormous array of biological facts which have been marshalled by supporters and opponents alike, it will be of interest to point out the novel aspects and lines of reasoning which have come into prominence through the voluminous discussion belonging to this subject. They were prepared before the appearance of Weismann's writings by the changed and enlarged conceptions which the discoveries of the middle of the century introduced concerning the general phenomena of Life, Death, and Disease. Three distinct convictions regarding these three main aspects of the living portion of creation have been forced upon the scientific and popular mind. First, we have the modern doctrine of the ubiquity of organisms and germs, at least so far as our planet is concerned: beyond this sphere we can say that we know no more of the existence of living matter than past generations. Secondly, we have the generally recognised doctrine that spontaneous generation of living out of not-living matter is unknown and inconceivable under such conditions as

48. Weismann on heredity.

we can realise or imagine. And thirdly, hand in hand with the conviction of this unique but ubiquitous character of life, the impression of the mutual interdependence of living creatures has gained ground, and has especially influenced our ideas of the cause and treatment of disease.

49. Biogenesis.

In one of those luminous addresses in which he has rivalled the combination of literary with scientific clearness characteristic of the French genius, the late Prof. Huxley has written the history of Biogenesis [1] —*i.e.*, of the theories of the origin of life from the time of the Italian Redi down to Pasteur, showing how experiment and theory alternately supported and contradicted the doctrine that living matter could be formed out of not-living matter, till the great French biologist, by his refined experiments, entirely banished from the provinces of science and practice the once admitted fact that, after exclusion or destruction of all living germs, phenomena peculiar to life, such as fermentation and putrefaction, could be generated. Those great departments of medical practice, the antiseptic and aseptic treatment, with their enormous development of prophylactic and antitoxic methods, form the daily and ever-growing argument against abiogenesis

[1] In his presidential address to the British Association in 1870, reprinted in 'Critiques and Addresses,' p. 218 *sqq.* A very readable and much earlier deliverance on "The Diffusion of Life" is that by K. E. von Baer, before the Academy of St Petersburg in 1838, reprinted in the first volume of his 'Reden,' &c., p. 161 *sqq.* In the preface of 1864 to this reprint, the illustrious author tells us that between 1810, and 1830 there were probably few naturalists who "did not consider the generation without parents of inferior organisms as proved, or at least as highly probable," and he himself would not at that time (1838) "declare it to be non-existent" (p. 173). In 1864 he describes the theory as having almost vanished, leaving the problem of the first beginnings of life in the numberless varieties, even after Darwin's hypothesis, unsolved (p. 177).

—*i.e.*, the generation of living out of dead or not-living matter.

But in proportion as abiogenesis or spontaneous generation has disappeared from our scientific text-books, life being recognised as a phenomenon between which and dead matter there exists no intelligible and no practical transition except that of destruction,
50. The ubiquity of life. the ubiquity of life has forced itself more and more on our attention. Not long ago, as Huxley[1] tells us, the adherents of spontaneous generation urged as an argument on their side that if biogenesis be true, innumerable facts and experiments prove "that the air must be thick with germs; and they regarded this as the height of absurdity. But nature," as Huxley continues, "occasionally is exceedingly unreasonable, and Professor Tyndall has proved that ordinary air is no better than a sort of stirabout of excessively minute solid particles." It is now, after a generation has passed, hardly necessary to refer to any special experiments of Tyndall or of others, when the daily press brings us records of the number of billions of germs contained in a cubic inch of the atmosphere of large cities, precisely as it does of the mortality of their population. The cellular theory of disease has been succeeded and amplified by the bacillar theory, and no modern scientific fact has fastened on the popular mind with a stronger hold than the ubiquity of the micro-organisms, which, with beneficent or fatal results, assist everywhere—chiefly in the larger organisms—in the struggle for existence.

It is, moreover, only a logical inference that if living

[1] 'Critiques and Addresses,' p. 233.

matter is not being continually formed out of not-living matter, while it is an undeniable fact that living matter is continually and everywhere passing out of existence, the preservation of life is dependent upon an enormous self-overproduction which, combined with the process of natural selection, secures its permanence and the development of the highest forms of which it is capable. The continuity—*i.e.*, the interdependence—of 51. The continuity of living forms.
all living forms in time and space guarantees the non-extinction of this phenomenon, which, for all that we know, is of a unique character. The modern scientific and popular view of life is that it is a unique phenomenon, that it is a ubiquitous phenomenon, at least within the area of what we call "our" world, and that it is a continuous phenomenon. The unique character or singularity of life has been directly demonstrated by the sameness of the ultimate units of all living matter, the cells, indirectly by the refutation of the older theory of spontaneous generation; and has been enormously strengthened by the doctrine of descent, the phenomena of overcrowding, and the possibility of natural selection. The ubiquity of life—within certain limits—has been revealed directly by the microscope, and indirectly by the modern theories of disease, and of many forms of growth.[1] The continuity of

[1] There is a striking passage in Nansen's 'Farthest North,' vol. i. p. 445, showing the ubiquity of organic germs: "When the sun's rays had gained power on the surface of the ice, and melted the snow, so that pools were formed, there were soon to be seen at the bottom of these pools small yellowish brown spots, so small that at first one hardly noticed them. Day by day they increased in size, and absorbing, like all dark substances, the heat of the sun's rays, they gradually melted the underlying ice and formed round cavities often several inches deep. These brown spots were . . . algæ and diatoms. . . . I actually found bacteria,—even these regions are not free from them."

life has—as an inevitable corollary—come more and more into prominence. It has been the subject of much discussion, as a phenomenon which is felt to require a mechanical explanation.

The problem of the continuity in time of the forms and properties of living matter forced itself on the great propounder of the modern theory of Descent, on Darwin. He looked upon the principle of "Reversion[1]—this power of calling back to life long-lost characters—as the most wonderful of all the attributes of inheritance."

52. "Pangenesis." At the end of his second great work, ten years after the appearance of the 'Origin of Species,' he ventured on a hypothetical explanation, his theory of "Pangenesis," "which implies that the whole organisation, in the sense of every atom or unit, reproduces itself; hence ovules and pollen-grains, the fertilised seed or egg, as well as birds, include and consist of a multitude of germs thrown off from each separate atom of the organism."[2] This idea, as the author himself admitted, and as has since frequently been pointed out, was not fundamentally new: it had been anticipated by Buffon in his celebrated "organic molecules," and since Darwin it has been restated and adapted in various modified forms. It is hardly an explanation, but it is a statement which emphasises the great fact of modern biology,—the fact brought out by the cellular theory, that the units of life are not the large visible organisms which were formerly studied by preference, but the innumerable, infinitesimal living beings

[1] 'Animals and Plants under Domestication,' vol. ii. p. 372.

[2] 'Animals and Plants under Domestication,' chap. 27, vol. ii. p. 358.

called cells which, through growth and reproduction by division and fusion, maintain life as a continuous unique phenomenon.

Into this view, which under the special form of pangenesis has not found much favour, but which, nevertheless, in some form or other, forces itself more and more on our attention, Professor Weismann has imported a further distinctive feature, not prominently brought out by Darwin, though it also dates farther back[1] than the present generation.

[1] The history of the knowledge and theory of sex and heredity has been written in English by Profs. Patrick Geddes and J. Arthur Thomson, in a book entitled 'The Evolution of Sex' (1st ed. 1889); in French by M. Yves Delage, in his much-quoted work, 'La Structure du Protoplasma et les Théories sur l'Hérédité et les grands problèmes de la Biologie' (1895). The latter work contains elaborate criticisms, and finally inclines towards a theory of life termed in France "Organicisme," the main idea of which is the assumption of two distinctive factors in all the phenomena of living matter—viz., "Organisation and Environment." This view, according to the author, has not yet gained sufficient strength to form a definite current of thought like the three earlier views defined by the terms "Animisme," "Evolutionisme," "Micromérisme." The first of these centres in the idea of vital force, the second in the older school of evolution; the last begins with Buffon, and comprises the modern theory of Evolution with Spencer, Darwin, Haeckel, Weismann. Of the last M. Delage says: "Ce dernier est, pour le moment, l'ouvrage le plus parfait créé pour expliquer l'Hérédité et l'Évolution. Nous croyons avoir montré qu'il est bâti d'hypothèses fragiles, invraisemblables, et, tout en rendant justice au talent de son architecte, nous conseillons de l'admirer de loin et de construire ailleurs" (p. 837). "Organicisme" is represented by W. Roux, Driesch, and O. Hertwig, and is historically traced back to Descartes (p. 838), and to von Baer and Claude Bernard (p. 720). To the theories of the others, "les Organicistes opposent le concours d'une détermination modérée et des forces ambiantes toujours agissantes, toujours nécessaires, non comme simple condition d'activité, mais comme élément essentiel de la détermination finale" (p. 720). As in this account the names of Roux, Driesch, and O. Hertwig are placed together, it is well to remark that since that time the two last-named authorities have in various polemical publications signified the divergence of their fundamental conclusions from the later attitude which Prof. Roux has assumed. For those of my readers who desire to get some insight into the drift of this most recent and advanced controversy, in which questions of principle, of scientific and philosophical method, alternate with discussions of minute

Growth by intussusception and assimilation has long been recognised as the characteristic property of all living matter, of every living cell. Mechanical causes suffice to explain the further process of division as a necessary consequence of continued growth, the formation of new cells out of existing ones, the process of reproduction. Only in the lower organisms, however, does reproduction exist simply as multiplication by division. In all higher organisms at least, reproduction by division seems connected with the phenomenon of death of a portion of the dividing organisms: a differentiation seems to set in between the new cells, some gradually losing their power of self-multiplication by division, and thus being doomed sooner or later to arrive at the end of their organic existence; while others retain this power or regain it by uniting with others—the process of fusion of male and female elements —and seem thus to be specially endowed with the work of reproduction—*i.e.*, the preservation of the continuity of life. The great morphologist Richard Owen, about the middle of the century, in a tract on Parthenogenesis, remarked that "not all the progeny of the primary impregnated germ-cell are required for the formation of the body in all animals: certain of the derivative germ-cells may remain unchanged and become included in

embryological development, assisted or disturbed by experiments carried on in microscopic dimensions, I recommend, besides the larger works of Hertwig and Roux, already referred to, the highly suggestive writings of Hans Driesch, notably his 'Analytische Theorie der organischen Entwickelung' (1894), and 'Die Biologie als selbständige Grundwissenschaft' (1893). As a very helpful introduction to the original views of this writer, English readers will welcome the concluding chapter of Prof. E. B. Wilson's book, 'The Cell in Development and Inheritance' (1896).

that body, . . . so included, any derivative germ-cell or the nucleus of such may commence and repeat the same processes of growth by imbibition, and of propagation by spontaneous fission as those to which itself owed its origin."[1] We have here the first enunciation of that idea of a differentiation between the germ-substance and the body-substance, between that portion of living matter which is destined to preserve the continuity of life, and that other portion which, destined to differentiate more and more into the aggregate of living cells, each bearing a special form and carrying out a special function in the economy of the higher organisms, is at the same time doomed to death, gradually losing, as it does, its power of assimilation, growth, and division—*i.e.*, of self-preservation. Prof. Haeckel in 1866, and Dr Jäger in 1877, elaborated the idea further, pointing out that the "germinal" element or substance was that portion which in the process of division is reserved for the preservation of the species (the φῦλον, hence termed the phylogenetic portion), whereas the "personal" element or substance goes to form the body or individual (the ὄν, hence termed the ontogenetic portion).[2]

53. Germ-substance and body-substance.

[1] Darwin quotes this passage in a historical note to his theory of "Pangenesis" in the concluding chapter of his 'Animals and Plants under Domestication' (vol. ii. p. 375). He adds further, "By the agency of these germ-cells Prof. Owen accounts for parthenogenesis, for propagation by self-division during successive generations, and for the repairs of injuries. His view agrees with mine in the assumed transmission and multiplication of his germ-cells, but differs fundamentally from mine in the belief that the primary germ-cell was formed within the ovarium of the female, and was fertilised by the male. My gemmules are supposed to be formed, quite independently of sexual concourse, by each separate cell or unit throughout the body, and to be merely aggregated within the reproductive organs."

[2] Complete references to the earlier statements of this theory, which, through the various writings of Prof. Weismann (since 1881, when he read a paper, "On the Duration of Life," before the

This provisional statement, which emphasises the now generally recognised difference between the germ-substance and the body-substance, requires, however, two further qualifications in order to embrace the great characteristic facts of life and death as modern embryology and the phenomenon of descent have unfolded them.

Only in rare instances can we observe the continuity of cells—*i.e.*, of those organisms which, so far as our knowledge goes, form the ultimate units of living matter. Weismann recognised, as did the great botanist Nägeli, and long before both of these the philosopher Herbert Spencer, that though in the cell, with its nucleus and protoplasm, we may have arrived at the last microscopically visible independent units of life, we must—with the atomic theory in chemistry—assume the existence of much smaller units in all living matter, compared with which even the nucleus of the cell is a very complex aggregate. If the continuity of life is dependent upon that of an underlying living substance, this substance must be only an infinitesimal portion of any visible cell or nucleus. The conception of a continuous germinal substance has thus taken refuge in the more refined conception of a germ-plasma, as distinguished from the body or somatic plasma: the former is immortal within the limits of the conditions of organic life, the latter is

54. Germ-plasma and body plasma.

Naturforscher - versammlung at Salzburg, reprinted in 'Essays upon Heredity,' transl. by Poulton and others, Oxford 1889; see also the 'Studies in the Theory of Descent,' transl. by Meldola, 2 vols., 1882, and the earlier essays of Weismann mentioned in the preface, p. viii.), has become both scientifically and popularly recognised and debated, are given in Geddes and Thomson, 'The Evolution of Sex,' p. 93; also in M. Delage's great work, p. 349, &c., and in Wilson, 'The Cell,' p. 295, &c.

perishable, mortal, doomed, after temporarily serving the purposes of individual development, to disappear from the category of living matter.

55. Differentiation of germ plasma.

And secondly, it appears that the germinal substance or germ-plasma, when once differentiated from the personal substance or body-plasma, cannot, as a rule, perform unaided the function of continuous preservation of the species or phylum. In all the higher animals the germ-substance appears in two distinct seemingly complementary forms, and only by the fusion of these does the development of the germ-substance become possible.

The great difficulties which stand in the way of applying these conceptions (which have found an exhaustive exposition in Prof. Weismann's 'Essays on Descent and Heredity') to the vegetable kingdom have been pointed out, and have prevented their general adoption by biologists;[1] nor have the elaborate modifications introduced in Prof. Weismann's later writings tended to make them more acceptable; the idea, nevertheless, of a fundamental differentiation of the elements of living matter into germinal and personal has got hold of the scientific mind at the present day, and cannot be

[1] On the objections of Prof. Strasburger, who points to the fact that in the case of begonias the fragment of a leaf planted in moist sand can reproduce the whole plant; of Prof. Vines, who shows that whole groups of champignons, which propagate annually, are nevertheless rich in genera and species, which have evidently descended from one another, see Yves Delage, 'L'Hérédité,' p. 526, &c.; 'Nature,' vol. x. p. 621; also O. Hertwig, 'The Biological Problem of To-day,' transl. by P. C. Mitchell (1896), p. 40, &c. On the discovery of Weismann "that in parthenogenetic ova only one polar globule is formed, while there are always two in ova which are impregnated," and the "momentary" presumption in favour of his theory which it afforded, see 'Essays on Heredity,' p. 333, &c.; Geddes and Thomson, 'Evolution of Sex,' p. 180, &c.; and Delage, 'L'Hérédité,' p. 151.

passed over in a history of Thought. Moreover, it has made itself felt by giving rise to two separate views of the cause of variation—*i.e.*, of that phenomenon in the living creation on which the entire modern theory of descent is founded.

If it be true that the preservation of the species, the continuity of living forms, is dependent on the germ-plasma, whereas the somatic plasma, from this point of view, only serves individual ends and is a receptacle or temporary dwelling-place for the germs which it transmits but does not create, the experiences of the body, its changes and development, can have little or no influence on the hidden germs and their further history. Thus Weismann is led to a denial of the influence of environment, of habit and acquired characters, except in those cases where, as in the lower organisms, no differentiation has set in between the germinal and the personal substance. This amounts to a negation of those modifying influences which Lamarck emphasised, and which play such a great part in the theories elaborated by Darwin, Haeckel, and especially by Herbert Spencer. On the other side, it has led Weismann to lay a much greater weight upon sexual selection and the effects of crossing in the process of descent and the phenomena of heredity. But for sexual selection, and the endless combinations of different germ-plasmas, there would, according to Weismann, be no variation, and hence no development of the higher forms of life. The controversy turns mainly upon the inheritance of acquired characters, of which indeed no genuine and authenti-

56. Weismann *v.* Lamarck.

cated case seems to have been established.[1] On the other side the influence of crossing, of the repeated division and fusion of different germ-plasmas, to which Darwin in his later writings attached more and more importance, and on which Weismann relies exclusively for an explanation of variation and natural selection, is denied by some biologists to tend in the direction of the gradual growth of definite characters: they point rather to the obliterating and diluting influence of such promiscuous fusion, and they maintain that the presence of an environment which always acts in a constant manner[2] is indispensable.

If we now look back for a moment on the fundamental change of ideas which the century has brought about in the biological aspect of nature, we are bound truly to halt in astonishment. In no department of thought have comparatively small beginnings and detailed discoveries, referring to infinitesimally small phenomena, led to such revolutionary ideas concerning those phenomena which most intimately affect our personal interests—the problems of life and death, of conduct and of health. The whole of this change has been brought about by introducing and extending those

[1] It is needless to give special references, as all the recent works on the subject, which have been largely quoted in this chapter, deal with this point. See, however, Yves Delage, 'L'Hérédité,' p. 196, for a very complete bibliography. He concludes as follows: "Il n'est pas démontré que les modifications acquises sous l'influence des conditions de vie soient généralement héréditaires, mais il paraît bien certain qu'elles le sont quelquefois. Cela dépend sans doute de leur nature. D'ailleurs on ne sait pas quelle est dans ce résultat la part de la transmission des modifications somatiques aux cellules germinales et celle de l'action directe des conditions ambiantes sur celles-ci" (p. 221).

[2] Hertwig, 'The Cell,' p. 319.

methods of investigation and reasoning which have been learnt in the mechanical, physical, and chemical sciences: the processes of observation, measurement, and calculation. And yet it may be asked, have we come nearer an answer to the question, What is Life? At one time, for a generation which is passing away, we apparently had. But a closer scrutiny has convinced most of us that we have not. The study of life has indeed been transferred from the higher and more complex forms to the lower, the minuter, and the simpler; and now lingers by preference among cells, germs, and primitive organisms, out of which we have learnt to consider the higher ones as put together on the principles of co-operation, division of labour, and mutual
57. accommodation. The problem "What is Life?" has

57. Two aspects of the problem of life.

in all this gained a twofold aspect. Wherein consists the peculiarity of the smallest unit of living as compared with not-living matter? In organisation we are told, in growth through intussusception, in metabolism; but we are far from being able mechanically to describe these phenomena or processes. The spectre of a vital principle still lurks behind all our terms.[1] On the other

[1] If we broadly summarise the properties peculiar to living things which the nineteenth century has dwelt on in an original manner under the three conceptions of adaptation (fitness), selection (natural or sexual), and organisation (order or harmony), the question presents itself, Is any of these much-used terms intelligible or definable without reference to something which is extraneous to the object we treat of, this reference existing in our own thinking or contemplating mind, and, if actually present in natural objects themselves, then also indicative of the existence of some immaterial principle? Though this is manifested in mechanical contrivances which it has left behind with its signature upon them, it is nevertheless vaguely analogous to the selective, purposeful, or orderly performances of a human intellect. The exclusive study of detail on the one side, the aspect of the whole on the other, will always induce opposite answers to this question. In addition to the literature given in the notes to this chapter, I may refer to the following tracts which deal specially with the problems of mechanism and vitalism. Hans Driesch, 'Die mathematisch-mechanische Betrachtung morphologischer Probleme der Biologie' (Jena, 1891); O. Bütschli, 'Mechanismus und Vitalismus' (Leipzig, 1901); Eugen Albrecht, 'Vorfragen der Biologie' (Wiesbaden, 1899).

side, the union or co-operation of many essentially similar units in a complicated organism brings out more and more, as we ascend in the scale of living things, a new phenomenon, a new kind of unity, that which we term "individuality," the wealth of an inner self-conscious life, to which the older school of biologists attached primary importance. Life accordingly has now for us two sides —first, the life of the smallest, the most primitive unit of living matter, say the cell, the amœba, or, if you will, the idioblast, the gemmule, the germ-plasma, the physiological unit. Secondly, the life of the complex society of cells, the higher organism in which the inner world with all its mental phenomena has become manifest. How is the unity of this higher complex possible? In what does it consist? What can we know of it? Neither the physiological nor the psychological unity is intelligible to us. An eminent biologist, to whom we owe the creation of an entire new science, the late Professor Virchow, the founder of Cellular Pathology, has told us recently[1] that only since biologists have ceased to try to understand the unity of life in the higher organisms, the psychological unity, and have realised the fact that the unity of life is in the autonomous cell, has biology in theory and practice made much progress. Be it so. It seems likely that the progress of biology depends entirely on the cultivation of the mechanical view; but from another and

[1] In the Huxley Lecture of 1898.

equally legitimate aspect, the unity of the complex as the bearer of all the phenomena of higher or inner life is equally important. In many ways it is a counterpart of the other, showing a peculiar continuity of its own, that continuity which I have made the special subject of this work. In proportion as the biological view of nature has become the science of the cell, another science has grown up which sets itself to study this higher phenomenon of living matter, the phenomenon of mind, directly by the methods of the exact sciences. This is the modern Science of Psycho-physics.

58. Transition to psycho-physics.

Even the microscopist and biologist of the most modern type are occasionally startled by phenomena akin to those which commonly are only visible in the highest organisms. Psychical existence, an inner side to the external phenomena of motion, has accordingly been attributed by eminent representatives of the mechanical view of biological phenomena to the lowest, the most primitive, unit of living matter. Another school of science has set itself to study this inner side of living organisms in its more perfect, as it were full-grown, manifestations, and by appealing in addition to the facts only known by introspection or self-consciousness. With the history of this movement, so far as it belongs to exact science, I propose to deal in the next chapter under the general title of the Psycho-physical View of Nature.

CHAPTER XI.

ON THE PSYCHO-PHYSICAL VIEW OF NATURE.

1. Abstract and concrete sciences.

In the three foregoing chapters I have attempted to trace the development of the different aspects under which our knowledge of the real things which surround us, and of nature as a whole, has been extended in recent times. I have brought these different aspects which respectively consider things natural according to their forms, their genesis, or their life and purpose, under the general name of the biological as distinguished from the abstract view, with which I dealt in the four previous chapters. The abstract view tries to arrive at the general properties of all things, which it has succeeded in our times in summing up under the great generalisations of Attraction, Atomism, Kinetics, and the doctrine of Energy. The biological view is interested not so much in general properties as in real specimens—the things, beings, and phenomena in which we see the general properties exemplified and become real and in their actual union or totality which we call nature. The abstract sciences started on their modern career with mathematics, and progressed through the development and application of

the mathematical methods to the data furnished by observation and experiment; the biological or concrete sciences began with a study of living things, and have progressed immensely in our times by viewing these not in isolation, but in their relations to each other and to the surrounding lifeless world—the so-called environment. An exact treatment, that to which the term "scientific" has been pre-eminently applied, seems here also to depend largely, if not exclusively, on the degree to which the mathematical processes of numbering and measuring can be applied, and on the utilisation of the general results arrived at in the abstract sciences.

2. Their different methods.

The method of the abstract sciences is that of building up from small beginnings, by the process of summation or integration, intricate complexes which not infrequently are found to correspond to phenomena of actual experience. It has at its command the unlimited resolving powers of the calculus, and the well-established assumption that things natural are made up of numberless particles entering into innumerable combinations. The whole is thus for the mathematical view the sum of its parts. The concrete or natural sciences, on the other hand, start with the ready-made things or creatures of nature, or on a larger scale with the great order and economy of our world or the universe, and only descend into the minutiæ of the observatory, the dissecting-room, or the laboratory, with the hope of better understanding the great and complicated objects of their study. The greatest progress in the abstract sciences has been made by those minds that could concentrate their attention on special points, not infrequently expressed in

mathematical formulæ, and expand their view through applications: the greatest progress in the natural sciences has been made by those who started with a large and comprehensive view of things natural, and gradually descended into detail. Newton, Lagrange, Fresnel, and Helmholtz are good examples of the former; Humboldt, von Baer, Claude Bernard, and Darwin of the latter.

Now, it is a frequent experience that in the study of things natural, through the unavoidable process of dissection and analysis, the subsequent synthesis or summing up has not carried the student back to the real thing from which he started, but to some artificial product differing essentially from the natural object. The real essence of the thing seemed lost when its parts were examined by themselves or in their apparent aggregation. A prominent example of this kind is to be found in the living organism. Theories have accordingly been formulated which looked upon life as a special principle to be superadded to any conceivable aggregation of mechanical processes, in order to raise them from the lifeless into the living order of things. The last chapter dealt with the various biological hypotheses, of which three are conspicuous: the purely mechanical, according to which the living organism is merely a very complicated chemical molecule; the vitalistic, which establishes an essential difference between the action and constitution of a living and a lifeless unit of matter; and an intermediate view, which looks upon organisms as manufactured machines built up according to some plan, design, or idea, the nature of which can be further inquired into, but which does not try to throw any additional

light on the mechanism itself, the working of which, like that of a clock, can be described on purely mechanical lines and without reference to the idea which preceded its construction.

According to many prominent naturalists, the evident design and purpose which characterise so many phenomena of living matter are explained on purely mechanical lines by the inherent or forced teleology of living things, which through over-production have to submit to an automatic process of selection or survival. To others this automatic process does not seem to suffice, and they assume a principle of progress which acts in a regulative manner. This vitalistic view is further supported by taking into account an extensive class of phenomena which I have, so far, hardly noticed—the marvellous properties of the higher creations of the animal world which exhibit the phenomena of

3. Inner experience.

consciousness or of an inner experience. That these phenomena belong to the realm of natural science as much as any other properties of living things cannot nowadays be doubted. The division into natural and mental science can no longer be upheld, or only with a very different meaning from that which it had for a bygone age.

It will be my object in this chapter to give an account of the various and changing aspects which this great phenomenon of an inner or conscious life has presented to naturalists—*i.e.*, to those who have approached the phenomena of Mind from the side of nature, and of the different lines of research and reasoning along which they have dealt with it. I shall comprise the whole of

this section of scientific thought under the general term of Psycho-physics.[1] It refers to the borderland or common ground where physical and mental or psychical phenomena meet or interact.

4. Psycho-physics.

Although the term psycho-physics is quite modern, the idea of a special science dealing with the relations of mind and body, or of the physical and mental life of the human organism, has been prominently before the scientific world ever since Cabanis published his celebrated 'Rapports du Physique et du Moral de l'Homme,' in which the well-known passage occurs which has been frequently repeated, modified, and quoted with varying approval or reproach:[2] "In order to arrive at a correct

[1] The term was first used by G. T. Fechner in the well-known work bearing this title, of which I shall have more to say in the course of the chapter. This work, dealing mainly with a certain numerical relation, narrowed the term down to a special investigation, whereas the larger problem, the study of the interaction of mind and body by the methods of the exact sciences, was variously designated as physiological psychology, mental physiology, psycho-physiology or physiology of the soul. As there is a tendency to regard physiology more and more as the physics of the living organism, it is evident that physics is the larger term; and in dealing with the relations of the physical and the psychical in the widest sense, the term psycho-physics seems the more appropriate.

[2] 'Œuvres complètes' de Cabanis (1834), vol. iii. p. 159. The simile has attained a sort of historical celebrity through the drastic version which was given to it by Karl Vogt in his 'Physiologische Briefe' (1847), p. 206, where, with a distinct intention of rousing an æsthetic disapproval, he compares the function of the brain with the secretion of bile by the liver and of urine by the kidneys. This dictum, which he repeated in his controversy with Rudolph Wagner, led in the middle of the century, as Du Bois-Reymond tells us, to a kind of systematic championship of the soul, the comparison with the kidneys being looked on as a degrading offence. "Physiology, however, has no knowledge of such grades of dignity. As a scientific problem the secretion of the kidneys is to her of the same dignity as the investigation of the eye or the heart or any other so-called noble organ." Vogt used the simile as an illustration of his purely materialistic view. Lange ('Hist. of Materialism,' vol. ii. p. 242) shows that with Cabanis the dictum is by no means bound up with such a view, as he really was a pantheist. The mistake, says Du Bois-Reymond, does not lie in the comparison, but in the implied suggestion, that psychical

5. Cabanis's simile.

idea of those operations from which thought arises, we must consider the brain as a particular organ, destined specially to produce it in the same way as the stomach and the intestines are there to perform digestion, the liver to filter the bile, the parotid, maxillary, and sublingual glands to prepare the salivary juice."

The argument which led Cabanis to draw this parallel between the functions of the brain and those of other organs of the human body was based upon the philosophy of Locke, which had been domiciled in France by Condillac and Helvetius. This philosophy, in its popular version, taught that all our thoughts and ideas were ultimately made up of sensations.[1] On the other side,

activity could be "explained through the structure of the brain, as secretion can be explained from the structure of a gland" ('Reden,' vol. i. p. 129).

[1] Cabanis (1757-1808), in the preface to the 'Rapports,' &c., p. 11, gives a list of contemporary French writers who, following in the line of Locke, to whom "philosophy is indebted for the greatest and the most useful impulse," have taken up different sides of the doctrine. Of their writings a very clear and exhaustive analysis will be found in M. Picavet's 'Les Idéologues, Essai sur l'histoire des idées et des théories scientifiques, philosophiques, religieuses, &c., en France depuis 1789' (Paris, 1891). Cabanis's own position is very clearly defined (p. 16) when he says that "Les opérations de l'intelligence et de la volonté se trouveraient confondues à leur origine avec les autres mouvements vitaux : le principe des sciences morales, et par conséquence ces sciences ellesmêmes rentreraient dans le domaine de la physique ; elles ne seraient plus qu'une branche de l'histoire naturelle de l'homme : l'art d'y vérifier les observations, d'y tenter les expériences, et d'en tirer tous les résultats certains qu'elles peuvent fournir, ne différeraient en rien des moyens qui sont journellement employés avec la plus entière et la plus juste confiance dans les sciences pratiques dont la certitude est le moins contestée." This was written in 1802. M. Picavet says of Cabanis with much truth: "Le continuateur d'Hippocrate, de Descartes et des philosophes du XVIII^{me} siècle, a été un précurseur de Lewes et de Preyer, de Schopenhauer et de Hartmann, comme de Lamarck, de Darwin et de bien d'autres penseurs qui appartiennent aux écoles les plus différentes, et ne soupçonnent quelquefois même pas que les idées dont ils sont partis leurs sont venues indirectement, mais par des intermédiaires authentiques, de l'auteur des 'Rapports du physique et du moral'" ('Les Idéologues,' p. 264). M. Picavet also gives valuable explanations how it came about that

the physiologists of the eighteenth century, notably Haller, had demonstrated that the properties of the physical organism culminated in those of the nervous system—irritability and sensibility. The phenomenon of sensibility, of producing and combining—as it were digesting—sensations, was thus the function of the brain, or the central organ of the nervous system, as other processes were the functions of other organs or physiological apparatus. Cabanis was led on from medical[1] studies, as Locke had been before him, to the study of mental and moral subjects, and he formed the conception of a science of Man, or Anthropology,[2] divided into Physiology, the Analysis of Ideas, and Morals, which would ultimately be of as much use for the practical purposes of education and government as the exact study of other natural phenomena then cultivated in France for the purposes of medicine, industry, and material civilisation.

6. Prepared by Locke and Haller.

Although it may be admitted that Cabanis created[3] physiological psychology, and that he cast far-reaching glances into the neighbouring departments of animal,

the line of philosophical thought so clearly indicated by Cabanis was not more systematically developed in France at the time, and, like many other lines of research which originated in that country, had to be re-discovered fifty years later in other countries. The question is important, and may occupy us later on. See, however, regarding the disfavour into which the "moral" sciences fell owing to political reasons, vol. i. p. 149 of this work.

[1] Cabanis blames in Condillac and Helvetius that they knew nothing of physiology. "S'ils eussent mieux connu l'économie animale, le premier aurait-il pu soutenir le système de l'égalité des esprits ? le second n'aurait-il pas senti que l'âme, telle qu'il l'envisage, est une faculté, mais non pas un être ; et que, si c'est un être, à ce titre elle ne saurait avoir plusieurs des qualités qu'il lui attribue" (ibid., p. 66).

[2] "C'est ce que les Allemands appellent l'anthropologie ; et sous ce titre ils comprennent en effet les trois objets principaux dont nous parlons" (Cabanis, 'Œuvres,' vol. iii. p. 40).

[3] Picavet, *loc. cit.*, p. 292.

embryological, and morbid psychology, from which he expected much assistance, his ideas remained vague, as did those of the contemporary school of the "Idéologues," among whom Destutt de Tracy[1] deserves honourable mention as having conceived the plan of a psychological treatment of grammar. Their merit lay more in drawing the plans of the new science of psychology as a natural science in its largest sense, and of urging its scientific and exact treatment, than in making a real and fruitful beginning on special lines.

7. Berkeley's 'Theory of Vision.'

It is a remarkable fact that the first attempt to analyse in detail one of the special instances of psycho-physical interaction came about a hundred years earlier from that successor of Locke who has always been counted as the extreme idealistic development of English speculation. Bishop Berkeley's 'Essay towards a New Theory of Vision' (1709) has been called "the veritable historical starting-point of psycho-physical investigation."[2] Although averse to any exact theory of the universe, deeming it "beneath the dignity of the mind to affect exactness,"[3] and at war with the mathema-

[1] Picavet (p. 398) says of Destutt de Tracy (1754-1836): "Venu par les sciences à la philosophie, D. de Tracy a donné à l'idéologie un nom et un caractère positif. S'il a cru, à tort, qu'il pouvait la constituer de toutes pièces, il a fort bien vu que, pour devenir une science indépendante et complète, elle devait s'appuyer sur la physiologie et la pathologie, sur l'étude des enfants, sur celle des fous et sur celle des animaux. Il l'a unie intimement à la grammaire et à la logique, à la morale et à l'économie politique, à la législation et à la politique."

[2] Dr Edmund Montgomery, in his very interesting and valuable critical analysis of 'Space and Touch,' three memoirs contained in the tenth volume of the first series of 'Mind' (1885), p. 385.

[3] See 'A Treatise concerning the Principles of Human Knowledge,' § 109: "As in reading other books, a wise man will choose to fix his thoughts on the sense and apply it to use, rather than lay them out in grammatical remarks on the language; so in perusing the volume of nature it seems beneath the dignity of the mind to effect an exactness in reducing each particular phenomenon to general rules, or showing how it follows from them. We should propose to ourselves nobler views, such as to recreate and exalt the mind," &c. In the following paragraph Berkeley refers to the 'Principia' as "the best grammar of the kind" he was speaking of.

ticians,[1] as Hobbes had been before him, Berkeley had a clear conception of the following definite problem: By what succession of physical and mental experiences, by what "organic and vital data," do we become aware of space and of body or matter? His answer, which makes tactile sensations the base, has been advocated and quoted by English psychologists of the Association school up to the present day, and forms the text for their various explanations.

The genesis of space perception was much discussed in the circle of Locke's friends, Molyneux proposing the celebrated query[2] named after him, and Cheselden describing at length, in the Philosophical Transactions, the experiences of an adult blind patient who had received his sight by couching. The eighteenth century brought other isolated researches of an experimental or mathematical nature, which may be regarded as the beginnings of an exact treatment of the relation of psy-

[1] A very full account of this controversy will be found in a paper by Prof. Geo. A. Gibson in the 'Proceedings of the Edin. Math. Soc.,' vol. xvii.

[2] The query is given in Locke's 'Essay,' Book II. ch. ix. § 8, as follows: "Suppose a man born blind, and now adult, and taught by his touch to distinguish between a cube and a sphere of the same metal and nighly of the same bigness, so as to tell when he felt one and the other, which is the cube and which the sphere. Suppose, then, the cube and sphere placed on a table, and the blind man made to see: Query, whether by his sight, before he touched them, he could now distinguish, and tell, which is the globe, which is the cube? To which the acute and judicious proposer answers, No." For a full analysis of actual cases, such as that of Cheselden, and more recent ones, see Wundt, 'Physiologische Psychologie,' vol. ii. p. 233. That Berkeley was, however, neither a psycho-physicist nor a physiological psychologist in the modern sense, is well remarked by Campbell Fraser in his essay on Berkeley (Blackwood's "Philos. Classics," 'Berkeley,' p. 45, &c.)

chical with physical phenomena. Fechner, the founder of psycho-physics as an independent doctrine, refers notably to two[1] such instances. They were contributed by two great mathematicians, Daniel Bernoulli and Leonhard Euler. The former pointed out that the value which we attach morally to the addition to any material possession is not measured by the actual magnitude of such addition, but by the relation it bears to that which we already possess. The first sovereign earned by a poor and starving labourer has an almost infinite value compared with what it has for a person already possessed of a million. Laplace and Poisson referred to this statement of Bernoulli, and introduced the terms "fortune physique," "fortune morale," showing that they stand in a simple mathematical relation. The same relation was shown by Euler to exist between our estimate of musical intervals in the harmonic scale and the difference of the number of vibrations of the strings which produce the two notes. It was above a century before Fechner correlated these isolated remarks with observations of modern psycho-physics in his celebrated law, of which more anon.

8. Bernoulli and Euler.

On the whole, little progress was made during the eighteenth century in the department of research I am now dealing with; but the end of the eighteenth and the beginning of the following century brought several important discoveries, some of which were at the time much over-estimated, whilst others were for a long time forgotten or overlooked.

The first is the accidental discovery by Galvani in

[1] 'Psychophysik,' 1860, vol. ii. p. 548, &c.

1786, followed, fifteen years after, by Volta's greater invention. The late eminent Prof. Du Bois-Reymond, in various passages[1] of his scientific and literary writings, has told us of the recurrent fascination which the *fata morgana* of Electricity has exercised over those interested in the explanation of the phenomena of innervation; how this seductive clue has been, in the

9. Animal electricity.

[1] See vol. ii. pp. 212, 386, 528 of Du Bois-Reymond's 'Reden,' also his 'Untersuchungen über thierische Electricität' (1848), vol. i. pp. 30-128. One of the first to take up in the interests of nervous physiology the clue which Galvani's discovery afforded was A. von Humboldt, who published in 1797, three years before Volta's discovery, his valuable "Versuche über die gereizte Muskel- und Nervenfaser, nebst Vermuthungen über den chemischen Process des Lebens in der Thier- und Pflanzenwelt." A lucid account of Humboldt's work is given by Prof. Wundt in the third volume of the German edition of Bruhns' 'Life of Humboldt,' p. 301 *sqq.* "It is difficult," he says, "to picture to oneself nowadays the excitement which the observations of Galvani produced in the scientific world. . . . Such experiments had almost become a general subject of entertainment in cultured circles. . . . It almost appeared as if what at that time was looked upon as the most general property of living matter, irritability, were by the experiment of Galvani to be for the first time unveiled in its real essence. . . . At the time when Humboldt made his experiments the contest was still going on between the followers of Galvani and Volta." This referred to a physiological or purely physical explanation of the phenomenon. "Barely three years after the publication of Humboldt's work the discovery of Volta's pile put an abrupt end to all theories which were based upon the physiological origin of galvanic phenomena. The brilliant development of physical galvanism from that moment pushed the physiological aspect of electricity for a long time into the background. . . . Humboldt's work was forgotten" (p. 310). In the meantime Humboldt had travelled in South America, where he had—*inter alia*—observed the "natural electromotors which stand in such extraordinary connection with the nervous system" of the electrical eel (*Gymnotus electricus*), giving a thrilling description of a battle between the horses and the eels which he witnessed in the waters of Calabozo. (See Humboldt's 'Personal Narrative,' vol. iv. p. 345 *sqq.*; also 'Ansichten der Natur,' vol. i. p. 33.) Interest in the subject of animal electricity was again revived by Italian physiologists about the year 1835. Nobili, Marianini, Santi-Linari, Matteucci repeated and enlarged the experiments of Galvani, and through the influence of Humboldt and Johannes Müller, the study of the whole subject was comprehensively taken up at Berlin by Du Bois-Reymond about 1840, and exhaustively treated in his great work on the subject (vol. i. 1848, vol. ii. 1860).

course of more than a century, alternately taken up with enthusiasm, and abandoned as misleading. At the turn of the centuries the mania for animal electricity was at its height. Men like A. von Humboldt took up the study with eagerness, and sovereigns like Napoleon offered special prizes, in the hope that here at last the secret of life and consciousness would be revealed. The school of the "Naturphilosophie" in Germany seized upon the suggestion of polarity and polar forces contained in the phenomena of galvanic action, and, supported by the still more mystical processes of the so-called animal magnetism which had been exhibited by Mesmer twenty years earlier, worked up these vague indications into fanciful theories of vitalism and animism. This brought the whole line of thought into discredit, drove away the soberer, more scientific students of nature, and retarded real progress in the knowledge of the electric phenomena of the muscular and nervous system for fully a generation. At length in the school of Johannes Müller the subject was again approached and was put on a firm scientific basis by Helmholtz, and notably by Du Bois-Reymond. It is now known that, as in inorganic, so also in organic systems, the energy proper to them can appear under the different forms of mechanical, thermal, electric or chemical energy, but also that in none of these can be found pre-eminently the principle of life, still less that of consciousness.

10. Phrenology.

Another important line of research which has had an equally fluctuating development, being sometimes enormously exaggerated, to the damage of sound pro-

gress, sometimes repudiated and treated with wholesale contempt, was that started by Gall, who from the year 1805 onward, and latterly in conjunction with Spurzheim,[1] started on an anatomical description of the brain as the centre of nervous and conscious mental

[1] The two most prominent teachers of phrenology were Franz Joseph Gall (1758-1828) of Pforzheim, and Joh. Christ. Spurzheim (1776-1834) of Trier, the former an excellent doctor, the latter a skilled anatomist. Their influence was centred in Vienna and Paris. In England and America phrenology dates its popularity from George Combe (1788-1858). The term phrenology was suggested by George Forster about 1815, ten years after Gall had started his 'Schädellehre' or 'Craniology.' Of eminent medical authorities, the great Broussais in France (1772-1838) and C. G. Carus (1789-1869) in Germany were both phrenologists, the latter attempting to give the doctrine a more scientific foundation. Though phrenology was never popular in France, where the Academy of Sciences from the beginning assumed a very sceptical attitude (see above, vol. i. p. 136 note), the opponents of Gall have always given him full credit for his ability, and for the great impulse he gave to anatomical science of the brain. Flourens, one of the most formidable critics of the doctrine of the special faculties, and consequently of the separate phrenological organs and their location, nevertheless says: "Gall fut un observateur profond, qui nous a ouvert, avec génie, l'étude de l'anatomie et de la physiologie du cerveau. . . . Je n'oublierai jamais l'impression que j'éprouvai la première fois que je vis Gall disséquer un cerveau; il me semblait que je n'avais pas encore vu cet organe" (quot. by Langlois, 'Grande Encyclop.,' vol. xxvi. p. 801). Somewhat earlier than phrenology the science or art of physiognomics, which was known already and practised by the ancients, had a representative in Caspar Lavater of Zürich, who, from 1772 onward, published his 'Physiognomische Fragmente,' a work which, accompanied by engravings by Chodowiecki, created a great sensation in philosophical, literary, and artistic circles, the whole of Europe being divided into followers and critics of Lavater. Among the latter was the celebrated Lichtenberg of Göttingen. Among scientific men were Camper in Holland, and later Charles Bell in England; the former putting forward the well-known theory of the "facial angle" as an external measure of intelligence, the latter publishing his 'Essay on the Anatomy of Expression' (1806). In more recent times no less an authority than Charles Darwin took up the subject in his work on the 'Expression of Emotions' (1872). Shortly before Ph. Piderit published his 'Wissenschaftliches System der Mimik und Physiognomik' (1867); Duchesne (1862) his 'Mécanisme de la physionomie humaine'; and more recently the Italian Mantegazza his 'Physionomie et l'expression des sentiments' (French transl., 1885). A very readable essay on the subject will be found among Prof. Wundt's 'Essays' (1885). See also his 'Physiologische Psychologie' (vol. ii. p. 598, &c., 4th ed.)

action. The scholastic notion of the older psychologists which divided the mental life into different powers or faculties as the body was dissected into parts and organs, lent itself to the idea of a localisation of these faculties or powers in different spheres of the brain, which Gall by a hasty generalisation maintained to be distinguishable on the external surface of the skull. Though these popular and practical applications, which form the basis of phrenology, were speedily and easily refuted, having always been regarded with suspicion by the medical profession, the anatomical labours of Gall were taken up and continued by others. Opinions fluctuated between the different views of Flourens, who insisted upon the unity of the central organ, as did Herbart in psychology on the unity of the mind; of G. H. Lewes, who assigns to the spinal cord together with the brain an important and initiatory *rôle* in conscious life; and of Hermann Munk and Friedrich Goltz, who by carefully devised experiments on living animals, by electrical irritation, and by systematic removal of parts of the brain, have to some extent succeeded in delimiting the special "spheres in which the various sensory nerves deliver their messages, and where the latter are transformed into conceptions and mentally stored."[1] Paul Broca had already, about forty years ago, succeeded in localising the powers of speech.

[1] Du Bois-Reymond, 'Reden,' vol. ii. p. 558: "Though there is, in principle, no hope that the causal connection between material processes in the brain and consciousness will ever become clear to us, this does not hinder our penetrating deeply into a knowledge of those processes, or prevent such knowledge being of the greatest importance and of fascinating interest. As a first step in this direction there presents itself naturally to our understanding the localisation of the different faculties into which we naturally and systematically divide mental activity. Out of the desire for such localisation there sprang up the fundamental idea of the phrenological follies; but, as so often, here also scientific superstition contained a kernel of truth. In the same cortex of the brain in which Gall and Spurzheim located their badly-chosen thirty-five mental faculties, Munk now describes the spheres in which the various sensory nerves deliver their messages, and where the latter are transformed into conceptions and stored. Thus, for the first time in the domain of sensation and intellection, a local basis of mental activity has been demonstrated, as had been done before by Paul Broca in the domain of volition, in the localisation of the faculty of speech." Most modern psycho-physicists would probably accept this statement with slight modifications; it is therefore well to note that one of the foremost and most original workers in this field of research, Prof. Fr. Goltz, takes a different view of the result of the experiments of himself and others. He does not consider Munk's teachings as the foundation of a physiology of the brain, but looks upon them as a system of error, and "hopes to see the day when all the beautifully elaborated modern hypotheses of circumscribed centres of the cortex will be laid in the same grave in which Gall's phrenology rests" (quoted from Goltz's memoirs, 'Über die Verrichtungen des Grosshirns,' in Pflüger's Archiv, by Carl Hauptmann, 'Die Metaphysik in der modernen Biologie' (1804), p. 240). Prof. Ferrier, whose 'Functions of the Brain' (2nd ed.) is a standard work in the English language, takes up a less negative position; yet he says (p. 23): "We are still on the threshold of the inquiry, and it may be questioned whether the time has even yet arrived for an attempt to explain the mechanism of the brain and its functions. To thoughtful minds the time may seem as far off as ever." Prof. William James of Harvard, in his excellent 'Principles of Psychology' (2 vols., 1891), gives, in his first chapter, a succinct account of the "localisation-question," which, he thinks, "stands firm in its main outline" (vol. i. p. 162). The standard work in the German language is Prof. Wundt's 'Physiologische Psychologie' (2 vols., 4th ed., 1893), which gives in the first division (chaps. 4, 5) a very exhaustive account of the experimental and theoretical work on localisation. Prof. Wundt himself takes up a position lying between the doctrine of sharp delimitation and that of a denial of local distinctions (vol. i. p. 159), but admits that the whole question is still highly controversial, though latterly the apparent differences of opinion have been much toned down (vol. i. p. 240).

Whilst animal electricity and the examination of the brain were taken up with ardour, over-valued by popularisers, and developed into fanciful theories which postponed for a long time the sober inquiries of science, another very fruitful vein of reasoning and research was struck early in the century, but left unexplored for fifty years. Since then it has been followed with success and profit.

11. Dr Young's colour theory.

The beginnings of this line of reasoning are to be found in the writings of Thomas Young, who here, as in several other directions, "marched far in advance of his age."[1] During the last decade of the eighteenth century Young had been occupied with the study of the phenomena of Light and Colours; and, being a student of medicine, he had given equal attention to the physical phenomena and the physiological sensations of Light, going back to the beginnings laid in Newton's writings on these two important branches of Optics.[2] I have treated of his epoch-making discoveries in physical optics in an earlier chapter. As to the physiological problem of colour sensations, he likewise reviewed Newton's work, and especially took up the remarkable fact noted by Newton, that it appears possible to refer the great variety of colour sensations to three primary elements, out of which the whole wealth of the colour scale—varying in intensity, tint, and saturation—can be made up. In two distinct points he made a definite

[1] Note, in many passages of Helmholtz's 'Physiologische Optik' (2nd ed., Braunschweig, 1896), and his often-quoted 'Vorträge und Reden,' the high esteem in which he held the work of Young.

[2] A very succinct and exhaustive account of how Young arrived at his colour theory is given in a paper by A. M. Mayer, of New Jersey, in the 'Phil. Magazine' for 1876 (5th series, vol. i. p. 111). Young first selected red, yellow, and blue as the three simple colour-sensations, but later modified his view in consequence of the experiments of Wollaston between the years 1802 and 1807. How little Young's theory was thought of may be seen from the words of Helmholtz, quoted by Mayer (p. 114): "The theory of colour, with all these marvellous and complicated relations, was a riddle which Goethe in vain attempted to solve; nor were we physicists and physiologists more successful. I include myself in the number; for I long toiled at the task without getting any nearer my object, until at last I found that a wonderfully simple solution had been discovered at the beginning of this century, and had been in print ever since for any one to read who chose. This solution was found out and published by the same Thomas Young who first showed the right method of arriving at the interpretation of Egyptian hieroglyphics."

advance upon Newton. For the three primary colours of the older opticians he substituted red, green, and violet; and for the remarkable fact that the simple colours of the rainbow can be compounded out of these three, he suggested a physiological reason—viz., that the eye possesses three distinct colour-sensations or three distinct senses in relation to light, dependent upon some peculiarity of nervous structure or function. Young did not elaborate his ideas, but it is clear that in the short passages in his 'Lectures on Natural Philosophy' and earlier papers, there were contained a variety of definite problems and hints which were destined to lead research for a long time after.

12. Charles Bell.

The next great step in advance, which has revolutionised and permanently fixed our ideas on the action of the nervous system, was taken about the year 1810 by Charles Bell, who discovered the anatomical difference between the anterior and posterior roots of the nerves of the spine, and also went a long way towards showing their different functions. The point as regards functions was established by means of experiments on living animals by Magendie, and independently by Johannes Müller.[1] Upon the combined labours of these three masters of anatomy and experimental physiology is based the distinction between sensory and motor nerves—namely, that the anterior nerves of the spine are employed to carry the nervous stimulus outward to the different organs (efferent or motor nerves), the posterior and better protected nerves serving to carry

[1] On the respective merits of Charles Bell, Magendie, and Johannes Müller, see the writings of Claude Bernard and Du Bois-Reymond, referred to *supra*, p. 384 of this volume.

the peripheral stimuli of the senses inward to the nervous centres (sensory or afferent nerves).

13. Müller's "specific energies."

About the same time Johannes Müller, under the influence of Goethe's observations on the subjective colour-sensations and of Kant's doctrine of the innate forms of perception,[1] introduced another important distinction into the theory of the action of the sensory nervous apparatus. This doctrine is known by the name of the "specific energies." It has for a long time governed all physiological reasoning on the subject of our sense perceptions. In the words of Helmholtz, who more than any other has lent the great weight of his authority to an elucidation of this theory, "physiological experience has found that by the stimulus of any single sensible nerve-fibre, only such sensations can be pro-

[1] The doctrine of the "specific energies" of the sensory nerves, one of Joh. Müller's earliest speculations, which has governed a large section of psycho-physical research, at least in Germany, has grown out of the philosophical discussions in the 'Kritik der reinen Vernunft,' and the æsthetic treatment in Goethe's 'Farbenlehre,' both of which deal with the subjective element in our sense-perceptions. In this regard the reform of physiology in Germany contrasts with the contemporaneous reform by Magendie in France, whose extreme experimentalism Müller even ridiculed. See on the historical origin of Müller's psycho-physics, Du Bois-Reymond's excellent "Eloge of Müller" ('Reden,' vol. ii. p. 159), also Helmholtz's lecture on "Goethe's Naturwissenschaftliche Arbeiten" ('Vorträge und Reden,' vol. i. No. 1, 1853), and his address before the Goethe Society in 1892. Helmholtz finds the cause which misled Goethe in his optical experiments to be the same which misled Brewster—viz., the difficulty of obtaining really pure homogeneous light of any special tint. He worked with impure light and dull media. Helmholtz experienced great difficulties in obtaining the necessary purity in his own labours. Goethe, however, was not alone in studying with predilection the subjective colour-sensations. Du Bois-Reymond mentions Erasmus and Robert W. Darwin in England, and Purkinje in Germany, as working in the same field (*loc. cit.*, p. 160). Müller's work is contained principally in the treatise, 'Zur vergleichenden Physiologie des Gesichtsinnes des Menschen und der Thiere nebst einem Versuche über die Bewegungen der Augen und über den menschlichen Blick' (1826), and in his larger work on Physiology. See also on Goethe's merits Helmholtz, 'Physiologische Optik,' p. 249.

duced as belong to the qualitative—or order—region of one definite sense, and that every stimulus which can at all affect this nerve fibre produces only sensations belonging to this definite order."[1] This means that, for instance, any effective stimulus of the optic nerve apparatus produces only and always the sensation of light, whereas the same stimulus would in the auditory nerve apparatus, if effective, produce the sensation of sound. "The same vibrations of the ether which the eye perceives as light, the nerves of the skin perceive as heat. The same vibrations of air which the latter perceive as a tremor, the ear perceives as a musical sound."[2] The quality of our sensations does not depend on the stimulus but on the nervous apparatus.

Helmholtz has said[3] that the law of the specific energies forms the most important advance which the physiology of the senses has made in recent times, and has even compared it with the discovery of the law of gravitation.[4] As we shall see immediately, he has him-

[1] See Helmholtz, 'Handbuch der Physiologischen Optik,' 2te Aufl., 1896, p. 233.

[2] Helmholtz, 'Vorträge und Reden,' vol. ii. p. 224; also 'Physiologische Optik,' p. 249: "Müller's law of the specific energies marks an advance of the greatest importance, for the entire doctrine of the sense-perceptions has since become the scientific foundation of this doctrine, and is, in a certain sense, the empirical exposition of the theoretical discussion of Kant on the nature of the intellectual process of the human mind." Cf. also p. 584.

[3] 'Vorträge und Reden,' vol. i. p. 378; vol. ii. p. 181.

[4] This excessive appreciation of Müller's theory is, however, very much limited to Germany, and there also almost entirely to what may be called Müller's school, in which Helmholtz is the central figure. In England the doctrine was subjected to a full criticism by George Henry Lewes, an important thinker, whose writings contain many original views, which have in some instances since been independently put forward by other authorities. See his 'Physiology of Common Life' (1860, chap. 8); 'Problems of Life and Mind' (vol. i. p. 135, 1874); 'Revue Philosophique' (Paris, 1876, No. 2); 'The Physical Basis of Mind' (1877, p. 184). Without knowing of Lewes's criticisms, Prof. Wundt was led to a criticism of the doctrine from the physiological side in the first

self made a very important application of it, by bringing it into connection with Young's colour theory. But before I refer to this, it will be well to note the different lines of research which were opened out by Müller's formula, and how they have led in many ways to very fruitful expansion of natural knowledge. In this respect it is indeed permissible to compare Müller's formula with that of gravitation, which, as we saw above, through the different ideas which it introduced, helped to guide research for fully a century. Müller in the original statement of his views had made use of the term "specific energy," and had applied this term to the process or sense of sight: he spoke of the seeing substance or apparatus of sight. Now this apparatus is a complicated one, consisting mainly of three parts—the external or

edition of his great work on Physiological Psychology in 1872. See the note on p. 332, vol. i., of the 4th German edition (1893). Wundt says (p. 331): "Historically, the doctrine . . . is to be traced to the fact that the philosophical foundation of modern science, and especially of the science of sensation, rests on Kant. In fact, that doctrine is nothing else than a physiological reflexion of Kant's attempt to find the conditions of knowledge which are given *a priori*, or, what was mostly considered to be the same, subjectively. This is very evident in the case of a foremost representative of that doctrine — viz., Johannes Müller." In opposition to Müller and his school, Lewes and Wundt put forward a view which has been termed the doctrine of indifference of the function of the nervous elements. The difference between the two views is very clearly stated in an excellent paper by E. Montgomery in the fifth volume of 'Mind' (1880): "According to the doctrine of functional indifference, the various qualities—*i.e.*, our well-known sensations—are merely due to differences in the stimulating rhythm, to differences, therefore, of motion communicated from outside to the chemically uniform nerve-substance, and the whole complex make-up of our consciousness is, consequently, thought to result from the coexistence and subsequent combination of such stimulated motions. According to the doctrine of specific energies, the varieties of sensation are due to pre-existing differences in the substratum in which they respectively arise, and all their manifold combinations to higher products are believed to be realised in materially higher — *i.e*, specifically pre-endowed — ranges of nervous substratum" (p. 4).

terminal organ, the connecting fibre or nerve, and the central or percipient organ situated somewhere in the brain. How are these different parts of the combined apparatus anatomically constituted, and what are their respective physiological functions—in particular, where does the specific energy reside? The answer to these questions as regards not only the process of seeing, but likewise that going on in other sense organs, involved an enormous amount of detailed anatomical and physiological, analysing and experimenting work. With this work many great names are connected — first of all,
Helmholtz, who in his two great treatises on 'Physio- 14. Helmholtz.
logical Optics' and 'Physiological Acoustics,'[1] has laid the foundation of those two psycho-physical sciences which bring us nearest to an understanding of the interaction of mind and body. Like Young before him, for whom he expresses the greatest admiration, Helmholtz had approached the study of nature from the side of medicine: from this he was, by the peculiarity of his genius, driven to mathematico-physical studies on the one side, to psychological on the other. The exact methods of the mathematical, the experimental methods of the medical sciences; the mental analysis of Kant and Fichte, as well as the logical methods of J. S. Mill, were equally familiar to him. Inventions of his own, like that of the eye-mirror, or of others, like

[1] 'Die Lehre von den Tonempfindungen; Physiologische Grundlage für die Theorie der Musik,' 1st ed., 1863. 'Handbuch der physiologischen Optik,' 1867, 2nd ed., much enlarged. A succinct and very lucid exposition of the principal contents of those two great treatises, by an authority in the same domain of science, will be found in chaps. x. to xii. of Prof. J. G. M'Kendrick's volume in the "Masters of Medicine" Series on H. von Helmholtz, 1899.

the stereoscope of Wheatstone; pathological cases, like those of colour-blindness; a host of ingeniously devised experiments, as well as the gift of an exceptionally musical ear,—all these factors, and innumerable others, contributed to the production of these two monumental works, which form an epoch in the history of science as well as of philosophy and psychology. They form the first magnificent examples of the comprehensive application of exact methods to phenomena which had before been treated only fragmentarily, and where the influences of taste, fancy, and belief, the vagueness of metaphysics and the difficulties of nomenclature, had created a confusion which to many must have appeared hopeless. This confusion of language and of terms, of objective observations and subjective fancies, of the data of experience and the prejudices of theory, Helmholtz has done more than any other thinker to unravel.

In his two great treatises on the psycho-physics of the Eye and the Ear, of Vision and of Music, he has drawn two elaborate and detailed charts, which for a long time to come will have to be consulted by those who, in the interests of physics, philosophy, or æsthetics, enter into these mysterious domains. Many celebrated theories or definite aspects and lines of reasoning invented by others, his forerunners or contemporaries, were adopted, but mostly with important modifications. It may be of use to enumerate briefly the principal ones, beginning with the most mathematical and exact and ending with the more general and metaphysical. In the beginning of the century Fourier had shown how any forces of motion in

two dimensions—however complicated or irregular that motion might appear to be—could be mathematically represented or calculated by the superposition or addition of a larger or smaller number of simple periodic motions; as it were analysed and dissected into these simple movements, just as any number can be looked upon as made up by the addition of others—say of prime numbers. Now, it was also known that sounds were produced by wave-like tremors of the air set going by the vibrations of strings or other sounding musical instruments; further, that definite musical notes were absorbed or transmitted by neighbouring sounding bodies according as these were in or out of tune with the vibrating source of sound. This is the well-known phenomenon of resonance. Ohm[1] had applied Fourier's mathematical analysis to the explanation of the partial notes, the ground tone and the harmonic overtones (or upper partial tones), of which musical[2] sounds are made up. Helmholtz invented a

[1] Geo. S. Ohm, the same to whom we are indebted for the well-known law which obtains in electric currents, published in 1843 a paper in Poggendorf's 'Annalen' (reprinted in 'Gesammelte Abhandlungen,' 1892, p. 575), "On the definition of a tone and the theory of the siren," in which he applied the mathematical methods introduced by Fourier in his 'Théorie analytique de la Chaleur' (1822); as he had already done in his earlier work on the galvanic current (1827). In fact, Ohm was one of the first to recognise the value of Fourier's conceptions in contradistinction from Laplace's, which were bound up with certain hypothetical notions as to the molecular constitution of bodies. See the introduction to his treatise on the galvanic current ('Ges. Werke,' p. 63).

[2] Cagniard de la Tour had invented (1819) and Seebeck the younger had improved (1841) the first mechanical counter for the frequencies of musical sounds, the siren; and the latter as well as Duhamel had studied the composition of such sounds out of their elements or simple notes. A suggestion had been thrown out as to the part played by the upper partial tones which accompanied the ground tone. Helmholtz treats first of this subject in a lecture (1857), reprinted in 'Vorträge und Reden,' vol. i. p. 79, dealing with "the psychological causes of musical harmony."

series of simple but ingenious apparatus by which these partial notes could be analysed, isolated, and made specially audible, or by which the ground tone could be purified, and thus led up to his conception of the human ear—the different parts of which he analysed anatomically and acoustically—as a most delicate resonator which separately absorbed the different elementary periodic movements that constitute musical sounds, the different nerve-fibres carrying them separately to the central organ of perception.[1] On the bases of these distinctions, Helmholtz succeeded in giving an accurate definition[2] of that property of musical notes termed "timbre" by the French, "Klangfarbe" by the Germans—that peculiar colouring or texture which characterises the same note[3] if produced by different instruments. He

15. "Timbre" defined.

[1] See 'Die Lehre von den Tonempfindungen,' 1st ed., 1863, pp. 92, 95, 97. "The main result of our description of the ear can be thus stated, that we have found that everywhere the ends of the auditory nerve are connected with special auxiliary apparatus, partly elastic, partly solid, which under the influence of external vibrations are made to vibrate correspondingly and then probably affect and agitate the nerve-substance" (p. 212).

[2] Helmholtz was the first to give a positive definition of "timbre." As he himself says (p. 114), before him it meant all the peculiarities of a musical sound which are not defined by its intensity or its position in the scale—*i.e.*, its "pitch." Of these he eliminates all such as are connected with the beginning, rising, and dying away of sounds, and deals only with sounds which are uniformly maintained (p. 116).

[3] The terminology of acoustics and of music has been considerably changed, especially in this country, through scientific literature, in which the work of Helmholtz forms a kind of epoch. According to Lord Rayleigh ('Sound,' vol. i. § 22, 1st ed.), the word "tone" in the English language has been adopted by Tyndall to denote a musical sound which cannot be further resolved. The word was used before, but in a general sense, not limited only to sounds, and where now "tone" is used in works on acoustics, the word "note" was more usually employed. Sir John Herschel ('Encyclop. Metrop.,' article "Sound," 1845) does not consistently use the word "tone" as an equivalent for the German "Ton," but makes use of "sound" or "note" or "tone" promiscuously. Still more uncertain was the terminology by which to express the quality of a musical sound other than loudness and pitch, and, to the present day, the English tongue has no equivalent for the French "timbre" or the German "Klangfarbe." Everett used the word character, and so does Lord Rayleigh. Dr Young, in his "Essay on Music" (1800, 'Miscell. Works,' vol. i. No. 5), speaks of the quality of sound, sometimes called its tone, register, colour, or timbre (p. 118). In the most recent scientific work on sound in the English language (Poynting and Thomson's 'Text-Book of Physics,' Sound, p. 69) we read, "It is convenient to use the term note for an ordinary compound sound to which a definite pitch may be assigned, and the term tone for each simple harmonic constituent which goes to form it." There is an important note on the terminology by Alex. T. Ellis, the learned translator of Helmholtz's 'Sensations of Tone' (1875, p. 36).

entered into an analysis of the processes by which vocal sounds and notes are produced, and showed their importance in musical and linguistic theories. Combined with all these deductions and applications, which started from Fourier's mathematical analysis of compound movements, Helmholtz's anatomical dissection of the organ of hearing leads him to the conclusion that there "must exist in the ear different parts which are set in vibration by notes of different pitch, and which have thus a sensation of these notes."[1] And here he takes up a different line of reasoning—that suggested by Johannes Müller's theory of the specific sense energies. In his studies in physiological optics he had already accepted Young's hypothesis that there exist in the eye three distinct kinds of nerve-fibres, to which belong distinct modes of colour-sensation. Something analogous exists in the ear.[2] The differences in notes — namely, pitch and colour [or character]—are reduced to differences of the sensitive nerve-fibres, and for each nerve-fibre there exists only the difference of the intensity of the stimulus."

16. Analogy between sound and colour.

This brings the action of the sensory nerves into line with that of motor nerves: everywhere the nerve itself is

[1] 'Tonempfindungen,' p. 215.

[2] Ibid., pp. 220, 221.

indifferent to the stimulus, which it carries in or out like a telegraph wire; which, whilst acting in every case in the same way, may, according to its terminal connection,[1] "deliver messages, ring a bell, explode a mine, decompose water, create or move magnets, produce light, &c. The same with the nerves. The state of irritation is, so far as the isolated nerve-fibre is concerned, everywhere the same, but in accordance with the nature of different parts, be it of the brain or of the external portions of the body, it produces motion, secretion, increase or decrease of blood, of heat in different organs, or lastly, sensations of light, sound," &c.

The physiology of hearing had its brilliant application in a clearer understanding of the elements of language, of the formation of the vowel sounds, and in the study of the development of music—that art which, more than any other, seems founded on definite rules.[2] In analysing

[1] 'Tonempfindungen,' p. 222.

[2] "From the time when Pythagoras is said to have discovered the arrangement of tones in an octave, by observing that the sounds of the blacksmith's hammer in the forge produce a fourth, a fifth, and an octave, and was then led to obtain harmonic proportion between the strings of the heptachord, all who investigate musical tones know that, although these are fleeting sensations, they depend physically on numerical relations between various kinds of movements; but it was Helmholtz, more than any other philosopher, who examined the whole range of the phenomena, physical as well as physiological, and whose work will for generations remain an enduring monument to his genius" (Prof. M'Kendrick in the Helmholtz volume of the "Masters of Medicine" Series, p. 168).

Since the appearance of the last edition of Helmholtz's great work, of which there exists an excellent English edition with valuable notes, many of the points first investigated by Helmholtz have been taken up by other experimentalists as well as by psychologists. The invention of the phonograph by Edison in 1877 gave a great impetus to exact research in the problems of audition, and various facts and theories have been advanced confirming or modifying the views put forward by Helmholtz. On these see the last chapter of Lord Rayleigh's 'Treatise on Sound,' 2nd ed., 1894. On the psychological side see the 2nd volume of Prof. Wundt's 'Physiologische Psychologie,' pp. 47-96.

these Helmholtz is led into æsthetical and psychological discussions, clearly distinguishing between such principles as are inherent in natural, physical, and physiological relations, and such others as depend on the inventions of genius and the gradual changes brought about by external requirements and ingrained by habit and education.[1]

The physiology of seeing had yet more remarkable consequences for the history of Thought. We may say that through Helmholtz's analysis of the formation of our space perceptions by the eye in connection with the tactile and muscular senses, psychology and metaphysics were brought into immediate contact with physics and physiology. It is here that Helmholtz takes up an 17. Helmholtz and Kant.
entirely different, and, previously, isolated line of reasoning, which centres in Kant's theory of space and time as innate forms of perception—the so-called subjectivity or ideality of time and space. The studies of this subject had been somewhat prepared by the writings of Herbart and Lotze. The teachings of Kant have had an influence in the direction indicated through two distinct channels, —through Johannes Müller's Physiology and through Herbart's Psychology: the latter seems to have had

[1] See the closing words of the 13th chapter of Helmholtz's work: "As the fundamental principle for the development of the European tonal system, we shall assume that the whole mass of tones and the connection of harmonics must stand in a close and always distinctly perceptible relationship to some arbitrarily selected tonic, and that the mass of tone which forms the whole composition must be developed from this tonic, and must finally return to it. The ancient world developed this principle in homophonic music, the modern world in harmonic music. But it is evident that this is merely an æsthetical principle, not a natural law. The correctness of this principle cannot be established *a priori*. It must be tested by its results. The origin of such æsthetical principles should not be ascribed to a natural necessity. They are the inventions of genius, as we previously endeavoured to illustrate by a reference to the principles of architectural style."

little influence over the Berlin school of physiology, but it has had a considerable influence on several members of the Leipzig school. In this school Lotze was educated.

Locke had taught, and his followers had accepted, the doctrine that the so-called secondary qualities of sensible things, such as colour, sound, hardness, &c., were subjective. Speculative physics had prepared this view by translating such properties into special forms of aggregation or periodic motion, leaving only extension and resistance as the primary properties inherent in things. Kant had gone a step further, and maintained that space and time were likewise only subjective forms of our perceiving sense apparatus. Two problems grew out of this view, which are not clearly stated in Kant's writings. First, How does the perceiving mind arrive at the elaborate and systematic space conception which is peculiar to us human beings?—*i.e.*, out of what perceptive elements, and by what psychical processes, is it gradually built up? Secondly—What is it that locates our sensations at definite places in space? There is a third question which Kant put and answered, that referring to the nature and validity of the geometrical axioms. According to his view the axioms of geometry are innate, expressive of the inborn nature of our space conceptions; in fact, the truths of geometry formed in his view the only instance of knowledge gained not by experience but *a priori*—before or outside of experience.

18. The brothers Weber.

An entirely independent series of psycho-physical investigations was started even before Johannes Müller, by Ernst Heinrich Weber of Leipzig, who, with his two brothers, Wilhelm and Eduard, may be considered as

the centre of the Leipzig school of Anatomy, Physiology, and Physics.[1] After having been among the first to import the exact methods of research into physiology, and having carried on a variety of investigations referring to physiological optics and acoustics,[2] he approached the subjective phenomena of sensation: recording, for example, with what degree of accuracy different parts of the surface of the skin on face, arm, leg, &c., perceive the distance between two points which touch the skin — say the two points of a pair of compasses; recording also the relation of the smallest increase of any given sensation to the corresponding increase of stimulus. In the latter series of experiments, he arrived at what has been termed [3] Weber's Psychophysical law. He did not call it so himself; he simply showed by experiment that in a variety of cases the stimulus had to increase in proportion to its own initial intensity in order to produce a just perceptible increase of sensation. These experiments did not attract much attention till Gustav Theodor Fechner took them up, building upon them his celebrated "Principles of Psychophysics." Before referring more in detail to these, I must mention a third line of reasoning which, as stated above, had a considerable influence on the Leipzig school of Psycho-physics, though probably it had as little

19. Fechner's Psychophysics.

[1] On the labours of the brothers Weber, see the references given above, vol. i. p. 196, also the present volume, p. 31, note.

[2] E. H. Weber published in 1817, 'Anatomia comparata nervi sympathici;' in 1820, 'De aure et auditu hominis et animalium;' from 1827 onward, 'Annotationes anatomicæ et physiologicæ,' in which, in 1831, there appeared his celebrated treatise "Tastsinn und Gemeingefühl." Joh. Müller's 'Vergleichende Anatomie des Gesichtsinnes' appeared in 1826.

[3] By Fechner in his 'Elemente der Psychophysik' (2 vols., Leipzig, 1860).

influence on E. H. Weber as the earlier philosophy of nature, to which it formed a pronounced opposition.

20. Influence of Herbart. Herbart was not an experimental philosopher; nevertheless a place in a history of scientific Thought belongs to him. Indeed, his philosophy, like that of Kant, and, in quite a different way, of Schelling, has had a marked influence on many thinkers and men of science who have prepared the ground for an exact treatment of the phenomena of Life and Mind. Among exact psychologists I need only name Volkmann, Drobisch, Lotze, and in our time Professor Wundt [1] of Leipzig. It is therefore of interest to mark the precise point where Herbart's influence comes in.

Although an exact school of psychology might aim at studying psychical and psycho-physical phenomena without reference to any general theory of the soul as the supposed centre and substance of these phenomena, the existing ideas and theories as to soul and mind have nevertheless always played a great part in these researches, just as it has been found impossible to free biological research altogether from some theory of life. Older psychologists were consciously or unconsciously governed by the conception of a number of distinct mental faculties. Even Kant's philosophy is still embarrassed by this view, which reigned supreme in the teaching of his predecessor Wolf. The attempt of

[1] This is not the place to speak about the Herbartian school, which is almost entirely confined to Germany. I have referred to Prof. Wundt because, in spite of a running criticism, in the 'Physiologische Psychologie,' of Herbart's special doctrines, the author of that important and comprehensive work himself declares (Preface to the 1st ed., 1874) that for the formation of his own views he is, next to Kant, most indebted to Herbart.

Herbart, therefore, to overthrow the so-called faculty-psychology, and to insist on the essential unity and simplicity of the inner life, must have made a great impression on all who came under the influence of his philosophy. It did this in two ways.[1] It first of 21. His attack on the "faculty-psychology."

[1] Besides Herbart (1776-1841), whose psychological writings date from 1813 to 1825, another German psychologist is usually mentioned as having helped to overthrow the older faculty-psychology. Beneke (1798-1854), a younger contemporary of Herbart, conceived of psychology as a natural science. His principal work, 'Lehrbuch der Psychologie als Naturwissenschaft,' appeared in 1833, and has been several times republished, the fourth edition appearing in 1877. Beneke worked in opposition to Hegel at Berlin, his historical forerunners being the German philosophers, Jacobi, Fries, and Schleiermacher, as well as the English philosophy of the so-called Association-school. An account of his philosophy does not belong to a chapter on psycho-physics except in as much as he introduced into the study of the inner life not indeed the facts and data of physical —*i.e.*, physiological—science, but the physical method. He was the purest representative of the psychology of the "inner sense." Whilst Herbart based his psychology alike on experience, metaphysics, and mathematics, Beneke accepted only the first, and discarded the latter. Standing thus outside the all-powerful school of Hegel and the increasing influence of Herbart, Beneke had during his lifetime only a limited audience, and received due attention in a wider circle, first and principally through Ueberweg, who was greatly impressed by him. In fact, his influence was felt in Germany about the same time as that of the English and Scottish philosophers. Ueberweg, in his well-known 'History of Philosophy,' vol. ii. pp. 281-292 (Engl. transl. by Morris, 1874), gives a full account of Beneke. Prof. Erdmann gives a very full account also in his excellent 'Grundriss der Geschichte der Philosophie' (3te Aufl., 1878, vol. ii. pp. 628-641). The fact that Beneke's method is introspective, brings him not only into contact with the English school, but also with French thought, which has always been characterised by subtle psychological analysis. This explains the fact that M. Marion (in the 'Grande Encyclopédie') calls Beneke "un des principaux philosophes Allemands du siècle,"—a designation which would hardly be echoed either in Germany or in England. The best account of Beneke's position in the development of psychology extant in the English language is that of Dr G. F. Stout, in his article "Herbart compared with English Psychologists and with Beneke," in the 14th volume of the 1st series of 'Mind' (1889). M. Ribot, in his well-known book on 'Modern German Psychology' (Engl. transl. by Baldwin, 1899), does not say much about Beneke, but his account of Herbart and his school, and their position in psycho-physical thought, is concise and much to the point. Dr Stout's articles on Herbart in 'Mind' (vols. 13, 14) are also much to be recommended.

all liberated them from the trammels of an antiquated and misleading terminology; and secondly, it impressed them with the necessity of giving an answer to the question how the multiplicity of sensations or the flow of ideas was held together in the unity of an inner existence. Thus it is a characteristic of all psycho-physical writers who have come under the influence of Herbart, that however much they may be occupied with detailed description of physiological processes, with the analysis of sensations or the dissection of the data of experience, they never lose sight of the underlying mental unity which is the central phenomenon of psychology and of psycho-physics, just as it must be the central problem of biology to arrive at some definition of life. Had the investigations of psycho-physical phenomena remained where Weber or even Helmholtz left them, we should have brilliant chapters on the phenomena of touch, of seeing, hearing, and other processes where the outer and inner worlds come into contact, but no attempt to sum up these brilliant contributions in a connected view of the inner and higher life—the most remarkable and unique phenomenon in nature. It seems

22. Unity of mental life.

to me that, in Germany at least, it is through Herbart, more than through any other thinker, that we have been preserved from a threatening disintegration of psychological research. It is the more necessary to recognise this, as most of those writers who at one time came greatly under Herbart's influence have found it necessary, after having become thoroughly saturated with this one great truth in his philosophy, to abandon almost the whole of the more detailed expositions contained in his works.[1] Herbart was quite as correct in his ideal of what psychology should be, as he was unfortunate in the particular manner in which he elaborated it.

Psychology was to be founded on experience, metaphysics, and mathematics. Kant had studied the inner activity of the mind as it is compounded of sensation, perception, and apperception; of understanding, judgment, and reasoning. In opposition to this Herbart went back to the position taken up by Locke and Hume, looking at the inner life of a conscious mental being or soul, not as a complex of mental faculties, but as a flow of ideas or perceptions. How is the unity and simplicity of this mental being preserved in the midst of this continuous flow of ideas? how is it regained as often as it is in danger of being lost? His investigations start at the point where the inquiries of the association school of psychologists started in England. Having, however, the mechanics and dynamics of physical forces more promin-

[1] Dr Stout has given an account of the Herbartian school in the 14th volume of 'Mind,' p. 353 *sqq.* He confines himself to Drobisch, Waitz, and Volkmann, the psychologists proper. M. Ribot (*loc. cit.*) has dwelt more on the development of the Herbartian school in the direction of anthropology and ethnology; he mentions specially Waitz, as well as Lazarus and Steinthal. He contrasts their work and their positions with those of the great anthropologists of the English school, such as Tylor, Lubbock, and Herbert Spencer, and notes, in the German school, the absence of Darwinian ideas. It is important to observe that both in the case of Prof. Wundt of Leipzig and of Mr Spencer in England—that is, in the case of the latest outcome of the Kant-Herbartian philosophy on the one side and of the Association philosophy in England on the other—and in each case under the influence of the exact and biological sciences, philosophy ends in elaborate treatises on Anthropology, which with Spencer is conceived under the name of Sociology. Similarly, the school of Hegel ended in elaborate historical treatises. Hume turned from abstract philosophy to political economy and history, and Herder—as we shall see later on—anticipated much of all this movement in his History of Mankind.

ently before his mind than they had, he was tempted to try how far the conceptions of equilibrium of motion and of the composition of forces could be applied to the inner play of ideas which chase, oppose, and displace each other, preserving all the time a kind of dynamical equilibrium. His elaborate mathematical calculations in the first part of his greater work on psychology do not specially refer to the purely intellectual process;[1] they refer rather to all inner processes which oppose each other, which come into conflict, restraining each other in proportion to their contrast, creating a tendency towards reversion to former conditions. Such a play of opposing forces is to be found likewise in the larger field of human society; this is accordingly quite as much a case for the application of those psychical mechanics which Herbart aimed at establishing.

23. Mathematical psychology.

In a history of scientific Thought, which aims at showing by what gradual steps the various provinces of phenomena have been brought under the methods of exact treatment, the psychology of Herbart has an important as well as a unique and isolated position. It

[1] Herbart himself says of his mathematical chapter, that the results therein given "do not follow immediately from the conception of a thinking being; but they refer to the mutual arrangements of any things, in so far as they are opposed and as they collide, restricting each other in proportion to their contrast, tending to revert to the previous condition, the unrestricted portions being fused into complex forces. The forces which are active in society are doubtless originally psychological forces. They meet in so far as they appear in language and in actions in a common sensual world. In the latter they restrict each other; this is the universal spectacle of conflicting interests and social frictions. Also the fusion no doubt exists. . . . We therefore assume that among men living together the same conditions appear which exist, according to our view, among the ideas in one and the same consciousness. We examine the result of their mutual restrictive action" ("Psychologie als Wissenschaft," 'Werke,' ed. Hartenstein, vol. vi. p. 31, &c.

led psychologists to consider more closely the conditions under which a mathematical treatment is at all possible, and to recognise that exact and accurate measurements must precede all application of an abstract calculus. Herbart's ideal was that of a psychical mechanics; he opposed[1] the idea of a union of physiology and psychology. And yet this was just the direction in

[1] In a very interesting note at the end of the introduction to the second part of his larger work on psychology, Herbart explains his position with regard to physiological psychology. It refers to certain extracts which he makes from Rudolph's 'Grundriss der Physiologie,' in which that eminent physiologist referred to Herbart's 'Lehrbuch der Psychologie.' "It is not only a metaphysical but also a logical error to confound psychological and physiological research. Psychological phenomena are not in space, but space itself, with all that appears in it, is a psychological phenomenon, and, indeed, one of the first and most difficult facts for psychology, which, in the treatment of it, would behave very improperly if it began by discussing the forces in the nerves; for the question is not, where sensations come from, but how sensations acquire the form of space. Now, I maintain further, that the difference between lifeless and living matter—that is, between physics and physiology—cannot be understood until we know mind by means of psychology, for in all the countless elements of the organised body—in plants as well as in animals—there is an analogue of mental development which cannot possibly be found on the surface of phenomena. We observe internally a fragment of *our own* mental existence. This fragment is developed into scientific knowledge through speculative psychology based on metaphysics. This knowledge meets another equally metaphysical science, natural philosophy, with its conception of matter—that is, of such matter as we know through chemistry and dynamics. Then only can the question be put, how such matter must be constituted, so that its separate elements are determined, not only through their original quality, but also through a development analogous to the mental one," &c. The section closes with the following characteristic passage: "Those who favour empiricism can learn from the present state of physiology how much, or rather how little, mere experience can do. Physiology, as an empirical doctrine, has attained a height which nobody can despise. Moreover, it proceeds in the light of modern physics. Nevertheless, it has eagerly sucked up, as the sponge sucks up water, that philosophy of nature which knows nothing, because it began by construing the universe *a priori*. Towards this error no science has proved so weak, so little capable of resistance, as physiology. The talk about life has become the Dead Sea in which all spirit of philosophical research is drowned, so that, if a resurrection is at all to be hoped for, it must be born anew in quite unbiassed minds" ('Werke,' vol. vi. p. 65, &c.)

which an exact or scientific treatment of mental phenomena could meet with any success at all. It was in the schools of physiology, in those of Johannes Müller and of Weber, that philosophers had to learn how to attack the borderland of bodily and mental phenomena.

24. Lotze's Physiology of the soul.

The first who approached the subject from this point of view was Hermann Lotze. He was a disciple of E. H. Weber, and had been led to psychological researches from two independent starting-points: first from the study of the medical sciences which, under the hands of his great master, had largely benefited by the application of the exact methods of the physical, the measuring, and calculating sciences, but also from an entirely opposite quarter.[1] "A lively interest in poetry and art had led him to philosophy." He was attracted by that great body of ideas which, through the systems of Fichte, Schelling, and Hegel, had become permanently domiciled in German culture. In this great realm he could move "with some freedom," for it had not become crystallised into a definite system of doctrine; exact studies had, moreover, easily convinced him "how absolutely untenable was the form into which Hegel had cast that valuable possession."

[1] The quotations in the text are taken from Lotze's polemical pamphlet, 'Streitschriften' (Leipzig, 1857), pp. 6, 7. As already mentioned (*supra*, p. 407 note), Lotze had been misunderstood by his critics, of whom some represented him as a materialist, others as a follower of Herbart. In refuting the latter charge he explains his position towards the idealistic systems of the first half of the nineteenth century. He acknowledges two great personal influences, that of C. H. Weisse, which, as it were, touches the kernel of his convictions, and that of the study of medicine, which, in his case, was intimately connected with that of the physical sciences. He admits, as did Herbart, having passed through the magnificent portal of Leibniz's Monadology to a general arrangement of his philosophical opinions.

We must bear in mind this twofold source of Lotze's reflections if we want to estimate correctly the value of his early criticisms regarding the then prevalent treatment of such questions as life and mind in the medical sciences. On the one side he had the object of clearing the way for purely mechanical explanations. We learnt in an earlier chapter how he was one of those who successfully chased out of biology the vague idea of a vital force. And when he approached the problem of mind and body, we find him insisting on the presence of a psycho-physical mechanism which rules[1] the inter-

[1] The opinion of Lotze regarding the relation of soul and body, or rather of psychical and physical phenomena, has been stated by him, variously, as parallelism, occasionalism, pre-established harmony, and was ultimately crystallised in the term psycho-physical mechanism. The question is fully discussed in the articles, "Leben und Lebenskraft," "Instinct," "Seele und Seelenleben," which he contributed to R. Wagner's 'Handwörterbuch der Physiologie.' They are reprinted in Lotze's 'Kleine Schriften,' ed. D. Peipers, 4 vols. (Leipzig, 1885-91). He there says, "The conception of a psycho-physical mechanism can be stated as follows: As ideas, volitions, and other mental states cannot be compared with the quantitative and special properties of matter, but as, nevertheless, the latter seem to follow upon the former, it is evident that two essentially different, totally disparate, series of processes, one bodily and one mental, run parallel to each other. In the intensive quality of a mental process, the extensive definiteness of the material process can never be found; but if the one is to call forth the other, the proportionality between them must be secured through a connection which appears to be extrinsic to both. There must exist general laws, which ensure that with a modification *a* of the mental substance a modification *b* of the bodily substance shall be connected, and it is only in consequence of this independent rule, and not through its own power or impulse, that a change in the soul produces a corresponding one in the body" (vol. i. p. 193). Lotze destroyed the idea of vital force, but he only chased the conception of the soul beyond the limit of the psycho-physical mechanism, and he maintains that natural and medical science have no interest in pursuing the question beyond that limit, "however interesting the further discussion of this subject may be to speculative psychology" (vol. i. p. 197)—"for it is quite indifferent to medicine, wherein the mysterious union of body and soul consists, as this is the constant event which lies equally at the bottom of all phenomena. But it is of the greatest interest to medicine to know what affections of the soul are connected in that mysterious

action of external and internal phenomena, of stimulus and sensation.

25. Two sides of Lotze's doctrine.

There existed indeed another side—that which we may call the philosophical; it does not at present enter into the course of our narrative, which deals only with the extension of scientific or exact thought, and with mental phenomena and the inner life in so far as they form a province—perhaps a very restricted province—of the whole of nature. This province Lotze was among the first to proclaim distinctly to be one which natural science had to conquer and to cultivate. He is careful to explain that it does not cover the whole ground of psychology, and at the end of his long discourse on the "soul and its life," which formed an important contribution to the great physiological encyclopædia published in the middle of the century, he clearly marks out "physiology of the soul as an exposition of the physical and mechanical conditions to which, according to our observation, the life of the soul is attached,"[1] as one of the several problems of psychology. It formed a counterpart to the physiology of the body, of the physical side of our existence, and was, like it, to become a natural —*i.e.*, a mechanical—science. Subsequently he collected the whole of his reflections belonging to these two departments in two treatises on the 'General Physiology of Bodily Life' (1851), and on 'Medical Psychology' or 'The Physiology of the Soul' (1852).

As little as it now enters into our programme to

manner with what affections of the body. Unfortunately, medical science has only too often lost sight of this its proper problem over fruitless speculations referring to that connection itself" (p. 197). Cf. also 'Medicinische Psychologie,' p. 78.

[1] 'Kleine Werke,' vol. ii. p. 204.

follow up the philosophical reasonings of Lotze beyond the limit of the psycho-physical mechanism, so little were these at the time of their appearance heeded by many of his readers, some of whom he seems to have converted to or confirmed in a purely materialistic conception of the phenomena of the inner or mental world. Lotze had banished "vital forces" from biology; why not follow him, and banish all other higher principles, and revive—as Carl Vogt did[1]—the dictum of Cabanis about the analogy between the functions of the brain and the kidneys? Why should the "anima" of Stahl not have the same fate as the "vital force" of Bordeu and Bichat?

This was a misconception of what Lotze had intended. He had, indeed, banished[2] the principle of life as a factor useless in physiological explanations; but not the principle of organisation, which must have presided over the beginning of all organic forms. This might be neglected by physiologists, who had nothing to do with origins but only with existing relations. It was quite different with mental phenomena, which, manifesting themselves alongside of physical processes, required to be dealt with and recognised as actually existing and concurrent events.[3] Herbart's psychical mechanism might

[1] On this, see the account given in Lange's 'History of Materialism' (Engl. transl., vol. ii. p. 285) and Lotze's reference to it in 'Med. Psychol.,' p. 43.

[2] "There is no doubt that a legitimate attack upon 'vital force' has marked in our days that line of reasoning, which has by the law of inertia carried many of our contemporaries far beyond the correct limit on to a negation of the existence of a soul" (ibid., p. 41).

[3] These various points are very fully discussed in Lotze's earliest philosophical work, 'Metaphysik' (Leipzig, 1841), pp. 251, 255, 259; and again in the 'Med. Psychologie' (1852), p. 78. Referring to the last chapter, in which I dealt with the development of the theories of life and organisation, two points

be an unrealisable ideal in that it dealt with inner phenomena as unconnected with outer ones: a psycho-physical mechanism was a nearer approach to a true description of reality, and could not be narrowed down to a purely physical occurrence; moreover, the unity of mental life was a special property which had to be recognised and defined.

26. The psycho-physics of vision.

Lotze himself, after formulating the conception of a psycho-physical mechanism, and utilising the elaborate and fundamental experiments and observations of Weber as illustrations of what was meant, made an important contribution towards an analysis of a compound physico-psychical process. He took up the problem which Berkeley had attacked, of the formation of our space perception. It had been introduced into German psychology mainly through Herbart with reference to the Kantian doctrine that space is a subjective form. Through Lotze, and subsequently through Helmholtz, it has been shown to have not only a psychological but likewise a physiological importance: it is a problem of psycho-physics.

There exists a peculiar difficulty in bringing home to the popular mind the fact that a special problem is involved in the manner in which our senses of sight and touch combine and arrange simple sensations into the whole of a well-ordered perception of space; for we do not become able to appreciate the fact of the slow and gradual growth of this perception, which takes place in the early days of our infancy, till long after we have actually gained full possession of it. Something similar exists with regard to language and thought: we only hear of grammar and logic long after the main difficulties of speech and thinking have been unconsciously mastered, and if it were not for the existence of other languages than our own, and of an erroneous logic as exemplified in errors of calculation and of measurement, it is doubtful whether grammar and logic would have been so early developed. As it is, the physiological problem of the formation of our space perception was actually first forced upon naturalists by the observation of pathological cases, such as the acquisition of sight in later life through couching, the existence of colour blindness, and a variety of optical delusions which still serve as indispensable test cases for the various theories that have been propounded. Only when something turns out to be palpably wrong do we begin to inquire what constitutes the right side of many things.

Thus the cases of Cheselden and Wardrop and the colour blindness of Dalton set physiologists thinking about the genesis of our space and colour perceptions. A very great impetus—perhaps the most valuable of all —was given by Wheatstone's invention of the stereo- 27. Wheatstone's stereoscope.
scope in 1838; an instrument which, as it were through

may be noted. First, it is clear that Lotze was an "organicist" before Claude Bernard and other more recent thinkers mentioned above. Secondly, it is very evident that Lotze belongs to the pre-Darwinian school of thought. In fact, he does not relish the genetic aspect. The historical beginnings of ideas are for him no indication of their value and correctness. He says on this point: "The genesis of a conception is no argument for its validity; in the ever indistinct manner in which language operates in forming its words, it may form the correctest conceptions in just as incorrect a manner as the most erroneous ones. What is important is whether the conception, formed anyhow, can justify itself" ('Med. Psychol.,' p. 41). I shall on another occasion have to refer more fully to this marked absence of the historical sense in Lotze.

a kind of deception, gave to perfectly flat surfaces the vivid appearance of depth and distance. And here we may note, in passing, how it was almost entirely left to foreign thinkers to utilise this remarkable invention for the benefit of the theory of vision and the science of psycho-physics;[1] Whewell having characteristically omitted this epoch-making fact, as in his well-known history he omitted to notice many other contemporary British contributions to science.

Philosophers, who are accustomed to find hidden problems where ordinary persons only see common-sense, had already approached the question of the genesis of our space perception from two definite points of view, which we may, for the sake of convenience, identify with the names of Kant and Herbart. The genetic view associated by the physiologists with the name of Kant, and supposed to have been prepared by Locke, Berkeley, and Hume, was this, that what we know of external things depends upon the peculiarities of our own perceiving

[1] Sir Charles Wheatstone (1802-1875), to whom several inventions of equal scientific and practical interest are due, invented the mirror-stereoscope in 1833. A notice of it was given in Mayo's 'Outlines of Human Physiology,' but neither its theoretical nor its practical importance was recognised till Wheatstone published his paper in the 'Phil. Trans.' in 1838. He there refers to Leonardo da Vinci as having been the only one before him to notice the difference of binocular and monocular vision. Since Wheatstone's invention became known and was perfected by Brewster, Moser, and others, and especially since Helmholtz entered the field with his extensive and original researches in optics, it has been found that ancient as well as more recent philosophers had approached the subject very closely; and many references are given in the new edition of the 'Physiologische Optik' (1896), p. 840. The invention of photography about the same time (1835, by Daguerre, after extensive and prolonged experiments by himself and Niepce, published in 1839 by Arago), which was of great importance to optical theory, was also for some time singularly little appreciated by theorists. See Rosenberger, 'Gesch. d. Physik,' vol. iii. p. 316. See also Helmholtz's lecture "Ueber das Sehen des Menschen" (1855).

and thinking self, on sensations, and on their arrangement or orderly presentation. The sensations themselves are the substance, the spatial arrangement of them the form, of our perception of external things. The question was gradually put more and more clearly, How we come to localise certain of our sensations at definite places in the totality of a spatial arrangement? Herbart added another important reflection, which really dated from Leibniz. Impressed with the unity of all mental existence, and claiming this as the characteristic property of our inner life, he asked the question, How can the oneness or simplicity of this inner existence, as it were, expand itself without losing its unity, into the orderly variety of a spatial contemplation? For the purpose of an answer to this question he fixed on the phenomenon of motion. The conception of an orderly arrangement of sensations or things in space is gained in great measure by the aid of definite movements of the sensitive organs, which are accompanied by definite sensations of motion — *e.g.*, by muscular sensations.

28. Localisation of sensations.

The first of these two questions may be expressed in the words, Given the subjective form of a space perception, either complete in its geometrical arrangement (the nativistic hypothesis) or gradually acquired in the early moments of our conscious life (the empiric hypothesis), how do we make ourselves familiar with, and at home in, this form of perception? And secondly, By what special properties or local signs do we localise or place each single sensation in its right and orderly position? The first is the problem of space construction, the second

that of localisation of things in space. Lotze was one of the first to attempt detailed answers to these questions.
29. Lotze's "local signs." In particular he propounded the theory of "local signs," which with certain modifications has been adopted by subsequent writers on the subject. The combination of physiological, optical, and psychological investigations in Helmholtz's great work on 'Physiological Optics' has brought definiteness and mathematical precision into many of the questions suggested by philosophers and naturalists before him. Through it and its great companion, the 'Physiological Acoustics,' psycho-physics has to a large extent become an exact science.

A great step in the direction of drawing psychical phenomena into the circle of the exact sciences was taken
30. Fechner. independently by Gustav Theodor Fechner;[1] in fact, it is

[1] G. T. Fechner (1801-1887) was a unique figure in German literature, science, and philosophy. Beyond his own country he is only very imperfectly known and appreciated. He was self-taught, and living ali his life somewhat outside the conventional categories of German academic activity, he made a position for himself which has only become intelligible to a larger public through the issue—after his death—of Prof. Wundt's oration, Prof. Kuntze's (his nephew's) charming biography (1892), and Prof. Lasswitz's monograph on Fechner (Stuttgart, 1896), in which for the first time a coherent exposition of his philosophical teaching is attempted. Prof. Wundt has also, in many passages of his work on psychology, and through the second edition of the 'Psychophysik,' contributed largely to a better understanding of Fechner's views and merits. He descended on both sides from ancestors whose position was that of highly esteemed Protestant pastors; he studied medicine like Lotze, and was the friend and colleague of Lotze's teachers, Weber and Weisse. In his autobiographical record, communicated by Kuntze, he confesses having become almost an atheist under the influence of his medical studies, until he became acquainted with the philosophy of Schelling, Oken, and Steffens, which dazzled him, touched the poetical and mystical side of his nature, and, though he hardly understood it, had a lasting influence on him. The simultaneous occupation with the best scientific literature of the day (he translated French text-books such as those of Biot and Thénard, and verified Ohm's law experimentally), however, forced upon him the sceptical reflection whether, "of all the beautiful orderly connection of optical phenomena, so clearly expounded by Biot, anything could

to him that we are indebted for the term Psycho-physics, which in the present chapter I have used in a more general sense. Fechner worked independently of Lotze and Helmholtz on the lines of E. H. Weber. He does not seem to have been much influenced by either Kant or Herbart. In 1860 he published his 'Elemente der Psychophysik,' which was to be an exact treatise on the relations of "mind and body," founded upon a measurement of psychical quantities.

Herbart's attempt to submit psychical phenomena to the exact methods of calculation had failed through the want of a measure for psychical quantities. Lotze had suggested the idea of a psycho-physical mechanism—*i.e.*, a constant and definite connection between inner and outer phenomena, between sensation and stimulus. E. H. Weber in his important researches on "Touch and Bodily Feeling" had made a variety of measurements of sensations, and shown that in many cases stimuli must be augmented in proportion to their own original intensity in order to produce equal increments of sensation. These observations lent themselves to an easy mathematical generalisation. Fechner was the first to draw

have been found out by Oken-Schelling's method?" This mixture or alteration of exact science and speculation, of faithfulness and loyalty to facts as well as to theory, runs through all Fechner's life, work, and writings. Much of his poetry, of his fanciful and paradoxical effusions, is meant seriously, and is really more coherent than it appeared to his readers, some of whom knew him only under his pseudonym of Dr Mises. He lived, thought, and worked truly on the borderland of nature and mind, of this world and another, of science and poetry, of reality and fiction. Like Lotze, he wanted the genuinely historical sense. Like Lotze, too, he received from others only suggestions which he elaborated independently in his own original fashion. As little as Lotze does he seem ever to have attempted to realise and understand any other philosophical system than his own. To both, the ultimate problem was capable only of a subjective solution. Cf. vol. i. p. 200.

the attention of philosophers to the existence of this relation in a variety of instances, and collected a large number of facts to prove its general correctness. He conceived the idea of measuring sensations by their accompanying stimuli, a mode of measurement based upon that relation which, under the name of Weber's law or formula, he introduced as a general psycho-physical proposition. The intervals in the numerical scale, the differences in the magnitude of stars, the facts established by Weber relating to our estimate of differences of touch, of weight, and of temperature; lastly, the relation of "fortune physique" and "fortune morale," known to Euler and Lagrange, could all be utilised towards proving the general accuracy, within certain limits, of the psycho-physical formula. The work gave rise to many discussions[1] as to the meaning of the term quantity applied to psychical phenomena, as to methods of measurement, and as to the significance to be attached to the new branch of research

[1] In addition to the 'Elemente der Psychophysik' (1860), of which a second edition appeared in 1890, the author enlarged, discussed, and defended his special ideas and theories in three further publications. The year 1877 produced 'In Sachen der Psychophysik,' the year 1882 the 'Revision der Hauptpunkte der Psychophysik,' and shortly before his death (1887) there appeared, in the 'Philosophische Studien' of Prof. Wundt, his last contribution, "Ueber die psychischen Maasprincipien und das Webersche Gesetz," which Prof. Wundt declares to be "the clearest and most complete exposition of the problem which he gave in the course of the forty years during which he was occupied with it." (See the obituary oration, reprinted in Kuntze's 'Biography,' p. 360.) The attacks on Fechner came from many quarters. In the polemical treatise of 1877 he notices how the views of his critics—Helmholtz, Aubert, Mach, Bernstein, Plateau, Delbœuf, Brentano, Hering, Langer—agree as little among themselves as with his own. He sums up with fine humour: "The tower of Babel was not finished, because the builders could not agree how to build it; my psycho-physical structure may remain standing, because the workmen cannot agree how to pull it down" ('In Sachen,' &c., p. 215).

as well as to the interpretation of the Weber-Fechner law of psycho-physical dependence.

We are indebted to Prof. Wundt of Leipzig for a complete and exhaustive examination of the new province of exact science.[1] He enlarged its boundaries, 31. Wundt.

[1] The psychological school, of which Prof. Wundt can be considered the head or centre, has been contrasted by M. Ribot, in his 'Psychologie Allemande Contemporaine' (1st ed., 1879), with the English school, and, in the exposition in the text, I have taken a similar view. It would, however, be unjust not to note that in England, prior to the publication of Prof. Wundt's principal writings, a development of psychology in the same direction had already begun. The principal representative of this development is Prof. Alexander Bain (born 1818), whose two great works, 'The Senses and the Intellect' (1855) and 'The Emotions and the Will' (1859), appeared even before Fechner's 'Psychophysik,' and were characterised by J. S. Mill as "an exposition which deserves to take rank as the foremost of its class, and as marking the most advanced point which the *a posteriori* psychology has reached," being "the most genuinely scientific analytical exposition of the human mind which the *a posteriori* psychology has up till this time produced" ('Edinb. Rev.,' October 1859, reprinted in 'Dissertations and Discussions,' vol. iii. pp. 99, 100). Bain carried out what had been called by Thomas Brown "the physical investigation of the mind," and was probably the first English psychologist who enriched the older associational psychology by an extensive use of the teachings of physiology; the germ of his theory being contained in a passage cited by him from Johannes Müller: in fact, he appreciated the well-known dictum of the latter, "*psychologus nemo nisi physiologus.*" Shortly after the appearance of Prof. Bain's works, the overmastering influence of the evolutionist school in England, headed by Mr Spencer and supported by Darwin, and the pronounced opposition with which the psycho-physical school started in Germany, cast somewhat into the shade the steady development, in this country, of the exact science of psychology by those who formed the direct succession to the older, purely introspective, school of Scottish thinkers. As I am not, in the present chapter, treating of psychology and philosophy, but of the attempt to gain, by the methods of the exact sciences, a conception of the phenomena of animation and consciousness, I leave for another occasion the appreciation of the English school of psychology. The members of this school considered physiology as an aid to psychological research, whereas most of the representatives of the modern German school were, to begin with, physiologists or physicists, and only became subsequently psychologists or philosophers. Characteristic of this school are two points: the opposition they made from the start to the existing methods, and their prominent use, not only of observation, but of experiment. The less ostentatious development of English thought would, no doubt, have led in the end, but for the reasons given above, to like results. An opposition similar to that so marked in Germany was, however,

taking in the ground covered by Lotze's medical psychology as well as by Helmholtz's physiology of hearing and seeing; added a large number of measurements of his own, some of them quite original, such as those referring to the time-sense, many of them in confirmation and extension of Fechner's collection of facts; invented new methods and new apparatus; brought the whole subject into connection with general physiology, as also with the more exclusively introspective psychology of the older, notably the English and Scottish, schools; and pointed to the necessary completion which these investigations demand from the several neighbouring fields of research. Through his labours "physiological psychology" as an independent science has for the first time become possible. The influence of his great work on this subject, as also of his teaching and demonstrations, has been very stimulating. With its place in the history of philosophical thought I shall have to deal in a later portion of this history. At present I will merely refer to the leading ideas and contributions it contains to our scientific reasoning on the psycho-physical problem.

32. Physiological psychology.

Wundt approached psychological research from the side of physiology;[1] his earlier writings referred to the

taken up in England in single instances—*e.g.*, by G. H. Lewes and Dr H. Maudsley, the former in favour of Positivism, the latter on the foundation of his 'Physiology and Pathology of Mind' (1st ed., 1867).

[1] The researches of Wundt and the earlier work of Fechner remained practically unknown in this country up to the time of the appearance of the periodical 'Mind,' edited by Prof. Croom Robertson, in 1876, under the generous patronage of Prof. Bain. Even Lotze and Herbart were hardly known in this country. A similar disregard of English psychology existed in Germany. The foremost writers on the history of modern philosophy, such as Erdmann and Ueberweg, wrote as if modern philosophic—including psychological — thought existed only in Germany. Even the singularly impartial and unbiassed

physiology of the senses, to physiology proper, and to such phenomena of psychical or inner life as can be traced, not only in man, but also in the brute creation. He thus seems to have approached psychology with the true instinct and methods of an exact student of nature. In the course of years his psycho-physical studies took more and more the character of an experimental psychology, and in the latest edition of his great work he describes it as such, maintaining that the designation of physiological psychology has rather a historical meaning.[1]

author of the 'History of Materialism,' Albert Lange, does only scant justice to the labours of the English school, J. S. Mill being, in fact, the only English philosophical writer of the middle of the century who was appreciated in Germany. The last twenty-five years have entirely altered this state of things. French and American writers such as M. Ribot, Prof. M'Cosh, and more recently Prof. James, treat impartially of the rival claims of German and English thinkers. 'Mind' has preserved its fairness in admitting contributions from opposite sides; and latterly there has been started by the publishing house of Frommann of Stuttgart, under the editorship of Prof. Falckenberg, a series of very useful monographs on recent thinkers, whose voluminous or scattered writings make it difficult to arrive at a comprehensive and just appreciation of the main drift of their doctrine. Ever since some provinces of philosophy were conquered by exact research, unity of plan has been to a great extent sacrificed; the natural science of mind is becoming split up into fragments like that of life. Prof. Lasswitz has given us for the first time a coherent account of Fechner's philosophy, and although Prof. Wundt had already put forth in his 'System der Philosophie' (1st ed., 1890) a statement of his systematic views, the monograph by Edmund König (1901) is very helpful in fixing the historical position of Wundt and the genesis of his doctrine. I refer to these volumes for a bibliography of the thinkers discussed.

[1] In the introduction to the 'Physiologische Psychologie' (4te Aufl., vol. i. p. 9) Prof. Wundt says, "The conception of experimental psychology has been expanded beyond its original limits, as we now comprehend under it not only those parts of psychology which are directly accessible to experiment, but the whole of psychology; as it makes a direct use of the experimental method wherever this is possible, and an indirect use in all other instances through applying the results gained in the former, and through rendering internal observation more acute. . . . The designation of physiological psychology, which originated in the peculiar historical antecedents of our science, is one-sided. . . . The centre of gravity of the experimental method lies in this, that it alone makes reliable inner observation possible."

Whilst his methods are exact and definite, his aim is, nevertheless, wide and comprehensive; for not only is the animal creation studied as a valuable field for enlarged psycho-physical research, but also the psychology of infancy and of human societies (ethnical psychology) are drawn into the circle of a scientific psychology. At the same time his exposition is directed towards the totality [1] of the phenomena of life and mind, it being his ultimate object to arrive at some appropriate conception of the whole of human existence. In this respect his scientific labours form a counterpart to those of naturalists like Humboldt and Darwin, who did so much to direct the attention of natural science to the whole of nature, her history and economy. It seems to me that Prof. Wundt has similarly introduced into the psycho-physical study of nature the prominent consideration of the mental side of life in its totality, starting, as Darwin and Humboldt did, from a large accumulation of detailed observations.

This regard for the whole problem distinguishes Wundt's writings from those of other eminent psycho-physicists, such as Helmholtz, who deals brilliantly and exhaustively with certain special problems, or Fechner, who relegated the discussion of the fundamental questions to a series of half-poetical treatises, which are full of suggestion rather than close scientific reasoning. But

[1] 'Physiologische Psychologie' (4te Aufl., vol. i. p. 2): "Our science has accordingly the task, first, to investigate those vital phenomena which, lying in the middle between outer and inner experience, require the simultaneous application of both methods of observation, outer and inner; and secondly, to throw light from the points thus gained on the totality of the phenomena of life, and, if possible, to gain in this way a comprehensive conception of human existence." See also his essay "Philosophie und Wissenschaft" in a volume of 'Essays' (Leipzig, 1885), p. 1; also 'Die Aufgaben der experimentellen Psychologie,' ibid., p. 127, &c.

33. Wundt, Fechner, and Lotze compared.

Wundt differs quite as much from Lotze, who also strove to arrive at a view of the totality of human life and its significance. Lotze belonged, in spite of the original and independent view which he took of the psycho-physical problem, to the older school of philosophers. Wundt belongs quite to the modern school.[1] Fechner forms the transition. Lotze begins his psychology, and even his physiology of the soul, with a lengthy dissertation on the unity of the soul as a special being, just as Herbart begins his psychology with metaphysics. This metaphysical introduction, these definitions relating to the essence of the soul, its unity, and its location, are absent in the modern psychology. Instead of founding psychology on experience, metaphysics, and mathematics, Wundt founds it on experience (including experiment), physiology, and mathematics. In consequence of this altered foundation a new problem has arisen, precisely as a new problem arose for biologists when they discarded vital force as a meaningless and useless encumbrance. For the older biologists life was the exhibition

[1] See the preface to the second edition of the 'System der Philosophie' (Leipzig, 1897), p. ix: "I have always tried to co-operate in the endeavour to secure for psychology an independent position as an empirical science outside of philosophy, and to see that she should not lack the support of the scientific method in so far as this could be transferred to her. . . . As I started from natural science and then came to philosophy through occupation with empirical psychology, it would have appeared to me impossible to philosophise in any other way than in correspondence with this sequence of the problems. But I quite well understand that the position may be different for him who begins with philosophy and then makes occasional excursions into the regions of science or psychology." Compare with this what Lotze says in the Introduction to his 'Streitschriften' (1857), or the following passage from one of his last essays ('Contemp. Rev.,' January 1880), "Except in rare cases, a prolonged philosophical labour is nothing else but the attempt to justify, scientifically, a fundamental view of things which has been adopted in early life."

of vital force. This having been dropped, the question arose for modern biology, What is life? We thus find thinking biologists of the modern, exact school aiming at a mechanical definition of life. Many answers have been attempted, such as that it is the action of a very complex chemical molecule, of dynamical equilibrium, of metabolism, of a special form or organisation, &c. Similarly, when the word soul dropped out of psychology in its older metaphysical meaning as a separate being or entity, when it was used to mean only the sum-total of the inner or psychical phenomena, a new problem arose for the psycho-physicist or experimental psychologist. The problem now was to give some definition of the unity and unified totality of all inner or mental phenomena. The older metaphysical psychology, as also for the most part the so-called empirical psychology, answered this question by placing the conception of an independent entity, the soul, person, or self, at the opening of their discussions. Modern exact psychology cannot do this. For it the unity of the inner life and its unified totality has become a problem. This problem Prof. Wundt faces fully and fairly. He asks himself the question, Wherein consists the unity of consciousness, wherein the totality of all mental life, individual and collective? Armed with the methods of exact research, he tries to extract from the whole array of mental phenomena an idea of their essence as distinguished from external or natural phenomena, and of their collective meaning and significance. In so doing he enters the domain of philosophy, and his results belong to the realm of philosophical thought. When dealing with that large section of my

34. The unity of consciousness.

subject I shall have to take up Wundt's theories where I now leave them.

Through the efforts and widespread influence of Prof. Wundt, the inner or psychical phenomena have been drawn into the circle of exact research; a large portion of psychology has become natural science. It is quite consistent with this that some of the disciples of the modern school should have assumed towards the new branch of natural science the attitude which has become habitual among those who cultivate other natural sciences. All these sciences are based upon observation, aided if possible by experiment; none of them, however, has succeeded in rising to the rank of an exact science without the aid of some generalisation which admitted of clear expression in a few definite conceptions, being the more valuable in the degree that it lent itself to a clothing of mathematical language. In the course of the last centuries, notably the nineteenth, several of these fundamental principles — such as the laws of motion, gravitation, atomism, vibratory motion, the conception of energy, natural selection, metabolism—have attained in various degrees, some almost perfectly, to this state of definiteness, and the sciences built up by their aid have accordingly acquired the character of certainty. Psycho-physics having through Weber, Lotze, Fechner, and Wundt gradually evolved the notion of a partial parallelism of physical and psychical phenomena, the conception of a mathematical dependence or of function could be introduced between the measurable external processes and the hidden internal events which we term mental; the whole of the latter being

looked upon as concomitant occurrences, as "Begleit-erscheinungen" or "Epi-phenomena" of the more accessible though very complex phenomena of the nervous system and its centres; whereby it had to be noted, that whilst the external visible processes exhibit that continuity in time and space which is characteristic of all physical phenomena, the epi-phenomena were subject to discontinuous appearance and disappearance, to sudden growth and collapse. Having got hold of this partial formula, which in some cases admits even of a rigorous mathematical expression, psycho-physics had no pressing need of investigating its meaning any further, or of inquiring into the supposed independent existence or significance of the "epi-phenomena" as such; similar general inquiries into the origin of gravitation, of atoms, of the essence of energy or inertia, having proved to be of little or no use in furthering astronomy, chemistry, thermodynamics. It cannot be denied that this is a perfectly tenable scientific attitude. Such an attitude has notably been taken up by Dr Hugo Münsterberg, and by what we may term the Freiburg school of psycho-physics. Also there is no doubt that through a series of very cleverly contrived experiments—particularly those referring to the muscular sense and the time sense—a good deal of light has been thrown upon such mental processes as association of ideas, attention, apperception, and voluntary effort, which have thus been brought into closer correspondence with changes taking place in the nervous system. In fact, a parallelism of neurosis and of psychosis has been more and more established.

35. Doctrine of parallelism.

This doctrine of psycho-physical parellelism, also

called the conscious automaton theory, is the central conception in psychology as a natural science, or, as I have termed it, of the psycho-physical view of nature. It was prepared[1] by earlier thinkers, such as Descartes, and, in a different form, by Spinoza,[2] and by Leibniz's doctrine of pre-established harmony.[3] It has been strengthened by the physiological theory of reflex action,[4] and, independently, by psycho-physics in the narrower sense of the word, as founded by Weber and Fechner. But the possibilities of the automaton theory were not scientifically tested till towards the end of the nineteenth century. In this country, two thinkers

[1] The doctrine of psycho-physical parallelism and its historical genesis is given by Huxley in his address before the British Association Meeting at Belfast in 1874, "On the Hypothesis that Animals are Automata, and its History," in which he goes back to Descartes and Charles Bonnet. A good account of the theory is also given by Prof. Wm. James in the 5th chapter of his 'Principles of Psychology'; and it is fully discussed by Prof. James Ward in his Gifford lectures, 'Naturalism and Agnosticism,' vol. ii. pt. iii.

[2] The passage from Spinoza which is constantly quoted, and, as Prof. Ward says, usually in ignorance of the context, is in 'Ethica,' part ii. prop. 7: "Ordo et connexio idearum idem est ac ordo et connexio rerum."

[3] Leibniz, as Huxley (*loc. cit.*) tells us, also invented the term "automate spirituel" and applied it to man.

[4] Du Bois-Reymond, in his "Éloge" of Johannes Müller, has shown that the principle of reflex action dates back to Descartes, who also introduced the term reflex. Next in time came Willis ('De motu musculari,' Amsterdam, 1682). The subject seems to have been overlooked to such an extent, that Prochaska (1784) got for a long time the credit of having established the notion of reflex action, and even his work had to be rediscovered by Eduard Weber (1846), after the principle of the transition of a reaction from the afferent to the efferent nerves in the central organs had been prominently put forward by Legallois (1811), Marshall Hall (1835), and Johannes Müller (1835). In more recent times, Prof. Pflüger's "Laws of Reflex Action," and his and G. H. Lewes's theory of the presence of consciousness in the spinal cord, have formed the subject of much discussion and much experimental work. A good historical account will be found in the 13th Leçon of M. Ch. Richet's 'Physiologie des Muscles et des Nerfs' (Paris, 1882), and a discussion of the whole subject in Prof. Wundt's 'Physiologische Psychologie,' ch. xxi., where especially the difference between automatic and reflex movement is brought out.

of eminence, Huxley and Clifford,[1] have made the theory accessible to the popular understanding, without, however, taking a comprehensive view of the study of mental phenomena, inasmuch as they approached the subject from the side of natural science—the former more from that of physiology, the latter from that of the mechanical sciences. Prof. Wundt treats the subject exhaustively in many passages of his works, notably in the last chapter of his great work on 'Physiological Psychology,' in which he broadly defines "the psycho-physical view as that view which starts from the empirically well-established thesis, that nothing takes place in our consciousness which does not find its foundation in definite physical processes. The simple sensation, the connection

[1] Although neither Huxley nor Clifford added anything new to the conception of parallelism as contained in the writings of many earlier Continental philosophers, the fact that they were driven from their purely scientific positions to discuss the subject, and were not psychologists and metaphysicians by profession, gave their expositions, which are otherwise as fresh as they are immature, a peculiar charm. Being both masters in style, they at once enriched the vocabulary with new terms which have since become classic. The word "epi-phenomenon," an equivalent for the German 'Begleiterscheinung,' which is of independent origin but expresses Huxley's view, is a real enrichment of thought. It is also the direct way to bring home the absurdity of the whole theory. The things of nature being first considered as "phenomena"—*i.e.*, as "appearing" to some one,—the some one is next looked upon as a secondary phenomenon, an epi-phenomenon. Clifford actually in his psychological atomism goes the length of saying, "Reason, intelligence, and volition are properties of a complex which is made up of elements, themselves not rational, not intelligent, not conscious" (see 'Mind,' vol. iii. p. 67). In the physical theory of atoms it has been truly said that you cannot get anything out of the atoms that you have not, to begin with, put into them. Clifford's dictum reminds one of Carlyle's definition of the object of political economy, which has to solve the problem, "Given a community consisting of fools and knaves, how to produce efficiency and honesty by their combined action?" Clifford's solution of the psychological deadlock is the "Mind-stuff" theory, the theory that all matter is the phenomenal correlate of the elements of mind. Clifford's essay "On the Nature of Things in themselves" is reprinted in 'Lectures and Essays' (1879), vol. ii. p. 71 *sqq*.

of sensations and perceptions, their associations, finally, the processes of apperception and volition, are accompanied by physiological nerve-processes. Other bodily processes, such as the simple and complex reflex actions, do not enter directly into consciousness, but they form important auxiliary processes of the phenomena of consciousness.[1] It is, accordingly, quite consistent, from a purely scientific point of view, to test this central conception of exact psychology, and to refrain from introducing any purely psychical conceptions so long as the possibilities of the conception, that mental phenomena are only concomitant occurrences of changes which take place in the nervous system and centres, have not been exhausted. Investigations, with or without this definite purpose, have been very largely prosecuted in the course of the nineteenth century, and have been in part purely anatomical, in part physiological, the latter again either referring to pathological or to normal cases. Systematic courses of experiments have been begun at Leipzig and taken up, according to a well-defined special programme, by Dr Münsterberg at 36. Münsterberg.
Freiburg, who in the researches of his laboratory has, more distinctly than any other philosopher, adopted the theory as a working hypothesis.[2]

[1] 'Physiologische Psychologie' (4 Aufl.), vol. ii. p. 644.

[2] The principal writings of Dr Münsterberg, in which his psycho-physical researches are contained, are: 1, 'Die Willenshandlung,' Freiburg, 1888; 2, 'Beiträge zur Experimentellen Psychologie,' 4 parts, 1889-92; 3, 'Ueber Aufgaben und Methoden der Psychologie,' being part 2 of the 'Schriften der Gesellschaft für Psychologische Forschung,' 1891. These writings, although starting from the position prepared by the Leipzig school of psycho-physical research, are largely polemical, and directed against some of Prof. Wundt's principal theories. They have received a considerable amount of attention in Germany and America and in this country, and also a good deal

It can hardly be said that this course of study has done more than make a start, and even those who are inclined to consider it a very one-sided attempt are bound to admit that it has a promising future. Thus Prof. Wm. James, whose 'Principles of Psychology' treat of the subject from many and very different points of view, refers to these experiments in a characteristic passage as follows: "Within a few years, what one may call a microscopic psychology has arisen in Germany, carried on by experimental methods, asking of course every moment for introspective data, but eliminating their uncertainty by operating on a large scale and taking statistical means. . . . Their success has brought into the field an array of experimental psychologists, bent on studying the elements of mental life, dissecting them out from the gross results in which they are embedded, and, as far as possible, reducing them to quantitative scales. . . . The mind must submit to a regular siege, in which minute advantages, gained night and day by the

of opposition. The late editor of 'Mind,' Prof. Croom Robertson, reported pretty fully upon Münsterberg's work in the 15th volume of the first series of 'Mind,' and drew especial attention to the confirmation which certain views contained in the writings of the British Associationist school have received through Dr Münsterberg's expositions. Prof. E. B. Titchener criticised Dr Münsterberg's experiments and theories somewhat severely in the 16th volume of the first series of 'Mind,' p. 521 *sqq.* As the subject is still under discussion, and as in more recent writings of Dr Münsterberg, who is now professor at Harvard University, his studies have shown quite a different side from that exhibited by the above-named earlier writings, it is impossible in this history to do more than refer to them as marking a distinct phase in modern psycho-physical thought. It does not appear that Prof. Wundt agrees with much of the outcome of the important movement he originated; see his article in 'Philosophische Studien,' vol. vi. p. 382, and a very valuable paper by Prof. J. Ward ('Mind,' 2nd series, vol. ii. p. 54 *sqq.*), entitled "Modern Psychology: a Reflexion." As these discussions refer more to the philosophical value than to the purely scientific aspect of psycho-physics, they would lead us beyond the regions of purely scientific thought.

forces that hem her in, resolve themselves at last into her overthrow. There is little of the grand style about these new prism, pendulum, and chronograph philosophers. They mean business, not chivalry. What generous divination and that superiority in virtue which was thought by Cicero to give a man the best insight into nature have failed to do, their spying and scraping, their deadly tenacity and almost diabolical cunning, will doubtless some day bring about. . . . The experimental method has quite changed the face of the science, so far as the latter is a record of the mere work done."

It is, however, only fair to remark that it has never been the object of any science, and can, therefore, no more be the object of exact psychology, to deal with everything at once, and that psycho-physical science has quite as much right to postpone the question, What is mind? [1] as biological science has had to postpone, or even to eliminate, the question, What is life? But this comparison reveals also the essential difference between the exact science of life and the exact science of mind. Of life we know only through the observation of living beings, but of mind we have not only the apparent knowledge of its unity, which introspection forces upon

[1] "Sensation, Retentiveness, Association by Contiguity,—these are to be our ultimate and sufficient psychological conceptions: the facts of feeling and conation are resolved into facts of sensation; and all mind-processes held to be not merely conditioned, but explained by brain-processes, which they accompany as epi-phenomena or 'Begleit-erscheinungen.' It is not so long since the world was shocked at Lange's *mot* about a psychology without a soul, but the 'modern' psychology is a psychology without even consciousness. 'Content of consciousness' as much as you like, but consciousness itself, consciousness as activity, is not our affair; we leave that to metaphysics, say our 'modern' teachers." (Prof. J. Ward, on "Modern Psychology," 'Mind,' 2nd series, vol. ii. p. 55).

us, but we have also a large array of external facts which have been appropriately defined by the term "the objective mind." There are, in fact, two properties with which we are familiar through common-sense and ordinary reflection as belonging specially to the phenomena of our inner self-conscious life, to the so-called "epi-phenomena" of the higher organic or nervous systems, and these properties seem to lie quite beyond the sphere and the possibilities of the ordinary methods of exact research. The first of these properties is the peculiar unity exhibited by the higher forms of organic existence, and still more evident in the phenomena of mental or inner life. Instead of unity, it might perhaps be better to call it centralisation. Now, the more we apply mathematical methods, the more we become aware of the impossibility of ever arriving at a comprehensive unity by adding units or elements together. The sum of atoms or molecules, however artfully put together, never exhibits to our reasoning that appearance of concentration which the higher organisms or our conscious self seem to exhibit. In this circumstance lies the difficulty of ever arriving at any really satisfactory definition of life—which definition eminent physiologists have, as we have seen, felt compelled ultimately to relegate to the realm of the idea. In the last chapter I showed how modern research into the phenomena of life has impressed upon our thoughts the ubiquity, the continuity, and the unique character or singularity of life, without being able to fix upon any one satisfactory mechanical definition of life. But as we ascend in the scale of living things we become aware of another property: they are centred—*i.e.*, they exhibit a

37. Phenomenon of centralisation.

special kind of unity which cannot be defined, a unity which, even when apparently lost in the periods of unconsciousness, is able to re-establish itself by the wonderful and indefinable property called "memory"—a centre which can only be very imperfectly localised—a together which is more than a mathematical sum; in fact, we rise to the conception of individuality—that which cannot be divided and put together again out of its parts.

The second property is still more remarkable. The world of the "epi-phenomena," of the inner processes which accompany the highest forms of nervous developments in human beings, is capable of unlimited growth; and it is capable of this by a process of becoming external: it becomes external, and, as it were, perpetuates itself in language, literature, science and art, legislation, society, and the like. We have no analogue of this in physical nature, where matter and energy are constant quantities, and where the growth and multiplication of living matter is merely a conversion of existing matter and energy into special altered forms without increase or decrease in quantity. But the quantity of the inner thing is continually on the increase; in fact, this increase is the only thing of interest in the whole world.

38. Externalisation and growth of mind.

Now, no exact scientific treatment of the phenomena of mind and body, no psycho-physical view of nature, is complete or satisfactory which passes by and leaves undefined these two remarkable properties of the inner life, of the epi-phenomena of nervous action, of consciousness. And it seems to me that Prof. Wundt is the only psycho-physicist who, starting from science and trying to penetrate by scientific methods into the inner or psychic

39. Wundt's treatment of central problem.

world, has treated the subject comprehensively, and fairly and fully tried to grapple with these two facts peculiar to the inner world—its centralised unity and its capacity of unlimited growth through a process of externalisation. He has done so by his philosophical theory of "apperception and will," and of the "growth of mental values," two conceptions which lead us into the realm of philosophical thought.[1]

But, before closing this chapter, which deals with the study of the phenomena of an inner life and the interaction of body and mind by the methods of exact research, it is well to note that long before psychology existed as a natural science, a large amount of knowledge had been accumulated by a different method. Especially in this country—ever since the time of Locke—there has existed a very large and influential school of thinkers who studied the inner phenomena by what has been appropriately termed the inner sense; every observer recording his own inner experience and leaving it to others, by doing the same, to confirm or correct his statements. Psychology, carried on through self-observation or by the

[1] It would serve no good purpose to string together a list of quotations from Prof. Wundt's voluminous writings in which these two central ideas of his philosophy find expression, especially as there is no one passage to be found in which his highest abstractions and final conclusions find an adequate expression, still less one which could be conveniently rendered in the English language. König has, it seems to me, done much to make Wundt's view more easily understood, and I must content myself at present with referring to his little volume, notably to the extracts given on pp. 134, 141, and 167, which explain more clearly the theory of apperception and will. On the theory of the "growth of mental values," see especially Wundt, 'System der Philosophie' (2 Aufl., pp. 307, 596), "Mental life is, extensively and intensively, governed by a law of growth of values: extensively, inasmuch as the multiplicity of mental developments is always on the increase; intensively, inasmuch as the values which appear in these developments increase in degree" (p. 304).

introspective method, had grown to large dimensions in Scotland and in England, long before Herbart and Beneke in Germany gave it a similar direction. In fact, most of the writings of the introspective school in Germany, which dates from the middle of the century, is concerned with the material accumulated by British psychologists. And even the psycho-physical method itself would carry us only a little way if its results and observations could not continually be checked, supplemented, and interpreted by what we already know by introspection. One of the foremost representatives of the English school of psychology has said, and many will agree with him,[1] "in our desire to know ourselves—to frame some conception of the flow of our feelings and thoughts—we work at first by introspection purely; and if at a later stage we find means of extending and improving our knowledge, introspection is still our main resort—the Alpha and Omega of psychological inquiry: it is alone supreme, everything else subsidiary. Its compass is ten times all the other methods put together, and fifty times the utmost range of psycho-physics alone."

40. Introspective method.

A history of Thought must accordingly contain some account of the view which our century has taken of the introspective method and the value of the inner sense as a means of enlarging our knowledge.[2] This discussion

[1] See Prof. Bain's essay in 'Mind,' 2nd series, vol. ii. p. 42: "The respective Spheres and mutual Helps of Introspection and Psycho-physical Experiment in Psychology."

[2] One result of the modern psycho-physical view, or of the doctrine of parallelism of physical and mental states, has been not only to develop a clearer view of physiological psychology, but also to define more clearly the object of psychology proper — that is, of the science which deals with the facts revealed by introspection. When, in the middle of the century, the physiology of the senses attracted the

will, in a future volume, form one of the appropriate links which join science to philosophy—which lead us on from exact to speculative thought. At present I have to refer to another and very extensive field of research, into which the natural as well as the speculative philosopher have been led from opposite sides, and which especially affords a hopeful prospect for an enlargement of the psycho-physical view of nature. If the natural philosopher cannot consistently and fairly enter into the mysteries of an inner consciousness from which his opponent—the speculative philosopher—starts, he may perhaps do so by a roundabout way or a side-door.

As I stated above, the inner world, the psychosis, which intermittently accompanies the neurosis, the epiphenomenon which lies on the other side of the phenomenon, is not only characterised by a peculiar unity or centred connectedness which we look for in vain in the external and physical world; it has also become external or objective, it has detached itself from the subjective and hidden source from which it sprang, and can be studied as such in the great creations of language, literature, society, science, art, and religion. Why not study its nature and its life in these great and undeniable manifestations, and instead of beginning at the hidden source, the unknown and indefinable centre, try to reach this by beginning at the periphery, measuring out the great circle and learning what it contains?

41. The "objective mind."

attention of psychologists in all the three countries, it became customary to introduce purely psychological treatises by an exposition of the psycho-physical relations, introducing into psychology chapters from physiology. The consequence of this has been that modern works on psychology have grown to inordinate length, and frequently exhibit a dual aspect and method. Quite recently it has therefore been insisted on that psychology can be written either from the physiological or from the purely psychological point of view. A good example of the latter is Prof. G. F. Stout's 'Analytic Psychology' (2 vols., 1896). "Physiological results," he says (vol. i. p. 37), "are likely to be valuable only in proportion as they are controlled and criticised by psychological analysis. This holds good apart from consideration of such metaphysical questions as whether the brain-process is the sole real agency, and consciousness a mere function, or consequence, or epi-phenomenon; or whether consciousness is the reality of which the correlated brain-process is a phenomenon, or whether they are two aspects of the same fact. Whatever may be our attitude to such questions, the psychologist has still his own work to do on his own lines; and for the sake of physiology itself, so far as it entertains the hope of throwing light on the mechanism of brain-processes, he must attempt to do it. It is idle to require psychology to wait for the progress of physiology. Such a demand is logically parallel to a demand that history or biography, or the practical estimate of character and anticipation of men's actions in ordinary life, shall come to a standstill until they have a sufficient physiological basis. On this view, Carlyle should have abstained from writing his 'French Revolution,' because he did not know what precise configuration and motion of brain particles determined the actions of the mob who stormed the Bastille."

Ancient philosophy, which found its consummation in the writings of Aristotle, had already begun this work, and, in establishing the rules of grammar and logic, had furnished the material for many modern speculations. What the ancients had only begun, modern thinkers of the most opposite schools have been induced to continue on more methodical lines, and with the more or less distinct object of learning something definite regarding that mental life and unity which they have, with little success, tried long enough to reach by various direct roads, such as introspection, speculation, physiological and psycho-physical experiment. Accordingly we find springing up almost simultaneously in the three countries, ever since the latter part of the eighteenth century, the study of mankind or of human culture in all its historical forms. Hume and Adam Smith, Montesquieu, and the French physiocrats, studied society and the great fabric of industry and commerce; Cabanis and the "Idéo-

logues" pointed to the importance of the philosophical study of language and grammar; the idealistic school in Germany ended by leading to the study of the objective mind in history, art, and philosophy; the school of Herbart in Waitz, Lazarus, and Steinthal led into "Völkerpsychologie" and "Sprachwissenschaft"; and it is well known how in our days the synthetic philosophy of Mr Herbert Spencer in England has entered on the study of sociology on the large scale. We hear on all sides of natural histories of mankind, of society, of religion, &c., and they appear either in the modest attire of the other and older natural histories which we have been accustomed to, preparing the ground by patient and unbiassed collection of facts, or they attach themselves to certain philosophical theories, such as are furnished by the dialectics of Hegel, or by the evolutionary doctrine of Darwin and Spencer, in connection with which we shall meet them in a future section of this work. For it has been found here, as it had been in the older natural histories, that the accumulation of facts and materials was of little use unless some leading idea was at hand by which it became possible to regulate and arrange them.

Thus we see how the psycho-physical problem—the question of the interaction of mind and body, of soul and nature, of the inner and the outer worlds—is being attacked from two entirely different sides,—from the side of the individual and from that of the collective life of the human being: the mental principle is being studied in its inner and hidden existence as the unifying and centralising factor of individual life, or in its ex-

ternal manifestations in history, society, science, art, industry, and religion,—in fact, in the history of culture and civilisation. If Bishop Berkeley has, with some propriety, been called "the historical starting-point" of psycho-physical investigation of the first kind, the importance of that of the second and wider kind is nowhere more clearly and definitely expressed than—over a century ago—in the writings of Johann Gottfried Herder.[1] His influence in this direction was very

42. Its study prepared by Herder.

[1] The influence of Herder (1744-1803) on German literature and thought was fully acknowledged by his contemporaries, as is testified by the frequent references to him in the biographies of nearly all the eminent men who lived at the end of the eighteenth and the beginning of the nineteenth centuries, as also in the voluminous correspondence which he carried on with many eminent contemporaries. Had it not been for the overpowering and one-sided influence which the critical, and, later, the transcendental, schools of thought gained, notably at the German universities, Herder's ideas would have been more generally acknowledged as forming, to a very great extent, the starting-point of many lines of research which were not exclusively controlled by the ruling philosophies, and which gradually and imperceptibly united at a later date to form the more modern current of German thought. Herder was much more allied with the historical studies referring alike to nature, literature, and culture, than with the critical and metaphysical systems, being also well acquainted with contemporary English thought, as, *inter alia*, with the curious writings of Lord Monboddo. Through Madame de Staël, who was intimate with Herder, his writings were early known in France, whereas Carlyle's studies in German literature, though most valuable and original in their way, do not give that prominence to Herder's writings which they deserve. In more recent times, after the indefatigable Düntzer, through the publication of his correspondence, had done much to revive the interest in Herder, full justice has been done to his great merit by Rudolf Haym, whose great work, 'Herder nach seinem Leben und seinen Werken' (2 vols., Berlin, 1885), is a perfect mine of information. The side of Herder's influence which is not sufficiently dwelt on by Haym, but which interests us most at present,—what we may call his anthropological view,—had already been exhaustively dealt with by Dr Heinrich Boehmer in his little-known 'Geschichte der Entwickelung der Naturwissenschaftlichen Weltanschauung in Deutschland' (Gotha, 1872), who especially draws attention to the psycho-physical ideas of Herder. It has been truly said that there is hardly any modern idea which has found widespread application that cannot be traced in the writings of Herder; but Herder had no method, having

great, and would have been greater had he not lived at a time when the study of the human mind by the purely introspective or speculative methods had absorbed all philosophical interest in England and Germany. His opposition to the (abstract) subjective philosophy of Kant and Fichte made him unpopular; he was only half understood at the time; and only towards the end of our century have his ideas been recognised as containing the clear conception of psycho-physics on the large scale—*i.e.*, of the natural history of humanity, the genesis and evolution of the objective mind.

Herder was a pupil of Kant during his pre-critical period. He was still more influenced by great naturalists like Haller, Buffon, Camper, Sömmering, Forster, and Blumenbach, who through physiology, comparative anatomy, and ethnology, attempted to bring the study of the human race and its mental development into connection with that of the brute creation, of the surrounding plant-life, of the characteristics of climate and soil, and of the great natural features of sky and landscape. He did not believe that we could study the great forces of nature and mind from inside or in the abstract—he desired to follow Haller's physiology, to complete and continue it into psychology. Irritability,[1]

characteristically maintained that method is frequently only a convention, and he was deficient in critical acumen. The German mind had to go through the severe discipline of the school of mathematical and critical thought, and to amass an enormous volume of experimental and historical knowledge, before the brilliant conception of Herder in his great work 'Ideen zur Geschichte der Menschheit' (4 pts., 1784-87) could be partially realised by A. von Humboldt in his 'Kosmos' (1841-59), and by Lotze in his 'Microcosmus' (1856-64). See especially the preface to the latter.

[1] See above, p. 471, on a similar development of Haller's teaching through Cabanis in France somewhat later in time.

the highest physical phenomenon of matter, was to be the starting-point of this psychology. In an early essay on understanding and sensation (1778) he wrote: "According to my thinking there is no psychology possible which is not at every step definite physiology. Haller's physiological work once raised to psychology, and, like Pygmalion's statue, enlivened with mind, we shall be able to say something about Thought and Sensation."[1]

But this psycho-physiological view was not limited to the study of the individual: it widened out and embraced the whole of mankind; nature on a large scale had to be observed; historical records had to be collected on all sides; origins had to be studied and the elementary forces followed up in the beginnings of poetry, art, and religion. Materials were gathered everywhere from historians, chroniclers, travellers, primitive records, and the "voices of the peoples." All this was to furnish the materials for a "History of Mankind." "In many

[1] "Vom Erkennen und Empfinden der menschlichen Seele" (1778), in the 9th vol. of the Works of Herder ('Abtheilung zur Philosophie und Geschichte,' 1828). To give an idea of Herder's anticipation of modern views, see p. 10: "We cannot penetrate deeper into the genesis of sensation than to the remarkable phenomenon called by Haller 'Reiz.' The irritated fibre contracts and expands again; perhaps a 'stamen,' the first growing sparklet of sensation, towards which dead matter has purified itself by many steps and stages of mechanism and organisation." Many passages could be quoted from Herder's 'Ideen,' &c., and other writings, anticipating modern Darwinian ideas, such as those of the struggle for existence, and even of automatic selection. See Prof. J. Sully's appreciative article on Herder in the 'Ency. Brit.' (9th ed.), and notably Fr. von Bärenbach, 'Herder als Vorgänger Darwin's' (Berlin, 1877). Haym ('Herder,' vol. ii. p. 209) objects to this extreme view of Herder as a forerunner of Darwin on the ground that, according to the former, no animal in its development ever forsook that adjustment of organic forces peculiar to it, nature having kept each being within the limits of its type. Accordingly, Herder's evolutionism would be more akin to that of K. E. von Baer than to that of Darwin and Haeckel.

43. His 'History of Mankind.' parts," he says,[1] "my book shows that one cannot as yet write a philosophy of human history, but that perhaps one may write it at the end of our century or of our chiliad."

And indeed the whole of our own century has been busy in carrying out this prophetic programme of Herder's, consciously as planned by him in Germany—unconsciously and independently in other countries. As a counterpart to the introspective labours of Kant and their followers, a large array of naturalists, historians, philologists, and ethnologists have in the spirit of Herder ransacked every corner of the globe and every monument of history with the distinct object of tracing there the physical basis and the workings of that inner and hidden principle which we call the human mind. In doing this, they or their numerous followers, who belonged to a generation which knew not Herder, have strayed far away from the common starting-point, and have frequently lost themselves in the bewildering details of special research. Above all, in the country to which Herder belonged, a

44. Separation of natural and mental sciences. separation set in early in the century between what have been termed the natural and the mental sciences. The former came more and more under the sway of the mathematical spirit, which, as I showed in an earlier chapter, turned the eyes of its votaries away from their own national scientific literature to that of their neighbours—first to France, latterly to England. The mental sciences, on the other hand,—history, philology, the social sciences,—came under the influence of exactly those philosophical ideas which Herder never understood nor assimilated:[1] the critical spirit of Kant, and the constructive canons of his successors, each of these distinct and separate movements, supplied exactly what was wanting in the prophetic, not to say dithyrambic, utterances of Herder; they supplied coherence and method. Earlier chapters of this book have shown how the mathematical spirit has permeated and revolutionised the natural sciences, and latterly how it has, in the science of psycho-physics, led philosophers back to the problem which Herder had adumbrated at the end of the previous century. A second large department of my task will consist in showing how what in Germany are called the mental sciences have been developed independently of the natural sciences, how the study of the mind as such—

[1] See the preface to the first part of the 'Ideen,' 1784.

[1] During the latter part of his life Herder was occupied to a great extent with those publications in which he gave expression to the opposition which he consistently maintained to the critical writings of his master Kant. His two principal works referring to this are 'Eine Metakritik zur Kritik der Reinen Vernunft' (2 parts, 1799) and 'Kalligone' (1800). Kant had reviewed the first volume of Herder's greatest work, the 'Ideen,' anonymously, criticising the absence of logical acumen and clear definitions, and also the attempt towards a genetic as opposed to a critical treatment of the intellect, the former being an enterprise "which transcends the powers of human reason, whether the latter gropes with physiology as a leader, or attempts to soar with metaphysics." In the second part of the 'Ideen' Herder had taken up a polemical attitude to Kant's teachings, and Kant had again reviewed it, dwelling upon the uncritical manner in which Herder had built up his hypotheses on unsifted material gathered from all sides. In the 'Metakritik' Herder, irritated by what he considered the arrogance of the Kantian school, undertook to put into systematic form his criticism of Kant's principal work, following to a great extent the suggestions thrown out by a mutual friend of himself and Kant, Johann Georg Hamann (1730-80), and falling back upon the earlier philosophies of Spinoza and Leibniz on the one side, and upon the common-sense philosophy of the Scottish school on the other, seeking for a solution of the problems raised by both, not in abstract reasoning, but in the realism of the concrete and the historical sciences. In the 'Kalligone,' Herder similarly attacks Kant's æsthetical philosophy ('Kritik der Urtheilskraft,' 1790), which had been enthusiastically received in Herder's immediate neighbourhood by Schiller. A full account of these controversies will be found in the 2nd vol. of Haym's work.

in its individual and collective existence—has proceeded when separated from that of nature. This survey will start with exactly that movement of thought which was so distasteful to Herder, the critical inquiry of Kant, and it will follow this up to the point when in our days a junction has again been attempted, not unlike in spirit to that dreamt of by Herder, though very much more accurate and precise in method. There is, moreover, one special problem where this has been markedly the case; one phenomenon stands out pre-eminently; it belongs equally to the realm of nature and of mind. After being independently attacked by philosophers, naturalists, travellers, philologists, and latterly by physicists, it has revealed itself as the psycho-physical problem *par excellence;* and it is exactly that which Herder himself treated with special attention. This phenomenon is that of human speech—the problem of language.[1]

45. The problem of language.

[1] The problem of language and the question of its origin independently occupied thinkers in the three countries in the latter half of the eighteenth century. In France the followers of Locke, notably Condillac ('Essai sur l'origine des connaissances humaines,' vol. ii.), wrote on the subject, while Rousseau opposed them ('Sur l'inégalité parmi les hommes,' 1754). In Germany the Pastor Süssmilch, of whom I shall have more to say in the next chapter, wrote an elaborate work to prove the divine origin of language ('Beweis dass der Ursprung der Menschlichen Sprache Göttlich sei,' Berlin, 1776). In order to settle the question the Academy of Berlin offered, in the year 1769, a prize in the following terms: "En supposant les hommes abandonnés à leurs facultés naturelles, sont-ils en état d'inventer le langage? et par quels moyens parviendront-ils d'eux-mêmes à cette invention?" a problem which Herder characterised as a "truly philosophical one, and one eminently suited for me." He had already—following Hamann—thought much about the subject, and he proposes, in his prize essay, which was subsequently crowned by the Academy, "to prove the necessary genesis of language as a firm philosophical truth." A short time after Herder had written his essay (1771), there appeared in England, by James Burnett, Lord Monboddo, a work 'On the Origin and Progress of Language' (1773), in which he refers to the ideas of James Harris in his work 'Hermes; or a Philosophical Enquiry concerning Language and Universal Grammar' (1751). The question attracted considerable attention, partly through the eccentricities of Lord Monboddo, of which it has been well said that they appeared more ridiculous to his own than they would to the present age, partly through the controversy which arose shortly after on the publication of Horne Tooke's celebrated 'Ἔπεα πτερόεντα, or the Diversions of Purley' (1786). Herder was acquainted with Monboddo's work, having occasioned a translation of it to be made and written a preface (1787); but he does not seem to have taken any notice of Horne Tooke (1736-1812), who, as the historian of the science of language (Theodor Benfey, 'Geschichte der Sprachwissenschaft,' München, 1869) says, would, for his novel ideas and method, deserve to be put at the entrance of the modern linguistic epoch, had he been able to avail himself of a knowledge of Sanskrit.

In no department of knowledge has the scientific spirit worked a greater change than in the science of language. With the exception of suggestions by Leibniz, who clearly saw the necessity of founding the theory of language on a broader basis than the small number of classical and modern tongues then current afforded, and of some glimpses of a correcter view such as those contained in the much ridiculed writings of Lord Monboddo, we find, up to the end of the eighteenth century, hardly any attempt towards a methodical treatment of the great problem. Philosophical theories and vague etymologies, amounting frequently to little more than punning with words, brought the subject into ridicule. Herder has the great merit of having urged the importance of the study of language and literature in primitive forms[1] as the great gateway into anthropology

[1] This refers to the second greatest work of Herder, his collection of popular songs, published under the significant title of "Voices of the Peoples" ('Stimmen der Völker in Liedern,' 1778), a work which had the greatest influence on German literature as well as on modern philological studies. See Benfey, *loc. cit.*, p. 316, &c. That the publication of the 'Percy Ballads' (1765), of Macpherson's 'Ossian,' and of Lowth's 'Lectures on Hebrew Poetry' (1753), formed a great stimulus to Herder in his historical and poetical studies is shown by Haym in many extracts and passages, also in the prefaces of Herder himself and of his editor, Joh. von Müller (Herder's 'Werke,' 1828, 'Zur schönen Literatur und Kunst,' vols. vii. and viii.)

and the science of humanity. Through his writings there rose two distinct views both fruitful for thought, the philosophico-historical and the strictly scientific. His immediate successors, or rather those who unconsciously imbibed the spirit of his writings, took up the former line. The great development of classical philology in the school of Wolf, the discovery of Sanskrit and the new field of oriental philology, for a time threw the purely scientific aspect into the background. Yet at the same time with Wilhelm von Humboldt and his philosophical interests in comparative philology, we find his brother Alexander giving a large share of his attention to the unknown languages of the New World, of which he has been called "the scientific discoverer."

46. Its exact treatment.

But the real beginnings of an exact treatment of the problem of speech were laid by one who did not come under the conscious influence of Herder, though he came under that of Goethe. By Johannes Müller it was carried further, and it was completed by some of his most illustrious pupils and followers—Donders, Brücke, Helmholtz, and Czermak of Vienna. Through the anatomical and physiological labours of these and other naturalists, joined to the physical analysis of musical notes and sounds contained in the great work of Helmholtz on Acoustics, aided by such instruments as the laryngoscope or throat-mirror, and the wonderful inventions of the phonograph and phonautograph, the organ of speech is now known to be a complicated wind instrument by which pure notes and an almost infinite variety of nasal, labial, dental, palatal, guttural, and other sounds can be produced which form the phonetic ele-

ments of speech. Simultaneously the discovery by Broca, in 1861, of the speech centre in the brain marked an epoch on the physiological side.[1] A new science, called Phonetics or Phonology, has sprung up, and is now universally admitted to have created the modern science of language.[2] In addition to this physiological and physical basis, the superstructure of the science of — 47. Phonetics.

[1] This localisation places the speech centre in "a very circumscribed portion of the cerebral hemispheres, and more especially of the left. This portion is situate on the upper edge of the Sylvian Fissure, opposite the island of Reil, and occupies the posterior half, probably only the posterior third, of the third frontal convolution" (Broca, 'Bulletins de la Société anatomique,' 1861). The discovery resulted from the examination of the brain of patients who had been afflicted with "aphasia," which is accompanied with "a lesion of the posterior half of the third, left or right, frontal convolution, nearly always—nineteen times out of twenty—of the left convolution." The phenomenon of aphasia has ever since been one of the great psycho-physical problems bringing together the most refined and intricate physiological, psychological, and linguistic analyses. To begin with, we have to distinguish *motor* aphasia and *sensory* aphasia. "Our knowledge of this disease has had three stages: we may talk of the period of Broca, the period of Wernicke, and the period of Charcot. Wernicke (1874) was the first to discriminate those cases in which the patient *cannot even understand* speech from those in which he can understand, only not talk; and to ascribe the former condition to lesion of the temporal lobe. The condition in question is *word-deafness*, and the disease is *auditory aphasia*. . . . The minuter analysis of the facts in the light of individual differences constitute Charcot's contribution towards clearing up the subject" (James, 'Principles of Psychology,' vol. i. p. 54).

[2] In the modern science of language we have one among the many cases where a historical or philosophical science is becoming an exact science by attaching itself to physics and physiology. On the other side we have the great movement initiated by Darwin in the purely natural sciences, which, as was shown above, relies on the historical collection of facts and the judicious critical sifting of evidence. "It is phonology," says Prof. Sayce ('Introduction to the Science of Language,' 2 vols., 1880, chap. iv.), "which has created the modern science of language, and phonology may therefore be forgiven if it has claimed more than rightfully belongs to it or forgotten that it is but one side and one branch of the master science itself. . . . It is when we pass from the outward vesture of speech to the meaning which it clothes, that the science of language becomes a historical one. The inner meaning of speech is the reflection of the human mind, and the development of the human mind must be studied historically."

language has likewise been stated to be no longer a historical or a philosophical, but to have become a physical, science. It is true that, as with other natural sciences, so also in this case, the morphological, genetic, and biological aspects can be specially studied; also analogies can be drawn betweeen geology and glossology as to their mode of inductive reasoning. The great authority who first took up this novel position was the late Prof. August Schleicher of Jena, and the same has to a great extent been simultaneously adopted by Max Müller in his celebrated 'Lectures on the Science of Language.' It is interesting to note that Schleicher wrote on the 'Morphology of Language' in the same year in which the 'Origin of Species' appeared, and that he recognised very early the importance of Darwin's work for the science of language.[1] This became still more evident on the publication, twelve years later, of the 'Descent of Man,' and of 'The Expression of the

[1] On August Schleicher (1821-68) see a very valuable article in the 'Allgemeine Deutsche Biographie' (vol. xxxi. p. 402 *sqq.*) by Johannes Schmidt. Very different currents of modern thought, such as we shall in the sequel frequently have to represent as opposed to each other, the study of the classical and of the modern languages, of critical and comparative philology, the historical and the exact spirit, Hegelianism and Darwinism—*i.e.*, logical and mechanical evolution—the influence of Grimm, Ritschl, and Bopp, of botany and grammar, combined to generate in this remarkable man the conception of linguistic as a natural science in contradistinction from philology as a historical science. The principal works in which he developed his original view were: 'Die deutsche Sprache' (1860); 'Compendium der vergleichenden Grammatik der indogermanischen Sprachen' (1861); 'Die Darwin'sche Theorie und die Sprachwissenschaft' (1863); and 'Ueber die Bedeutung der Sprache für die Naturgeschichte des Menschen' (1865). Schleicher's ideas have been taken up in France, notably by Abel Hovelacque ('La Linguistique,' 4ème ed., 1857), who says of him that "he had completely liberated himself from metaphysical aspirations" (p. 6). On the one-sidedness of the purely physical theory of language see Sayce, 'Introd. to the Science of Language' (1880), vol. i. p. 76, &c.

Emotions in Man and Animals' a year after. These writings did more than any others to impress upon philosophers the genetic or historical view, the existence of an unbroken chain or transition from the lower to the higher and the highest forms of animal structures, and culminated in the well-known expression of Darwin, that "in a series of forms graduating insensibly from some ape-like creature to man as he now exists, it would be impossible to fix at any definite point when the term 'man' ought to be used."[1] This dictum has been the theme on which endless variations have been played down to the present day—Prof. Ernest Haeckel's address to the Congress of Zoology at Cambridge in 1898 being the latest summary of the physical aspect of the problem. But the problem has also a psycho-physical side, and this aspect is concentrated in the problem of language. Even those philologists who, like August Schleicher and Max Müller, look upon the science of language as a natural science, bring in at this point the accumulated and weighty evidence of the historical, psychological, and philosophical researches into the growth and development of human speech and human thought, as absolutely negativing the possibility of a gradual transition from the brute to the human creation. To the latter, language, which he considers to be the union of definite concepts with definite names, is the Rubicon which cannot be crossed,[2] the chasm which divides that portion of the

48.
The dividing line between man and brute.

[1] 'Descent of Man,' 1st ed., vol. i. p. 235.

[2] See Max Müller, 'The Science of Thought,' *passim*, notably chap. iv. p. 177, where he quotes and maintains his dictum of 1861 ('Lectures on the Science of Language,' vol. i. p. 403): "Language is our Rubicon, and no brute will dare to cross it." Referring to Schleicher, he says (p. 164): "Professor Schleicher, though an enthusiastic

living creation which is capable of an unlimited development and an external realisation of its inner life from that which has no mental history or development: it is the point of discontinuity in the physical development. The study of language in its physical and mental aspects —*i.e.*, in phonetics and in sematology—affords, according to this view, the only means of penetrating from outside into the inner world of thought: it is the psycho-physical problem *par excellence*—the "Science of Thought."

Inasmuch as in this latest development of psychophysics the whole of the accumulated material and most of the arguments have been drawn from the historical and philological researches of such thinkers as Schlegel, W. von. Humboldt, Bopp, Grimm, and their followers, who were without exception trained, not in the mathematical but in the philosophical schools of Thought which ruled in the earlier part of our century, the further consideration of their ideas belongs properly to that portion of this work which will deal specially with philosophical thought and its application in such separate branches as are presented, *inter alia*, by the historical sciences.

admirer of Darwin, observed once jokingly, but not without a deep irony, 'If a pig were ever to say to me, "I am a pig," it would *ipso facto* cease to be a pig.' This shows how strongly he felt that language was out of the reach of any animal, and constituted the exclusive or specific property of man. I do not wonder that Darwin and other philosophers belonging to his school should not feel the difficulty of language as it was felt by Prof. Schleicher, who, though a Darwinian, was also one of our best students of the science of language. But those who know best what language is, and still more, what it presupposes, cannot, however Darwinian they may be on other points, ignore the veto which, as yet, that science enters against the last step in Darwin's philosophy."

It now only remains for me to sum up in a few words the leading conceptions which the psycho-physical view of nature has forced upon us. In the last chapter I showed how the study of life has in the course of our century more and more brought out the conviction that life is a continuous, a ubiquitous, and a unique phenomenon; an exhaustive or even a working definition of life being so far hardly possible. In this chapter we have learnt, by following the psycho-physical lines of research, to distinguish another and peculiar side of the higher forms of living matter, that which is commonly called the mental, inner, or self-conscious side. This appeared, when viewed externally, as a discontinuous epi-phenomenon—"eine Begleiterscheinung"—of some very complex physiological processes and anatomical arrangements of living matter, and as such it exhibits a property with which we are otherwise not familiar in the visible phenomena of nature—namely, discontinuity. Viewed externally, the inner phenomena, which we comprise under the term "mind," appear and disappear, their continuity being preserved in association with the permanence of the external substratum or basis to which they are attached, and internally regained by the indefinable property of memory. But inasmuch as we have not only an external but also an internal knowledge of at least some of these epi-phenomena, we have had forced upon us an entirely different view of this inner life, of mind. To the inner view there exists in self-conscious beings a centre of relatedness—a special kind of unity which we call individuality or personality; and this inner unity is capable of being externalised or made objective in the

49. Summary.

mental life of mankind, language being the great instrument by which this is accomplished. In this external or objective existence—which, however, is only intelligible to beings which form a part of it—that continuity is regained which in the existence of every individual is continually being interrupted and in danger of being lost. Psycho-physical research reveals to us the existence of a unity different from that visible in merely external or physical nature,—a centred unity which is something else than the sum of parts in a mathematical whole. Through this process of centralisation and externalisation there has been formed in the physical world, or in nature, a new world—the world of mind, which is continually growing in contrast to the former, which only changes without increasing or losing its two constituents, matter and energy.

This new world within the old one, this creation of man, forms indeed a portion of nature—it is the microcosm in the macrocosm. It might be investigated by the usual methods of exact research; and the science of anthropology, with its many branches, proposes to study it in the same way as natural history in modern times has studied the social life of certain animals, such as bees, ants, and beavers. Inasmuch, however, as the exact methods do not lead very far, and have continually to appeal to the interpretations of psychology, gained by personal experience and introspective methods,[1] it seems

[1] Prof. E. Hering ('Ueber das Gedächtniss als eine allgemeine Funktion der organischen Materie,' Vienna, 1870) says: "So long as the physiologist is only a physicist he stands in a one-sided position to the organic world. This one-sidedness is extreme but quite

more practical to range the whole of these researches within that great realm of thought which starts with a distinct recognition of conscious individual life as its source and centre. As such, in fact, these researches have been till quite recently carried on, and the main lines of their recent development belong accordingly to philosophic as distinguished from scientific or exact thought.

The three great facts, however, which even the exact treatment of mental phenomena has impressed upon us —namely, the existence of centralised material systems, termed "individuals," the discontinuity of their inner life as viewed from outside, and the phenomenon of its growing external manifestation — have driven natural philosophers to form some explanation, or at least to venture upon a definition of this hidden principle, which shows itself in the highest forms of living matter, and which, though discontinuous to the external observer, acquires in the aggregate of human society a continuous and ever growing reality and development. Two dis- 50. The three facts impressed by psycho-physics.

legitimate. As the crystal to the mineralogist, the vibrating string to the student of acoustics, so also the animal, and even man, is to the physicist only a piece of matter. That the animal experiences pleasure and pain—that with the material life of the human frame are connected the joys and sorrows of a soul and the vivid intellectual life of a consciousness; this cannot change the animal and human body for the physical student into anything other than it is — a material complex subject to the unalterable laws which govern also the stone and the substance of the plant, a material complex whose external and internal movements are causally as rigidly connected amongst each other, and with the movements of the environment, as the working of a machine is with the revolution of its wheels (p. 4). . . . Thus the physiologist as physicist. But he stands behind the scene, and while he painfully examines the mechanism and the busy doings of the actors behind the drop-scenes, he misses the sense of the whole which the spectator easily recognises from the front. Could the physiologist not, for once, change his position?" (p. 5.

tinct views have been evolved by modern science on this matter.

The one emphasises the fact of the discontinuity of mental—*i.e.*, conscious—life, regards it as an ultimate fact, as a mystery beyond which we cannot travel. This idea presents itself in various forms, and has been notably insisted on—with very varying philosophical inferences—by Du Bois-Reymond in Germany, by Mr A. R. Wallace, and quite recently by the late Prof. St George Mivart in England.

The other takes refuge in the hypothesis of unconscious or subconscious mental life, and again with very different philosophical inferences assumes that all physical existence has an inner side which only under certain favourable conditions rises into the light of self-knowledge or consciousness. The late W. K. Clifford's "mind-stuff" theory, as also the speculations of Fechner and of Prof. Haeckel, are types of this view, which has been consistently and connectedly elaborated in Hartmann's 'Philosophy of the Unconscious.'

These speculations can be summed up under the title "The Creed of Science," and as such will occupy us later on in one of the chapters on the Philosophical Thought of the century.

By many natural philosophers it is felt that the time has not yet come to arrive scientifically at any definite conclusions on these last questions. Sufficient facts have

51. Transition to statistics.

not been collected; or even if collected, they have not yet been classified and tabulated. This is especially the case with the vast materials referring to the collective life of mankind. Leibniz had in his time foretold the

necessity of extensive statistical information before building theories. In one instance, that of language, his advice was followed with signal success.

But even some of the purely physical sciences, like meteorology, are still almost entirely limited to statistical information.

Statistics have thus become a very important department of knowledge, and before taking leave of the exact lines of thought, it will be well to note more precisely the part which these have played in our age, as also the methods by which they proceed. This will be the object of the next chapter, which will accordingly deal with the Statistical View of Nature.

CHAPTER XII.

ON THE STATISTICAL VIEW OF NATURE.

I HAVE now treated of the several grand and general aspects under which the objects of nature can be scientifically regarded, and have tried to show how these aspects, not unknown to former ages, have nevertheless, in the course of the nineteenth century, become more definite, and accordingly more useful, as means for describing, measuring, and, in many cases, predicting phenomena. It is true that the two last chapters, which dealt with the phenomena of Life and Mind, had to take notice of a principle or of principles which have hardly yet received any scientific definition at all, and which in the progress of the sciences which deal with them have played rather a negative part. It has been mainly by eliminating the conceptions of life and of mind as special agencies, factors, or entities that the scientific study of living and conscious beings has progressed; by showing more and more how an accurate and useful knowledge of much of their nature and behaviour can be gained with the aid of the methods adopted in other scientific inquiries, which we may call mechanical.

1\. Life and Mind as limiting conceptions.

Scientific inquiry in biology and psycho-physics has thus advanced on the lines indicated in the earlier chapters, where it was shown how several positive scientific conceptions have been gained, defined, and applied. These conceptions are all generalisations based upon definite observable facts of nature, such as attraction, atomic constitution, motion (rectilinear, periodic, and rotational), energy, form, and change of form,[1] and they have given rise to great branches of science, containing special methods of thought and reasoning. They have all shown themselves accessible, in a greater or less degree, to mathematical treatment, and have consequently been the means of introducing the exact scientific spirit into large fields of research, into ever

[1] The statement in the text is not strictly correct; for of the six definite conceptions mentioned we really, even in single cases, only see two exemplified—viz., motion and form. Neither attraction, nor the atom, nor energy, nor development is, even in single cases, observable, though, with the exception of energy, they are very early and very familiar abstractions. This remark may suggest that motion and form are, at least for the present, the simplest and most obvious conceptions into which we can analyse or resolve all external observations, and that consequently kinetics and morphology may be the fundamental sciences, the first in natural philosophy, the latter in natural history or biology in the widest sense. That a kinetic view will gradually supervene in natural philosophy is, I think, generally admitted. It seems less generally conceded that morphology will supervene in biology; especially as all the rage is just now for evolution and development. But as development must start from something, it is likely that it will lead back to morphology. As tending in this direction I read the expositions of Lotze, Claude Bernard, and the "Organicists." Organisation must mean a certain arrangement, and arrangement is ultimately the same as order, structure, or form. It may mean something more—viz., unity or centredness; but this is a conception not capable of a purely mechanical or geometrical definition; we know of it only through introspection. A great deal has been written on Morphology and Morphogenesis by that very suggestive author, Hans Driesch; see a list of his writings, *supra*, p. 456 note. I here only refer to them; for, being myself unable clearly to apprehend his main drift, I hesitate to quote him as confirming the argument of this note. The reader must judge for himself.

widening circles of phenomena and events. This has been most decidedly the case with the sciences in which the law or formula of gravitation has become the leading principle. As we advanced on the other lines of thought, marked by the conceptions of atomism, of the various forms of motion and of energy, this subjection to precise formulæ became less perfect, more complicated and hypothetical, whilst the study of the typical forms of natural objects, and even more of their genesis and developments, opened out a field for much conjecture and fanciful reasoning, amid which little more than the general outlines of a definite theory could be established. Lastly, in applying these various conceptions to the phenomena of the living and self-conscious creation, we have struck upon the limiting ideas of life and mind, of which, from a purely external point of view, little more can be said than that they indicate to us the existence among natural objects of a unity of a different kind from that which we can understand mechanically as the sum of many parts. In the higher forms this unity revealed itself to us through the analogy of our own inner life as a peculiar kind of centralisation, discontinuous when viewed from outside, but possessing, when viewed from another side, a continuity, connectedness, and capacity of unlimited growth of its own which is the special object of the psychological and historical sciences. These characteristics belong to the great realm of philosophical as distinguished from exact scientific thought.

2. Results of abstract science.

Before entering on this other great branch of our subject, we may well pause for a moment and cast a general and unbiassed glance at the world outside, leaving our study, our observatory, our laboratory, our dissecting- or our measuring-room, and ask ourselves the simple question, By the work carried on in these various secluded places, in the "sapientum templa serena," how much of the world outside have we really learnt to comprehend, or even only to describe and picture to ourselves correctly and completely? The answer is hardly encouraging. The first thing we notice in stepping out of our door is a phenomenon still as incalculable as it has ever been, and yet bound up with the enjoyment of our lives and the success of our work as much as ever—the weather. What do we know of it which is practically reliable and useful? The reply must be, "Next to nothing." Some general astronomical and some more detailed physical and chemical relations permit us to describe a few general meteorological and a few recurring seasonable events, but scarcely with more practical detail and certainty than the unscientific ancients or the untaught children of nature of to-day. We know in general the cause of storms, of changes of temperature, of the seasons, of rain, hail, drought, and cold, but we do not know much more of the exact when and where of these various changes than did our forefathers. The natural atmosphere and climate which surround us are still elements of conjecture and uncertainty.

Assume, however, that we go a step further, and having accustomed ourselves to take the weather, good or bad, as it is, enter into the artificial atmosphere and surroundings of practical life, of industry, trade, and

commerce, of politics and society, in which most of us have to spend the larger portion of the working hours of our existence. We can again put the question, What do we know with certainty of the changes and vicissitudes of this artificial atmosphere which surrounds us; what of the chances of a fall or rise in prices, of increased or lessened demand, of impending labour troubles, of the risks even of famine, fire, shipwreck, disease, or war? Again we may say that in general we know the proximate causes, natural or artificial, which may bring them about, but the exact when and where of their occurrence is so slightly known to us that such knowledge is of little, if of any, practical value, and proceeds, moreover, where it exists, more from general good sense and practical experience than from the discoveries of science. Indeed, the latter have, through the wonderful applications in the inventions of arts and crafts, tended to make our artificial atmosphere more complex, liable to more rapid and more drastic changes, and accordingly its features less permanent and less calculable and reliable.

3. Uncertainty in the concrete.

Thus, in spite of the wonderful increase of scientific knowledge and the general diffusion of scientific thought in the course of the century, uncertainty is still the main and dominant characteristic of our life in nature and society; the atmosphere and climate of each are as fickle and changeable, as incalculable and unreliable, as ever. Neither the great law of gravitation nor the fixed proportions of chemistry, neither the intricate doctrine of undulations nor the conception of energy, neither the knowledge of typical forms of nature nor that of their orderly evolution, has, in the hands of those who govern, regulate, and fashion the practical work of life and society, become an instrument of personal use and daily importance. Statesmen, legislators, organisers of men, captains of industry, contractors, practical engineers, colonisers, pioneers, and leaders of all kinds are still mostly ignorant of these scientific ideas. They regard them from a distance, themselves relying mainly on common-sense, on personal experience, or on the innate but indefinable impulses of individual genius; professional, scientific knowledge is only one, and hardly the most important, of the many agencies with which they deal and which they have to take into account.

4. Scientific spirit in business.

And yet, in spite of this fact that the ordinary routine of life is a very different process from the ways of science, we must admit that the scientific spirit very largely pervades the business of to-day. You cannot enter any commercial, shipping, or general trading office without being struck with the number of carefully prepared charts, tables, and statistical registers of all kinds of curves showing the rise and fall of prices, the production and consumption, the stocks and values of metals, coal, grain, chemicals, cotton, and produce of every kind; and in quite recent years, not only material things of all sorts, but the intangible thing called energy—after supplanting the older term horse-power—has become the subject of elaborate tabular and graphical registration. The streets of even the smaller towns in every civilised country show, besides the sign-boards of shops, offices, and banks, an increasing array of insurance firms, whose whole business depends on elaborate calculations, based on long tables of births, deaths, marriages, shipwrecks,

and other casualties. The daily newspapers bring us weather charts with isothermic, isobaric, and other lines, on which they found weather predictions or storm warnings. Surely, if counting, measuring, and calculating are the elementary processes of the scientific method, it must be admitted that the latter has permeated our practical life to an enormous extent. Thus the question can be asked, If the calculating spirit is so general, how does it come about that in its application to life and commerce it has led to so much grasp but to so little certainty; whereas in science itself it has led to so much actual and reliable knowledge? How does its application in practice differ from that in theory? The answer to this question is not far to seek, and it will introduce us to a special branch of science, to a special form of scientific thought which again is, if not a creation of the nineteenth century, yet one of its characteristic developments.

That which everywhere oppresses the practical man is the great number of things and events which pass ceaselessly before him, and the flow of which he cannot arrest. What he requires is the grasp of large numbers. The successful scientific explorer has always been the man who could single out some special thing for minute and detailed investigation, who could retire with one definite object, with one fixed problem into his study or laboratory and there fathom and unravel its intricacies, rising by induction or divination to some rapid generalisation which allowed him to establish what is termed a law or general aspect from which he could view the whole or a large part of nature. The scientific genius can "stay the moment fleeting"; he can say to the object

of his choice, "Ah, linger still, thou art so fair"; he can fix and keep the star in the focus of his telescope, or protect the delicate fibre and nerve of a decaying organism from succumbing to the rapid disintegration of organic change. The practical man cannot do this; he is always and everywhere met by the crowd of facts, by the relentlessly hurrying stream of events. What he requires is grasp of numbers, leaving to the professional man the knowledge of detail. Thus has arisen the science of large numbers or statistics,[1] and the many methods of which it is possessed. It will form the subject of the present chapter.

5. The science of large numbers.

[1] Gottfried Achenwall (1719-1772) is commonly termed the "father" of statistics. This, however, is hardly correct, either in relation to teaching or to the practical part of the subject, or even so far as the name is concerned. In connection with administration statistics existed in antiquity. They were taught by the celebrated professor, Conring, the elder contemporary and rival of Leibniz, and the name occurs in the seventeenth century in the 'Microscopium statisticum, quo status imperii Romano-Germanici repræsentatur auct. Heleno Politano' (1672). By Achenwall and his successor, Ludwig August Schlözer (1735-1809), statistics were treated in connection with history. The latter says, "Statistics are history standing still, and history is statistics put in motion." See on this subject, Wegele, 'Geschichte der deutschen Historiographie' (München, 1885), p. 793; also Roscher, 'Geschichte der National-Oekonomik' (ibid., 1874), p. 466. A very valuable and exhaustive account of the etymology and gradual change of meaning of the words "statist" and statistics will be found in Dr V. John, 'Geschichte der Statistik,' 1. Theil. (Stuttgart, 1884), pp. 3-14. He divides the history of the subject down to Quetelet into that of the "German University Statistics," following in the lines of Conring, Achenwall, and Schlözer, also called the "Göttingen School," and that of statistics as an exact, an enumerative science, which he calls the modern science of statistics. It appears that in English also the two meanings of the word are exemplified in the older use of the term "statist" by Shakespeare ("Hamlet," v. 2.; "Cymbeline," ii. 4.) and Webster, in which sense it meant simply "statesman"; and the modern title 'Statist,' for a statistical and financial periodical. Nor must we forget that England has in her 'Liber judiciarius seu censualis Willelmi I., regis Angliæ,' called 'Domesday-book' (1083-86), as David Hume says, "the most valuable piece of antiquity possessed by any nation" ('Hist. of England,' chap. iv.)

The grasp of large numbers, the methodical array of figures and the registration of events, would in itself be of little use were it not for a fundamental assumption which appeals to common-sense and has been confirmed by science, though it is hardly anywhere expressly stated —namely, the belief in a general order, in a recurrent regularity or a slow but continuous change and orderly development of the things and events of the world. Science, in the different aspects which we have so far passed in review, tries to give a definite expression to this general Order, to this all-pervading rule and regularity. Statistics and the practical use of them limit themselves to the bare fact that such order and regularity do exist, though the formula or reason for them may be unknown or unknowable. It may also be well to note that this belief in a general order is common to all schools of thought, be they ancient or modern, pagan or Christian, religious or scientific, optimist or pessimist. The dictum, "est modus in rebus," is the fundamental axiom of all thought and all practice; and the statistical view of nature, which merely puts into form and figure this general axiom or truism, has accordingly been appealed to as much by those who uphold a divine order of things as by others who insist on a natural or mechanical one. In the school of Quetelet, through whose influence statistical knowledge has been so greatly furthered in the course of our century, the regular recurrence of events and the stability of large numbers has been sometimes used as the basis for a fatalistic and pessimistic view, whereas nearly a hundred years before Quetelet, statistics had been elaborated by

6. Belief in general order.

the Pastor Süssmilch in Prussia, in a celebrated book bearing the title 'On the Divine Order,' with a tendency towards optimism, and as a proof of an overruling Providence.[1]

Although it is generally admitted by writers on statistics that in the narrower sense of the word they have existed ever since the existence of governments which required to know the number of their population, the natural resources of the country, and its means of subsistence or defence, there is a general opinion current that what we now call the statistical methods in science and in practice were introduced, or at least expressly recommended, by Lord Bacon under the name of the "Method of Instances." This method, which consisted in a kind of tabulating of numbers of facts referring to any special subject under investigation, has been criticised

7. Bacon's "Method of Instances."

[1] The difference seems to narrow itself down to this, that one class of writers refers everything to a physical, the other to a moral, order. M. Maurice Block, an eminent writer on statistics, discusses this question, passing a number of modern authors under review in the fifth chapter, § 3, of his excellent 'Traité théorique et pratique de Statistique' (2me éd., Paris, 1886). Referring to the theological statistician, A. von Oettingen, and comparing him with Quetelet, he says (p. 146): "Sous certains rapports, l'opinion de M. le professeur de théologie Alexandre d'Œttingen, pourra paraître l'opposée de celle de Quetelet, mais elle nous semble en différer beaucoup moins que le savant professeur ne le croit. . . . Nous pouvons caractériser en peu de mots ce que MM. d'Œttingen et Quetelet ont de commun et comment ils diffèrent: ils ont de commun le fond de la science; ils constatent l'un et l'autre la régularité du mouvement des faits; ils ne diffèrent que par l'interprétation: Quetelet voit des lois naturelles là où M. le professeur d'Œttingen voit des lois morales institutées par Dieu. Aussi l'un nomme-t-il son livre Physique sociale, et l'autre Éthique sociale. M. d'Œttingen est un croyant qui aime à s'appuyer sur la science. Il dit, page 13 de la première édition: 'Dans les sciences comme dans la religion, ce que l'homme invente ne peut être que faux, tandis que les vérités qu'il découvre, sont uniquement des faits ou des lois qui rayonnent du Créateur.'" The reconciliation of either physical or moral order with the existence of freewill is not a statistical but a philosophical problem.

by writers like Whewell, von Liebig, Stanley Jevons, and many others, and shown to be of very doubtful value; the example given by Bacon himself—the research into the nature of heat—being especially unfortunate and badly chosen. In spite of this, it is noteworthy that, up to quite recent times, the Baconian method is continually referred to, mainly by writers who are desirous of introducing what they call the exact methods of research into other sciences than those of external nature. A good example of this kind is given by Walter Bagehot, and as it serves to make an important point more intelligible than a general statement would, I will here give it in full. He speaks of the Enumerative, or, as he calls it, the "All-case method," and then continues: "A very able German writer[1] has said of a great economical topic—banking—'I venture to suggest that there is but one way of arriving at such knowledge and truth, namely, a thorough investigation of the facts of the case: by the facts I mean not merely such facts as present themselves to so-called practical men in the common routine of business, but the facts which a complete historical and statistical inquiry would develop. When such a work shall have been accomplished, German economists may boast of having restored the principle of banking—that is to say, of German banking, but not even then of banking in general. To set forth principles of banking in general, it will be necessary to master in the same way the facts of English, Scottish, French, and American banking—in short of every

[1] Prof. Cohn in 'Fortnightly Review,' Sept. 1873.

country where banking exists. . . . The only, but let us add also the safe, ground of hope for political economy, is following Bacon's exhortation to recommence afresh the whole work of economic inquiry. In what condition would chemistry, physics, geology, zoology be, and other branches of natural science which have yielded such prodigious results, if their students had been linked to their chains of deduction from the assumptions and speculations of the last century?'" To this Bagehot replies: "The method which Mr Cohn suggests was tried in physical science and failed. And it is very remarkable that he should not have remembered it as he speaks of Lord Bacon, for the method which he suggests is exactly that which Lord Bacon himself followed, and owing to the mistaken nature of which he discovered nothing. The investigation into the nature of heat in the 'Novum Organum' is exactly such a collection of facts as Mr Cohn suggests, but nothing comes of it. As Mr Jevons well says, Lord Bacon's notion of scientific method was that of a kind of scientific book-keeping. Facts were to be indiscriminately gathered from every source and posted in a kind of ledger, from which would emerge in time a clear balance of truth. It is difficult to imagine a less likely way of arriving at discoveries."[1]

[1] 'The Postulates of English Political Economy' (1885), p. 17, &c. He further remarks: "If we wait to reason till the 'facts' are complete, we shall wait till the human race has expired. I think that Mr Cohn, and those who think with him, are too 'bookish' in this matter. They mean by having all the 'facts' before them, having all the printed facts, all the statistical tables. But what has been said of nature is true of commerce. 'Nature,' says Sir Charles Lyell, 'has made it no part of her concern to provide a record of her operations for the use of men'; nor does trade either—only the

In fact, the eight chapters of this work which have dealt with the various abstract views from which natural phenomena have been considered in recent times, form an elaborate refutation of the so-called Baconian, of the enumerative or "all case," method. It was the light of the idea which brought life and order into the "rudis indigestaque moles" of badly collected facts, and in many cases even led for the first time to their useful and intelligent enumeration. But now we come to a further important question. Allowing that in certain large but nevertheless secluded spheres of science a few general ideas have been found to apply and work wonders of calculation, prediction, and useful application, how about those complicated phenomena which form our natural and social environment, and where so far no scientific formula has proved powerful or comprehensive enough? Are all these elaborate enumerations and graphical representations in meteorology, in sociology, commerce, industry, and finance, to which we have instinctively and increasingly had recourse during the whole of the century, of no value? Is no useful

smallest of fractions of actual transactions is set down so that investigation can use it. Literature has been called the 'fragment of fragments,' and in the same way statistics are the 'scrap of scraps.' In real life scarcely any one knows more than a small part of what his neighbour is doing, and he scarcely makes public any of that little, or of what he does himself. A complete record of commercial facts, or even of one kind of such facts, is the completest of dreams. You might as well hope for an entire record of human conversation." Stanley Jevons ('Principles of Science,' Preface, p. vii), says: "Within the last century a reaction has been setting in against the purely empirical procedure of Francis Bacon, and physicists have learnt to advocate the use of hypotheses. I take the extreme view of holding that Francis Bacon, although he correctly insisted upon constant reference to experience, had no correct notions as to the logical method by which, from particular facts, we educe laws of nature."

result to spring from them? Had they been conducted under the influence of no useful general idea, our answer would indeed have to be in the negative. But if, as practice shows, they have been of use, if, in fact, they prove to be in many cases quite indispensable, we may ask, What is the idea, the abstract thought, which dominates them? I will give the answer at once and then fix the aspect with which the present chapter has to deal. It is the conception and doctrine of averages.

8. General idea underlying enumeration.

9. Doctrine of averages.

Although to the general reader nothing may seem to be simpler than a process of counting and of registration, the science of statistics, the systematic collection of large numbers, and the fixing of averages, is comparatively young: it dates from the beginning of the seventeenth century, when Sully in France, followed by Richelieu and Colbert, had organised what may be called the first statistical bureau.[1] It emanated from the same spirit which called into existence the Paris Academy of Sciences. Characteristically for the two other nations with which we are mainly concerned in this history, the

[1] M. Block (*loc. cit.*, p. 25) says: "En France Sully avait déjà organisé, vers 1602, un *cabinet complet de politique et de finances*, qui peut être considéré comme le premier bureau de statistique. Les rapports que Sully demandait embrassaient l'armée, la marine, les finances et un grand nombre de branches de l'administration, et le résultat de ses investigations se trouve exposé dans l'ouvrage qui a été souvent réimprimé sous le titre de 'Mémoires de Sully.' Richelieu et Colbert se sont également fait adresser des rapports, auxquels on a puisé, dans ces derniers temps, bien des éléments utiles à l'histoire et que la statistique pourrait également utiliser." The Romans, who in antiquity may be regarded as the forerunners of the French in administrative ability and business-like conduct of State affairs, seem also to have developed an extensive system of registration. The question has been fully treated by the late Prof. Hildebrand of Jena in the 'Jahrbuch für Nationale Ökonomie und Statistik' (1866), in an article entitled "Die Amtliche Bevölkerungs-statistik im alten Rom."

10. Statistics in France, Germany, and England.

labour of statistics was taken up in Germany by the Universities, whereas in England it fell to the lot chiefly of a single person—the celebrated Sir William Petty, the creator of the term "Political Arithmetic." Thus, as in science generally, so in statistics, France marched ahead with her systematic and administrative genius; Germany followed in the person of Professor Conring,[1] who introduced the matter as a subject of university teaching; whilst Sir William Petty[2] wrote his essay with the practical object of disproving an opinion then much current in England, and which has periodically cropped up in the writings of journalists at home and abroad—the threatened decline of the English nation.

[1] Hermann Conring (1606-81), Professor of Medicine and Philosophy at Helmstädt, lectured on "Staatskunde, Notitia Rerum Publicarum," from about 1660.

[2] About the same time when lectures on "The Science of the State" were begun in Germany by Conring, Sir William Petty (1623-87) in England, one of the founders of the Royal Society, occupied himself for practical reasons with similar subjects, collecting his views in a tract called 'Political Arithmetic' about the year 1677, besides contributing various papers to the 'Philosophical Transactions' and publishing several 'Essays' (1681-86). The 'Political Arithmetic' would have been printed, but for the French policy of Charles II., to whom it was presented in manuscript. It was not published till 1690, after the author's death, on a permission "given at the Court of Whitehall on the seventh day of November," by Lord Shelburne, the son of the author. In the preface, he characteristically says: "I have thought fit to examine the following Persuasions; which I find too current in the world, and too much to have affected the minds of some, to the prejudice of all—viz., That *the rents of land are generally fallen;* that therefore, and for many other reasons, *the whole kingdom grows every day poorer and poorer.* That formerly it abounded with gold; but now, *there is a great scarcity, both of gold and silver.* That *there is no trade, nor employment for the people;* and yet that *the land is under-peopled.* That *taxes have been many and great.* That *Ireland and the Plantations in America, and other additions to the Crown, are a burden to England.* That *Scotland is of no advantage.* That trade, *in general, doth lamentably decay.* That *the Hollanders are at our heels, in the race for naval power; the French grow too fast upon both; and appear so rich and potent, that it is but their clemency that they do not devour their neighbours.*"

And as in science, so also in statistics, Germany in time followed the example of France by introducing organisations similar to that of the "Cabinet complet de politique et de finances" of Sully. It was notably during the reign of Frederick the Great that the population statistics were regularly and systematically collected in Prussia, this enterprise being greatly stimulated by the publication of J. P. Süssmilch's[1] 'Treatise on the Divine Order.' In England—with a notable exception to be mentioned immediately—the line of research opened out by Sir William Petty was not followed up, and MacCulloch, when publishing, at the beginning of our cen-

[1] Johann Peter Süssmilch (1707-67) published, in the year 1741, a book with the following title: 'Die göttliche Ordnung in den Veränderungen des menschlichen Geschlechts, aus der Geburt, dem Tode und der Fortpflanzung desselben erwiesen von Johann Peter Süssmilch, Prediger beym hochlöblichen Kalcksteinischen Regiment. Nebst einer Vorrede Herrn Christian Wolffens.' The book, as well as the author, was for a long time but little appreciated; for although the former was dedicated to Frederick the Great, and must presumably, to judge from the several editions which appeared, have been made use of in the statistical labours of the Prussian administration, the author, not having been connected with any university, had, for a long time, little influence on the so-called "university school" of statistics. In the course of the last fifty years, all prominent writers on statistics, such as Wappäus, Roscher, von Oettingen, Knapp, and V. John, in Germany, M. Block and others in France, as also Italian writers on statistics, have taken increased interest in the book. Dr V. John ('Geschichte der Statistik,' vol. i. p. 241, &c.) gives an exhaustive analysis of the work. He calls the author "the first statistician in the modern sense," the precursor of Quetelet, and says, moreover, "It is easily explained how the philosopher Süssmilch would vanish into the background as soon as the conception of the encyclopædists, that only matter in motion exists and no mind, came to be generally accepted, and that the politician Süssmilch should utterly disappear in the turmoil of the French Revolution." Von Oettingen, who, on the other side, agrees in accepting with Süssmilch the existence of a Divine or moral order, says of the latter, that "he has become, through his magnificent labours, the founder of the science which we now call moral statistics," inasmuch as he, "for the first time, recognised the intrinsic regularity in the apparently most accidental human phenomena and actions, and tried to establish it by inductive methods" ('Moralstatistik,' 3rd ed., 1882, p. 21). That he was known to Herder and appreciated by him, we saw *supra*, p. 536 note.

tury, his 'Statistical Account of the British Empire,' had hardly any similar work to refer to during the whole of the eighteenth century.

The exception just referred to was "The Tables of Mortality," which date back to the middle of the sixteenth century, and in a more regular form to 1603.

11. John Graunt and Halley.

They were analysed by John Graunt, captain, in 1661, in a tract with the title 'Natural and Political Observations upon the Bills of Mortality.'[1] Of Graunt's[2] work, M. Maurice Block says that the difficulties of preparing such a table at that time were so great that it might wellnigh be considered a performance of genius. The invention once made, improvement

[1] The tract was presented to the Royal Society in 1662, and printed by order of the latter in 1665, the author becoming a fellow at the request of the king. V. John gives a full account of the book, and as much of the author as he could collect from the scanty records of him which exist (*loc. cit.*, pp. 161-178). He was born in 1620, was a man of business, and latterly became connected with the Gresham College and with sundry matters pertaining to the administration of the City. He died in 1674. In 1676 a new, sixth, edition of the tract was published by Sir W. Petty, whom both Halley and Evelyn erroneously referred to as the author.

[2] 'Statistique,' p. 194. Süssmilch, a century after Graunt, says that the material for the determination of the 'Divine Order' existed in the parish registers since the time of the Reformation. "But who," he exclaims, "made use of it for this purpose before Graunt? The discovery was just as easy as that of America, but the Columbus was lacking" (quoted by V. John, *loc. cit.*, p. 177). The author, however, who suggested to Süssmilch the researches which led to the celebrated 'Divine Order,' was not John Graunt, but Dr William Derham (1657-1735), an eminent divine and natural philosopher, who published in 1713 his 'Physico-Theology; or a Demonstration of the Being and Attributes of God from His Works of Creation,' a book which ran through six editions in ten years, being translated into French and several times into German. This book contained, as Süssmilch himself says, besides numerous notes, a collection of the observations of other English authors on the lists of births, deaths, and marriages. On following up the clue given by it he arrived ultimately at Graunt and Petty, of whom the former had, as he says, broken the ice, whereas Petty had mainly discussed the influence of the changes of population in politics (V. John, 'Statistik,' p. 243).

was easy; the invention was the difficulty. The next great name connected with this subject was the astronomer and mathematician Edmund Halley,[1] who had before him, in addition to John Graunt's work, the figures of birth and mortality during the five years 1686 to 1691 collected by Kaspar Neumann for the city of Breslau, capital of the province of Silesia. Tables of mortality, based upon several thousands of life annuities, were prepared in Holland by order of the Grand Pensioner, John de Witt, and used in 1671 as the basis for a loan in the form of annuities.[2] The growing practice of life insurance, as is well known, attaches a great interest to these tables of mortality, which have been slowly perfected in the course of the last hundred and fifty years; it having been reserved for the labours

[1] For a long time is was not known how Halley came into possession of Kaspar Neumann's mortality-tables; but, in recent times, mainly through examination of the local records of the city of Breslau by Bergius and others, and notably by the aid of S. Grätzer ('Edmund Halley und Kaspar Neumann,' Breslau, 1883), it has become almost certain that Neumann's registers were communicated to the Royal Society by no less a person than Leibniz, who corresponded with Neumann on the one side as well as with the secretaries of the Royal Society on the other. Some of the original documents have been traced in the archives of the Society by Dr Bond and Prof. Burdon Sanderson. It is well known that Leibniz himself attached great importance to accurate statistical knowledge of all kinds, and considered the collection of such to be one of the main duties of the various academies which he planned or founded.

[2] "Le grand pensionnaire de Hollande, Jean de Witt, se fondant sur les calculs de probabilités enseignés par Chrétien Huygens, se servit, comme éléments d'observation, des résultats constatés sur quelques milliers de rentiers viagers. Il présenta sa table aux états généraux le 25 avril 1671, pour servir de base à un emprunt fait sous la forme d'annuités viagères. Cette table citée par M. de Baumhauer, se trouve dans les registres des états de Hollande, année 1671" (Block, *loc. cit.*, p. 196). A translation of this document appeared in 'Contributions to the History of Insurance' by F. Hendriks, 'Ass. Mag.,' vol. ii., 1852.

12. Probability, Co-operation, Equitable Distribution.

of quite recent writers[1] to place the whole matter upon a thoroughly scientific basis. But it is not these necessary technical refinements that interest us most at present; rather let us take note how the needs of governments, as well as the uncertainty and risks of life, have automatically led to the definition and study of three distinct statistical conceptions, which in our age govern a very large part of all our practical enterprises. These three conceptions are the probability of future events based upon long series of past
1
experiences, the idea of reducing or averaging risks
2
by "amicable" co-operation, and the "equitable" dis-
3
tribution of the burdens of such co-operation according to the individual units who co-operate.[2] It will at

[1] It is generally admitted that Prof. G. F. Knapp created a kind of era in the more rigorous mathematical treatment of the subject by his various publications, dating from the year 1868 with his tract 'Ueber die Ermittelung der Sterblichkeit aus den Aufzeichnungen der Bevölkerungs-statistik.' M. Block (*loc. cit.*, p. 232) says: "Ce livre a fait une véritable sensation parmi les hommes spéciaux; non que l'auteur ait apporté beaucoup de nouvelles pierres à l'édifice, mais il a donné à ces pierres une ordonnance, une disposition qui les constituent un monument." In the year 1874 he published his 'Theorie des Bevölkerungswechsels.' Many other writers have followed in the new track, among whom I will only mention Becker, Zeuner, and Lexis. The graphical method is largely employed by these authors, amongst whom Zeuner resorts to a representation in three dimensions with some very elegant results. See his 'Abhandlungen zur mathematischen Statistik' (Leipzig, 1869). A historical and critical review of these and older writings is given in the last-named work of Knapp, p. 53, &c. See also Prof. Lexis's 'Einleitung in die Theorie der Bevölkerungs-statistik' (Strasburg, 1875).

[2] This is not the place to discuss the social and moral aspects of co-operation, which by future historians will possibly be looked upon as one of the very few novel political ideas which our century has evolved or at least elaborated in a practical form; the older co-operative attempts, such as were made under the influence of the great Revolution by Fourier, Saint Simon, and Babeuf in France, and by Robert Owen in this country, not having contained the elements of permanent success. These elements seem to belong almost exclusively to the line of development started by the "Rochdale Pioneers."

once be seen how all arrangements which are based upon these three conceptions — viz., probability, co-operation, and equitable distribution — lead us away from the study of individual cases to that of totals and averages; how they merge the interests of single persons and the peculiarities of single cases in those of the aggregate of a large number and the properties of the average event or the "mean" man. Their value and success depend on the consideration and participation of large numbers, and they have accordingly only arisen during the latter days which have witnessed the steady growth of modern populations and the bewildering complication of modern business. The moral or social aspect which has simultaneously been evolved during our period does not for the moment concern us. We are concerned at present only with the fact that statistics as the science of large numbers and of averages has been increasingly drawn into use. In fact, we might call our century —in distinction from former centuries—the statistical century.

The necessity of having recourse to elaborate countings, to registrations of births, deaths, and marriages, to lists of exports and imports, to records of consumption and production of food-stuffs and many other items, forced upon those who were entrusted with the gathering and using of these data the observation that all such knowledge is incomplete and inaccurate. Owing to the variability, within certain limits, of recurring events and the errors of counting and registration, we have to content ourselves always with approximation instead of certainty. Error bulks

very largely in all statistics, and vitiates them; and as regards coming events, our minds are in a state of expectation rather than of assurance. But events can be more or less probable, errors can be greater or smaller, cumulative or compensatory, and our expectations may be well- or ill-founded. And so there has arisen the

13. The science of Chances.

science of Probabilities and of Chances, and the Theory of Error, two subjects intimately interwoven. The former arose in the seventeenth century out of the frivolous or vicious practice of betting and gambling,[1] whilst the latter was founded when astronomical observations accumulated, and the question presented itself how to combine them so as to arrive at the most reliable result. The greatest mathematicians and philosophers, such as Pascal, Huygens, and Leibniz, the Bernoullis, De Moivre, Laplace, Gauss and Poisson, have bestowed much thought on the subject,[2] which has nevertheless been very differently judged—praised beyond measure by some, and ridiculed by others; sometimes pronounced to be merely common-sense put in figures, and then again wrapped up

[1] See *supra*, vol. i. p. 120 *sqq.*

[2] In addition to the references given in vol. i., the following are of importance. The history of the Theory of Probabilities, as stated above, has been written by Isaac Todhunter. This history brings the subject down to the writings of Laplace, whose two works mentioned in the text still remain the two standard works on the science. In quite recent times the history has been written and brought up to date by Prof. Emanuel Czuber in his 'Entwickelung der Wahrscheinlichkeits-Theorie und ihre Anwendungen,' contained in the seventh volume of the 'Jahresbericht der Deutschen Mathematiker Vereinigung' (Leipzig, 1899). The latter work is written on a different principle from that of Todhunter. Whereas Todhunter deals in separate chapters with the work of the foremost mathematicians on this subject, Prof. Czuber gives an independent historical and critical analysis of the different developments of the theory and its applications. Quite recently the same author has published an independent treatise on the subject (Leipzig, 1902).

in appalling mystery.[1] There is, however, no doubt that the Theory of Probability increasingly pervades scientific as well as statistical work in our age, and that in the

[1] In spite of the encomium on the theory of probabilities quoted in vol. i. p. 123, Sir John Herschel gave only a qualified adherence to one of its principal applications (see 'Brit. Assoc. Rep.,' vol. i. p. 165). The two foremost adverse critics of the theory were Auguste Comte in France and John Stuart Mill in England. In the second volume of the 'Philosophie Positive' (1st ed., 1835, p. 371) the former explains why he omitted to deal with so important a subject in his mathematical philosophy. "Le calcul des probabilités ne me semble avoir été réellement, pour ses illustres inventeurs, qu'un texte commode à d'ingénieux et difficiles problèmes numériques, qui n'en conservent pas moins toute leur valeur abstraite, comme les théories analytiques dont il a été ensuite l'occasion, ou, si l'on veut, l'origine. Quant à la conception philosophique sur laquelle repose une telle doctrine, je la crois radicalement fausse et susceptible de conduire aux plus absurdes conséquences. Je ne parle pas seulement de l'application évidemment illusoire qu'on a souvent tenté d'en faire au prétendu perfectionnement des sciences sociales: ces essais, nécessairement chimériques, seront caractérisés dans la dernière partie de cet ouvrage": and in the fourth volume (1839, p. 512), "La seule aberration de ce genre . . . c'est la vaine prétention d'un grand nombre de géomètres a rendre positives les études sociales d'après une subordination chimérique à l'illusoire théorie mathématique des chances. . . . Quelque grossière que soit évidemment une telle illusion, elle était néanmoins essentiellement excusable, quand l'esprit éminemment philosophique de l'illustre Jacques Bernoulli conçut, le premier, cette pensée générale, dont la production, à une telle époque, constituait réellement le précieux et irrécusable symptôme du besoin prématuré pour ce temps, mais qui n'y pouvait être éprouvé même ainsi que par une intelligence vraiment supérieure." John Stuart Mill, in the second volume of his 'Logic,' has devoted a whole chapter to the subject, in which he corrects a statement made by him in the first edition of his book, attributing a "fundamental fallacy" to the arguments of Laplace and other mathematicians, but nevertheless takes an unfavourable view of the usefulness of the calculus. In more recent times the subject has been exhaustively treated from a logical point of view by Mr John Venn in his work, 'The Logic of Chance' (3rd ed., London, 1888), and by Stanley Jevons in 'The Principles of Science' (vol. i. ch. x.) The doubts with which Mill, and still more Comte, regarded the subject, seem to have been dispelled in works on Logic; and the increasing use to which the methods for the correction of error have been put in many branches of science have convinced mathematicians of its applicability. The ninth edition of the 'Ency. Brit.' contains an excellent article on "Probabilities" by M. W. Crofton. Among the clearest and safest guides in this intricate subject must be counted the late Prof. Augustus de Morgan, whose profound treatise in the 'Ency. Metrop.' (vol. ii.), as well as his 'Essay on Probabilities' (London,

course of the last hundred years much has been done to make it more easily understood.

James Bernoulli had already in his celebrated book which bears the title, 'De arte conjectandi,' promised to show the application of the mathematical doctrine of probability to political, moral, and economical subjects,[1] but the fourth and last part of the book which was to give this, remained unfinished. It was left to his successors, notably to Daniel Bernoulli, to take up this side of the question. But the first practical states-

14. Condorcet. man who — as we are told by Condorcet[2]—held the

1838), still rank with the best that has been written. Stanley Jevons sums up his opinion in the words: "This theory appears to me the noblest creation of the human intellect, and it passes my conception how two men possessing such high intelligence as Auguste Comte and J. S. Mill could have been found depreciating it, or even vainly attempting to question its validity. To eulogise the theory is as needless as to eulogise reason itself" ('Principles of Science,' vol. i. p. 227).

[1] James Bernoulli (1654 - 1705) was the eldest of the celebrated family of mathematicians. Daniel, his nephew, lived half a century later (1700-82). The 'Ars Conjectandi' was published posthumously in 1713 by Nicholas, another nephew of the author. In a letter to Leibniz the author says: "Absolvi jam maximam libri partem, sed deest adhuc præcipua, qua artis conjectandi principia etiam ad civilia, moralia, et œconomica applicare doceo." Daniel Bernoulli, as we saw above (vol. i., chap. v. p. 434), was the father of the kinetic theory of gases, of which more hereafter. He was also the first to make a distinction between mathematical and moral expectation, — a difference which led Laplace to distinguish between "fortune physique" and "fortune morale," to which reference was made in connection with Fechner's psycho-physical measurements.

[2] 'Essai sur l'application de l'Analyse à la Probabilité des Décisions, Rendues à la pluralité des voix' (Paris, 1785): "Un grand homme, dont je regretterai toujours les leçons, les exemples, et surtout l'amitié, était persuadé que les vérités des sciences morales et politiques, sont susceptibles de la même certitude que celles qui forment le système des sciences physiques, et même que les branches de ces sciences qui, comme l'astronomie, paroissent approcher de la certitude mathématique. Cette opinion lui était chère, parce qu'elle conduit à l'espérance consolante que l'espèce humaine fera nécessairement des progrès vers le bonheur et la perfection, comme elle en a fait dans la connoissance de la vérité." It is evident from this extract that Condorcet (1743-94) thought that his friend Turgot shared his own well-known opinions as to the unlimited perfectibility of the human race.

view that morals and politics might derive the same benefit from the science of calculation as the physical sciences had already experienced, seems to have been Turgot. To show the importance of this view, Condorcet wrote his much quoted but little read essay on the application of analysis to decisions based on the plurality of votes. In his Infroduction the author laments that his friend, on whose suggestions he had commenced his work, did not live to see it finished.[1] It would have been interesting to know whether so eminent a practical philosopher as Turgot is considered to have been, would have been encouraged by his friend's specimen of political algebra, or whether he would have held the opinion of Mill, who saw in these "applications of the calculus of probabilities . . . the real opprobrium of mathematics."[2]

[1] (*Loc. cit.*, p. i.) "Si l'humanité n'eût pas eu le malheur, longtemps irréparable, de le perdre trop tôt, cet ouvrage eût été moins imparfait: éclairé par ses conseils, j'aurois vu mieux ou plus loin, et j'aurois avancé avec plus de confiance des principes qui auroient été les siens. Privé d'un tel guide, il ne me reste qu'à faire à sa mémoire l'hommage de mon travail, en faisant tous mes efforts pour le rendre moins indigne de l'amitié dont il m'honoroit."

[2] There is no doubt that the writings of Condorcet, through the useless accumulation of formulæ with very little substance behind them, contributed to bring the whole theory into discredit. Another still more eminent contemporary mathematician, D'Alembert, after having occupied himself at considerable length with problems in probabilities, formed an unfavourable opinion of the usefulness of the calculus. Gouraud (quoted by Todhunter, p. 293) says: "Quant au reste des mathématiciens, ce ne fut que par le silence ou le dédaint qu'il répondit aux doutes que d'Alembert s'était permis d'émettre. Mépris injuste et malhabile où tout le monde avait à perdre et qu'une postérité moins prévenue ne devait point sanctionner." It is interesting to note that Laplace, in his historical account at the end of his 'Essai Philosophique,' does not refer either to Condorcet or to D'Alembert. J. S. Mill ('Logic,' vol. ii. p. 66) says: "It is obvious, too, that even when the probabilities are derived from observation and experiment, a very slight improvement in the data, by better observations, or by taking into fuller consideration the special circumstances of the case, is of more use than the most elaborate application of the calculus to probabil-

15. Laplace.

So far as the formal part of the subject was concerned, it was left to Laplace to place it on the foundation upon which it has ever since rested. He brought together the ideas of his predecessors, notably of De Moivre, the two Bernoullis, Stirling, Bayes, and Lagrange, as well as his own extensive researches, in his great analytical theory of Probability, which appeared in 1812, and, with several editions and an elaborate introduction, in two subsequent editions during his lifetime. This work has been justly considered a monument of human genius, and stands worthily beside the great 'Mécanique Céleste' of its author. The

ities founded on the data in their previous state of inferiority. The neglect of this obvious reflection has given rise to misapplications of the calculus of probabilities which have made it the real opprobrium of mathematics. It is sufficient to refer to the applications made of it to the credibility of witnesses, and to the correctness of the verdicts of juries." I have already referred to the position which Comte took up. De Morgan, with his usual clearness and wisdom, at the end of his "Theory of Probabilities" ('Ency. Metrop.,' vol. ii. p. 470), whilst reducing to a very narrow province these applications of the calculus of probabilities, says: "There are circumstances connected with the mathematical theory of independent evidence which it may be useful to examine. In this, as in several other preceding investigations, it is not so much our wish to deduce and impose results, as to inquire whether these results really coincide with the methods of judging which our reason, unassisted by exact comparison, has already made us adopt. The use of the process is, that both our theory and our preconceptions thus either assist or destroy each other: in the former case we feel able to trust this science for *further directions;* in the latter, a useful new inquiry is opened. For when we consider the very imposing character of the first principles of the science of probabilities, and the mathematical necessity which connects those simple first principles with their results, we feel convinced that, even on the supposition that the main conclusions of the present treatise are altogether fallacious, there must arise a necessity for investigating the reason why a *methodical* treatment of certain notions should lead to results inconsistent with the *vague* application of them on which we are accustomed to rely. For it must not be imagined that opposition to the principles laid down in this treatise is always conducted on other principles: on the contrary, it frequently happens that it is only a result of themselves obtained without calculation, which is arrayed against arithemetical deduction."

labours of mathematicians since Laplace in the field of probabilities have consisted mainly in commentaries on and simplifications of his expositions, and in a great improvement in the formal methods, due mostly to English workers.[1] At present we are not interested in the purely mathematical side of the subject, which for some minds has a great fascination, but rather in the question: To what extent have the anticipations of such men as Condorcet, Turgot, and Laplace, as to the practical value of these researches, been realised? in how far have they proved to be "the happiest supplement to the ignorance and weakness of the human mind"?[2] This idea, though ridiculed by some, has as often cropped

[1] The problems suggested by the calculus of probabilities gave rise, collaterally, to several important mathematical developments, notably the combinatorial analysis, the calculus of finite differences, and, in the hands of Laplace, the theory of generating function and the recurrent series. A large part of Laplace's great work is taken up with this purely mathematical device. It has in more recent times been supplanted, especially under the hands of English mathematicians, by the calculus of operations, of which the germ is to be found, according to Laplace, in a suggestion of Leibniz (see 'Essai Philosophique sur les Probabilités,' p. 65).

[2] "La théorie des probabilités n'est, au fond, que le bon sens réduit au calcul : elle fait apprécier avec exactitude ce que les esprits justes sentent par une sorte d'instinct, sans qu'ils puissent souvent s'en rendre compte. Elle ne laisse rien d'arbitraire dans le choix des opinions et des partis à prendre, toutes les fois que l'on peut, à son moyen, déterminer le choix le plus avantageux. Par là, elle devient le supplément le plus heureux à l'ignorance et à la faiblesse de l'esprit humain. Si l'on considère les méthodes analytiques auxquelles cette théorie a donné naissance, la vérité des principes qui lui servent de base, la logique fine et délicate qu'exige leur emploi dans la solution des problèmes, les établissemens d'utilité publique qui s'appuient sur elle, et l'extension qu'elle a reçue et qu'elle peut recevoir encore, par son application aux questions les plus importantes de la Philosophie naturelle et des sciences morales ; si l'on observe ensuite que dans les choses mêmes qui ne peuvent être soumises au calcul, elle donne les aperçus les plus sûrs qui puissent nous guider dans nos jugemens, et qu'elle apprend à se garantir des illusions qui souvent nous égarent, on verra qu'il n'est point de science plus digne de nos méditations, et qu'il soit plus utile de faire entrer dans le système de l'instruction publique" (*loc. cit.*, p. 273 *et seq.*)

up again in the course of the century, and is at present occupying the attention of distinguished thinkers. It will be interesting to give some account of these practical applications.

16. Four applications.

Of these, four notably attract our attention. First, the theory of error, prominently associated with the name of Gauss. Secondly, the writings of Adolphe Quetelet, and the great impetus given by him to statistical research. Thirdly, the peculiar development of the Atomic theory known as the Kinetic theory of gases, which gave to many scientific investigations what Clerk-Maxwell termed the statistical, in opposition to the historical or descriptive, character. Lastly, the Darwinian ideas which deal with the great and increasing numbers of living things, and the changes inherent in their growth and development. These have led to statistical enumerations and registrations which, beginning with Mr Francis Galton's researches into the phenomena of heredity, are at the present moment being continued on special lines by Prof. Karl Pearson.

17. Theory of Error.

That Error is subject to law, or, to express it mathematically, to regularity, is a reflection which forced itself upon the attention of thinkers who occupied themselves with the doctrine of chances, and of statisticians who collected registers of large numbers of events. Let special known sources of error be eliminated or allowed for in every instance, there still remains a very large, practically an infinite, number of unknown sources of error which—where we have to do with simple magnitude—may increase or reduce our result by mutually

destroying or augmenting each other. The repeated measurement of a physical quantity, of the position of a fixed star; the arrangement of the bullet marks on a target; the grouping of the impressions made on the sand by a stone let fall vertically from the same point at a considerable height; even the countings by a large number of skilled persons of the same number or the estimates of the same distance or height of an object, of the weight of a heap of materials: all these statements will show a certain regularity around the mean number which we consider to be the most probable or correct one. Small errors will be more frequent than large ones; very large ones will be practically absent; and the mean will be the result of a mutual destruction or compensation of many small sources of error acting both ways. Mathematicians, from the time of Lagrange and Bernoulli, have tried to put into a mathematical formula this regularity in the distribution of error; and, since Laplace and Gauss approached the subject from different points of view, they have arrived at a definite analytical expression[1] for the distribution of errors of increasing magnitude around a fictitious centre or mean which is considered in every instance to be the most probable quantity. Practical trials on a very large scale have been made by Bessel, Encke, Quetelet, Faye, and others, and they have in every case yielded a satisfactory approximation to the figure given by the theoretical formula; so that at present little doubt as to its usefulness exists in the minds of those who employ it for the purposes of

[1] This is the well-known "curve of Error."

elaborate calculations in astronomy, geodesy, and in various physical and statistical researches.

18. Method of Least Squares. Gauss.

Bound up with the theory of Error is the celebrated method of least Squares, first used by Gauss in 1795, published by Legendre in 1805 in his memoir 'On a New Method of Determining the Orbit of a Comet,' and elaborately discussed by Laplace, Gauss, and many subsequent writers to this day.[1] It may be looked upon as an extension or generalisation of the common-sense

[1] In addition to the references given in the notes to pp. 120 and 183 of vol. i., I can now recommend two excellent summary accounts of the history and theory of the method of least squares—the one in Prof. Czuber's 'Bericht,' quoted above (pp. 150 to 224); the other in Prof. Edgeworth's article on "The Law of Error" in the Supplement to the last edition of the 'Ency. Brit.' (vol. xxviii., 1902, p. 280, &c.) Prof. Cleveland Abbe, in a "historical note on the method of least squares" ('American Journal of Mathematics,' 1871), has drawn attention to the fact, that already in 1808 Prof. R. Adrain of New Brunswick had arrived at an expression for the law of error identical with the formula now generally accepted, without knowing of Gauss's and Legendre's researches. See a paper by Prof. Glaisher in the 39th vol., p. 75, of the 'Transactions of the Royal Astronomical Society.' The logical and mathematical assumptions upon which the method is based have been submitted to repeated and very searching criticisms, many rigid proofs having been attempted, and every subsequent writer having, seemingly, succeeded in discovering flaws in the logic of his predecessors. In connection with another subject, I may have occasion to point out how nearly all complicated logical arguments have shown similar weakness, and how, in many cases, the conviction of the correctness or usefulness of the argument comes back to the self-evidence of some common-sense assumption, which cannot be proved, though it may be universally accepted. Many analysts have tried to prove the correctness of the everyday process of taking the arithmetical mean, but have failed. Prof. Czuber says, *inter alia* (*loc. cit.*, p. 159): "The fact that Gauss, in his first demonstration of the method of least squares, conceded to the arithmetical mean a definite theoretical value, has been the occasion for a long series of investigations concerning the subject, which frequently showed the great acumen of their authors. The purpose aimed at—viz., to show that the arithmetical mean is the only result which ought to be selected as possessing cogent necessity, hereby giving a firm support to the intended proofs, has not been attained, because it cannot be attained. Nevertheless, these investigations have their worth because they afford clear insight into the nature of all average values and into the position which the arithmetical average occupies among them."

method of taking the arithmetical mean in determining what figure to accept in a number of slightly differing computations. Where more than one quantity is to be determined—for instance, where from a series of observations dotted on a chart the continuous curve which marks the course of a planet or comet is to be deduced—the simple method of averaging cannot be applied. Every set of three complete observations suffices, as Gauss has shown, to determine the elements or constants of an elliptical orbit. But astronomers try to get as many observations as possible, and none of these is a repetition of the same observation—as, for instance, are the repeated weighings of a substance in chemistry, of the measurings of a length in surveying, or the counting of a number in statistics: on the contrary, each is the independent ascertainment of definite positions in a moving object. It is clear that the method of averaging must be more general than the common-sense method of taking the arithmetical mean, but must — where the latter is applicable — coincide with it. It has been shown that the following rule answers this purpose. Fix the average constants or elements so that the sum of the squares of the differences between the observed and calculated positions is a minimum. In mathematical language this results in the algebraical determination of the constants in an equation.

Whereas the labours of Gauss and the school of astronomers which he headed in Germany were mostly occupied in the mathematical proof of this rule, and in its applications in astronomical and geodetic computations, the doctrine of probabilities acquired a larger

meaning and attracted much popular attention in France
19. Laplace. and Belgium through the dominating influence of Laplace. He had not only collected in his abstract and very difficult 'Analytical Theory of Probabilities' all that himself and others had done in this line of research, but he had in a similar manner to that adopted in his 'Celestial Mechanics' tried to bring the substance of the theory home to the non-mathematical student in his 'Essai Philosophique sur les Probabilités.'

The analytical formulæ of probabilities can, he maintained, "be regarded as the necessary complement of the sciences which are founded on a mass of observations which are subject to error. They are indeed indispensable for solving a large number of questions in the natural and moral sciences. The regular causes of events are mostly either unknown or too complicated to be submitted to calculation: frequently also their effect is disturbed by accidental and irregular causes, but it always remains impressed on the events produced by all these causes, and it brings about changes which a long series of observations can determine. The analysis of probabilities shows these modifications: it assigns the probability of their causes, and it indicates the means of increasing their probability more and more."[1] Then, referring to the phenomena of the weather, Laplace proceeds: "Moreover, the succession of historical events similarly shows us the constant action of the great moral principles in the midst of the diverse passions and interests which agitate society in every direction. It is remarkable how a science

[1] 'Essai Philosophique,' p. 271.

which began with the consideration of play has risen to the most important objects of human knowledge."

In 1823, soon after the appearance of the works of Laplace and other French writers, this application of the theory of probabilities was taken up by Adolphe Quetelet, who collected his researches in his celebrated work, 'Sur l'Homme et le Développement de ses Facultés, ou Essai de Physique sociale.'[1] Quetelet 20. Quetelet.

[1] In addition to this work, which was published at Brussels in 1836 in two small volumes, and which Quetelet (1796-1874) describes as a 'résumé de tous mes travaux antérieurs sur la statistique,' he published, besides a great number of memoirs, a series of 'Lettres sur la Théorie des Probabilités' (begun in 1837, pub. 1845, Eng. trans. by O. G. Downes, 1849), and as a continuation of the former work in 1848, 'Du Système social et des Lois qui le régissent.' Less known than those of Quetelet, but about the same time, and independently, there appeared in France the writings of A. M. Guerry, beginning with the publication in 1829—in collaboration with A. Balbi—of 'Statistique comparée, et l'état de l'instruction et du nombre des crimes,' and in 1833, 'Essai sur la statistique morale de la France.' The term "moral statistics" appears here for the first time. Quetelet was the inventor of the term "Social Physics." Guerry employed graphical methods, and published in 1864 'Statistique morale de l'Angleterre comparée avec la statistique morale de la France.' M. Block ('Statistique,' p. 43) attributes to Guerry and Charles Dupin the general introduction of the graphical method in statistics; geometrical representation having been adopted at the end of the eighteenth century by Wm. Playfair in England, and, before him, by Crome, professor at Giessen, in 1782, and tabular synoptical statements going back to the Danish writer J. P. Anchersen, in his 'Descriptio Statuum Cultiorum in Tabulis' (Copenhagen and Leipzig, 1741); see V. John, 'Geschichte der Statistik,' p. 88. Referring to Guerry, V. John (p. 367) says: "Quetelet is incontestably to be regarded as the founder of the new science (viz., moral statistics), for the rival works of the French lawyer Guerry appeared only partly before Quetelet's, and are excelled by the latter in the use made of the material. Independently of this formal difference, the two authors have quite different conceptions of the new science. Guerry regards its object as consisting mainly in collecting data in order to gain an opinion of the moral status of a country. Thus he looked upon moral statistics as auxiliary to the history of civilisation. Quetelet went beyond this, inasmuch as he was the first to inquire into the cause of the moral level of a population, and in as much as in his criminal statistics of Belgium, 1833, he had already given expression to the fundamental idea, 'Society bears the germs of crime in itself.'"

was astronomer-royal of Belgium and the founder of the Observatory at Brussels. Having opened his career by some memoirs on geometrical subjects, he directed his attention to questions of meteorology and statistics, which he was probably the first to extend into the region not only of the physical but also of the moral attributes of man, studying the phenomena of crime, suicide, and disease as revealed by the criminal courts in France, the Netherlands, and other countries.

Subsequently it was mainly through his influence that a series of international statistical congresses was held in the principal cities of Europe, and a greater uniformity in the methods of research and registration attempted and partially attained.

21. The "mean man." Quetelet's statistical inquiries centre in the conception of the average or mean man who, in a very geometrical fashion, is looked upon as an analogue of the centre of gravity[1] of a body, being the mean around which the social elements oscillate. "If one tries," he says, "to

[1] Quetelet defines the object of his work as follows ('Sur l'Homme,' vol. i. p. 21): "L'objet de cet ouvrage est d'étudier, dans leurs effets, les causes, soit naturelles, soit perturbatrices qui agissent sur le développement de l'homme; de chercher à mesurer l'influence de ces causes, et le mode d'après lequel elles se modifient mutuellement. Je n'ai point en vue de faire une théorie de l'homme, mais seulement de constater les faits et les phénomènes qui le concernent, et d'essayer de saisir, par l'observation, les lois qui lient ces phénomènes ensemble. L'homme que je considère ici est, dans la société, l'analogue du centre de gravité dans les corps; il est la moyenne autour de laquelle oscillent les élémens sociaux: ce sera, si l'on veut, un être fictif pour qui toutes les choses se passeront conformément aux résultats moyens obtenus pour la société. Si l'on cherche à établir, en quelque sorte, les bases d'une *physique sociale*, c'est lui qu'on doit considérer, sans s'arrêter aux cas particuliers ni aux anomalies, et sans rechercher si tel individu peut prendre un développement plus ou moins grand dans l'une de ses facultés."

establish in some way the foundation of Social Physics, it is the mean man whom one must consider without stopping at particular and anomalous cases and without investigating whether some individual can take a development more or less great in one of his faculties.[1] . . . After having considered man at different epochs and among different peoples, after having successively determined the different elements of his physical and moral condition, . . . we shall be able to fix the laws to which he has been subjected in different nations since their birth—that is to say, we shall be able to follow the course of the centres of gravity of every part of the system."[2] In an astronomical fashion Quetelet speaks of the perturbing forces and variations, and of the "stability of the social system,"[3] and compares the new science of society to the mechanics of the Heavens.[4] The influence of Laplace and his school is evident in every page of Quetelet's work. Whilst speaking of the "variability of the human type and the mean man among different peoples and in different centuries," he

[1] 'Sur l'Homme,' vol. i. p. 22.

[2] Ibid., p. 23.

[3] Ibid., p. 26.

[4] Vol. ii. p. 338. Quetelet speaks of the annual and diurnal periods, and continues: "Les causes régulières et *périodiques*, qui dépendent ou de la période annuelle ou de la période diurne, exercent sur la société des effets plus prononcés et qui varient dans des limites plus larges, que les effets combinés *non périodiques*, produits annuellement par le concours de toutes les autres causes qui agissent sur la société; en d'autres termes, le système social, dans sa manière d'être, paraît être plus dissemblable à lui-même pendant le cours d'une année ou même pendant l'espace d'un jour, que pendant deux années consécutives, si l'on a égard à l'accroissement de la population. La période *diurne* semble exercer une influence un peu plus prononcée que la période *annuelle*, du moins en ce qui concerne les naissances. La période annuelle produit des effets plus sensibles dans les *campagnes* que dans les *villes*, et il paraît en être de même des causes en général qui tendent à modifier les faits relatifs à l'homme."

anticipates discussions which came fifty years later.[1] His aim is to arrive at a precise knowledge of things hitherto vaguely known and merely sketched by artists and literary persons; but he evidently looks beyond the study of the average man to that of individual departures, as of special interest to the physician,[2] for instance, in the case of disease, and he significantly recommends what he calls the "study of maxima."[3] He regards the "mean man in the circumstances in which he is placed as the type of all that is beautiful and all that is

[1] Vol. ii. p. 270: "Les anciens ont représenté avec un art infini l'homme physique et moral, tel qu'il existait alors; et la plupart des modernes, frappés de la perfection de leurs ouvrages, ont cru qu'ils n'avaient rien de mieux à faire que de les imiter servilement; ils n'ont pas compris que le type avait changé; et que, tout en les imitant pour la perfection de l'art, ils avaient une autre nature à étudier. De là, ce cri universel, 'Qui nous délivrera des Grecs et des Romains!' De là cette scission violente entre les classiques et les romantiques; de là enfin, le besoin d'avoir une littérature qui fût véritablement *l'expression de la société*. Cette grande révolution s'est accomplie, et elle fournit la preuve la plus irrécusable de la variabilité du type humain ou de l'homme moyen chez les différens peuples et dans les différens siècles." It is interesting to see from this quotation that the opposition to a one-sided classical education emanated at that time from the romantic movement, whereas in our days it is the scientific movement which forms the opposition.

[2] Vol. ii. p. 281: "Comme dans le plus grand nombre de cas, le malade ne peut présenter aucune observation satisfaisante faite sur sa propre personne, ni aucune des élémens qui lui sont particuliers, le médecin se trouve forcé de la ramener à l'échelle commune, et de l'assimiler à l'homme moyen; ce qui au fond semble présenter le moins de difficultés et d'inconveniens; mais peut causer aussi de graves méprises dans quelques circonstances; car c'est encore le cas de faire observer ici que les lois générales relatives aux masses sont essentiellement fausses étant appliquées à des individus: ce qui ne veut pas dire cependant qu'on ne peut les consulter avec fruit: et les écarts sont toujours considérables."

[3] Vol. ii. p. 284: "Il ne faut pas confondre les lois de développement de l'homme moyen à telle ou telle époque, avec les lois de développement de l'humanité. Elles n'ont en général que peu de rapport entre elles: ainsi je serais très disposé à croire que les lois de développement de l'homme moyen restent à peu près les mêmes aux différens siècles, et qu'elles ne varient que par la grandeur des *maxima*. Or, ce sont justement ces *maxima*, relatifs à l'homme développé, qui donnent, dans chaque siècle, la mesure du développement de l'humanité."

good."[1] And further, "one of the principal things accomplished by civilisation is to draw closer and closer the limits within which the different elements oscillate which are characteristic of man."[2]

There was, however, another idea besides that of the mean man which followed in the course of this mathematical or astronomical treatment of social statistics—namely, the seeming negation of the scope of freewill and of moral responsibility, which seemed inconsistent with the regularity of the statistical records. In his treatise, 'Sur l'Homme,' Quetelet had drawn attention to the regular recurrence of crime —of the tendency to crime—as one of the most remarkable features in society; which, through its physical and moral constitution, "prepares crime, the guilty being only the instrument which carries it 22. Social statistics and freewill.

[1] Vol. ii. p. 287: "J'ai dit précédemment que l'homme moyen de chaque époque représente le type du développement de l'humanité pour cette époque; j'ai dit encore que l'homme moyen était toujours tel que le comportaient et qu'exigeaient les temps et les lieux; que ses qualités se développaient dans un juste équilibre, dans une parfaite harmonie, également éloignée des excès et des défectuosités de toute espèce; de sorte que, dans les circonstances où il se trouve, on doit le considérer comme le type de tout ce qui est beau, de tout ce qui est bien." P. 289: "Un individu qui résumerait en lui-même, à une époque donnée, toutes les qualités de l'homme moyen, représenterait à la fois tout ce qu'il y a de grand, de beau et de bien."

[2] Vol. ii. p. 342: "Un des principaux faits de la civilisation est de resserrer, de plus en plus, les limites dans lesquelles oscillent les différens élémens relatifs à l'homme. Plus les lumières se répandent, plus les écarts de la moyenne vont en diminuant; plus, par conséquent, nous tendons à nous rapprocher de ce qui est beau et de ce qui est bien. La perfectibilité de l'espèce humaine résulte comme une conséquence nécessaire de toutes nos recherches. Les défectuosités, les monstruosités disparaissent de plus en plus au physique; la fréquence et la gravité des maladies se trouvent combattues avec plus d'avantage par les progrès des sciences médicales; les qualités morales de l'homme n'éprouvent pas de perfectionnemens moins sensibles; et plus nous avancerons, moins les grands bouleversemens politiques et les guerres, ces fléaux de l'humanité, seront à craindre dans leurs effets et dans leurs conséquences."

out";[1] society, as it were, exacting a certain proportion of crime, as it does of suicide, poverty, physical and mental disease, for the maintenance of its equilibrium and as an "alarming"[2] tribute to its stability. The extreme consequences which seemed to flow from this doctrine were not drawn by Quetelet, who believed in a gradual though slow development of human society, and in moral as well as physical causes and influences. They were drawn, however, by what we may term the mathematical school of social philosophers, who relied greatly upon the figures collected by Quetelet and confirmed by others. In this country the statistical labours of Quetelet were made known by Sir John Herschel in a brilliant article[3] in the 'Edinburgh Review' on the "Translation of Quetelet's Letters to Prince Albert on the Theory of Probabilities." They do not seem to have been regarded as detrimental to the moral aspect of human history till

23. Buckle. Henry Thomas Buckle, in his celebrated 'History of Civilisation,'[4] made use of Quetelet's statistics in support of one of his favourite theses — viz., that the course of historical progress depends on the combined action of the external physical surroundings and of the intellectual side of human nature. Apart from intellectual modifications the moral side is a constant. In the course of the discussions following the appearance of Buckle's History, especially in Germany, it was conclusively shown that statistical figures prove neither one view nor the other: indeed, one of the most complete and exhaustive treatises on moral statistics comes from the orthodox pen of Alexander von Oettingen, a Professor of Theology, just as we saw that the first great work on political arithmetic in Germany came from the pastor Süssmilch a century earlier. Philosophical writers like Lotze[1]

[1] 'Sur l'Homme,' vol. ii. p. 241.

[2] Cf. vol. ii. p. 262; also 'Système Social' (1848), p. 95, and the 'Mémoire sur la Statistique Morale' (1848).

[3] Vol. xcii. p. 18.

[4] The 'History of Civilisation,' vol. i., appeared in 1857, and was very soon translated in Germany, running in a short time through five editions. There the statistical theories of Quetelet had not made that impression which they made in some other countries. This is explained by the fact that the philosophy of Kant, to which Buckle himself referred in a long passage in his "Introduction," had long before Quetelet accustomed thinkers to abandon the popular conception of freewill, which sees in it merely the absence of causal determinateness, in favour of the causal connection of so-called free actions with the motives and the moral character. The subject has been very fully discussed by F. A. Lange in his well-known 'History of Materialism' (Eng. trans. by Thomas, vol. iii. p. 196, &c.) Lange refers to a remark of the well-known political economist, Prof. Adolph Wagner, who, in his work 'Die Gesetzmässigkeit in den scheinbar willkührlichen menschlichen Handlungen' (Hamburg, 1864, p. xiii, &c.), mentions the fact that Quetelet's writings had not received the attention merited: "This reproach does not quite hit the right point. . . . Wagner might, in fact, have been led by Buckle . . . to see that German philosophy in the doctrine of the freedom of the will has for once an advantage which permits it to regard these new studies with equanimity; for Buckle supports himself above all upon Kant, adducing his testimony for the empirical necessity of human actions, and leaving aside the transcendental theory of freedom. Although all that materialism can draw from moral statistics . . . for the practical value of a materialistic tendency of the age as against idealism has thus been conceded by Kant, it is by no means indifferent whether moral statistics, and, as we may put it, the whole of statistics, is placed in the foreground of anthropological study or not; for moral statistics direct the view outwards upon the real measurable facts of life, while the German philosophy, despite its clearness as to the nullity of the old doctrine of freewill, still always prefers to direct its view inwards upon the facts of consciousness."

[1] Lotze's deliverances on this subject will be found in the third chapter of the seventh book of the 'Microcosmus' (Eng. trans. by Hamilton and Jones, vol. ii. p. 200, &c.), and also in the 'Logik' of 1874 (Book II. chap. 8). In the former passage he says: "The dislike with which we hear of laws of psychic life, whilst we do not hesitate to regard bodily life as subordinate to it own laws, arises partly because we require too much from our own freedom of will, partly because we let ourselves be too much imposed upon by those laws. If we do not find ourselves involved in the declared struggle between freedom and necessity, we are by no means averse to regarding the actions of

24. Criticism of pretension of statistics.

and Drobisch[1] have long ago reduced to their proper measure the pretensions of statistics, and it is now generally admitted that in the sciences dealing with human nature and society, as in those which investigate purely physical phenomena, observations, figures and measurements rarely if ever suffice to establish a valid generalisation; but that, if such be suggested by other processes of thought, notably through attentive reflection on, and analysis of, single and accessible cases, statistics supply the indispensable material by which

men as determined by circumstances: in fact, all expectation of good from education and all the work of history are based upon the conviction that the will may be influenced by growth of insight, by ennoblement of feeling, and by improvement of the external conditions of life. On the other side, a consideration of freedom itself would teach us that the very notion is repugnant to common-sense if it does not include susceptibility to the worth of motives, and that the freedom of willing can by no means signify absolute capacity of carrying out what is willed." And, further, he remarks on "the extreme overhastiness with which the statistical myth has been built up from deductions which cannot be relied upon. We have yet to obtain from exacter investigations the true material for more trustworthy conclusions—material which should take the place of the statistical myth above referred to."

[1] Before Lotze, and as early as 1849, M. W. Drobisch, the Herbartian, had reviewed Quetelet's Memoir, 'Sur la Statistique morale,' &c.: and later (1867), after the publication of A. Wagner's work, he came back to the subject in an important tract, 'Die moralische Statistik und die menschliche Willensfreiheit,' which should be read by every one who desires to form just views on the subject. "In all such facts," says Drobisch, "there are reflected not natural laws pure and simple, to which man must submit as to destiny, but at the same time the moral conditions of society, which are determined by the mighty influences of family life, of the school, the Church, of legislation, and are, therefore, quite capable of improvement by the will of man" (Zeitsch. für exacte Philos.,' vol. iv. p. 329). After all that has been said by Quetelet, Buckle, and others, the words of Schiller ('Wallenstein's Tod,' ii. 3) still remain the best statement of the problem:—

"Des Menschen Thaten und Gedanken, wisst!
Sind nicht wie Meeres blind bewegte Wellen.
Die innre Welt, sein Microcosmus, ist
Der tiefe Schacht, aus dem sie ewig quellen.
Sie sind nothwendig, wie des Baumes Frucht;
Sie kann der Zufall gaukelnd nicht verwandeln,
Hab'ich des Menschen Kern erst untersucht,
So weiss ich auch sein Wollen und sein Hendeln."

these generalisations can be tested, elevated to the rank of leading canons of thought and research, and in rare cases to that of the expression of a law of nature. So far, therefore, as the complicated phenomena presented in meteorology, agriculture, and economics are concerned, the suggestions leading to so-called laws have in every case been got elsewhere—from astronomy, chemistry, psychology, history, &c.; and the work of science has subsequently consisted largely in gathering the necessary statistical materials by which to prove, amplify, curtail, or refute them. In many cases it has been found that even elaborate series of observations had not been performed in such a manner[1] as would permit of the necessary inferences being drawn from them. Similarly biologists after Darwin have had to rearrange the collections made by those who came before the epoch marked by that great name.

[1] This refers as much to statistical figures as to the knowledge accumulated in many of the natural sciences. Especially it refers to the statistical material upon which Quetelet based his startling and epoch-making assertions: the earlier critics had, as V. John observes ('Geschichte der Statistik,' p. 364), dealt with the deductions which Quetelet had drawn, without dealing with the empirical material itself. It was therefore of great importance that Prof. Rehnisch of Göttingen for the first time submitted the figures themselves to a searching analysis. He did this in the years 1875-76, in his articles in the 'Zeitschrift für Philosophie und Philosophische Kritik,' through which it became evident that the inferences were, as Lotze had lready suggested, to say the least, premature. "In the memoir 'Sur le Penchant au Crime' (1831), only four years, and in the work 'Sur l'Homme,' only six years 1826-31) of the 'compte général,' furnished the data upon which the astounding regularity with which crime repeats itself was maintained" (V. John, p. 365). Rehnisch adds many other examples of the extreme incompleteness of the records upon which the theory of Quetelet is built up. More recent labours have therefore been to a large extent directed towards gathering more complete statistical data, as well as towards improving the mathematical methods themselves to which not only these but also the population and mortality statistics have been submitted, for the purpose of arriving at average figures.

With the scientific treatment of the phenomena of human society, the name of Adolphe Quetelet will always be associated; yet the mathematical or exact school was not the only one which in the course of the first half of the century had approached the subject.
25. Historical criticism. Notably in Germany, under the ruling influence of philosophical, historical, and critical studies, a school of research had grown up calling itself the historical. If the centre of gravity of the mathematical view lies in the conception of a certain uniformity and stability of social phenomena, the other school looked more to historical changes and developments, opposing the doctrine of the movement or of the dynamics to that of the statics of society. Its inspiration came from a different quarter, and will occupy us in a later portion of this work. For the moment it suffices to remark how here also, in the study of economics and social phenomena, the developmental or genetic view has gradually dispelled the earlier search for recurrent forms and regularities, which we may term the morphological aspect: the physiology has succeeded the anatomy of society.

But statistical methods, with the accompanying doctrines of probability and averages and the theory of error, have not only been extensively and usefully employed where large numbers of similar facts and events crowd in upon our observation, and, as it were, overwhelm us by their multitudes, as in astronomy, meteorology, economics, and political arithmetic: they have also shown themselves applicable by what we may term the inverse method. Quetelet, when dealing with long columns of human statistics, felt a relief in studying the average or mean man. Is it not possible that in many instances what nature and experience show us is only the average itself — our senses and our intellect being too coarse to penetrate to the numberless individual cases out of which the sum or the average is made up? May not even the simplest phenomenon or thing in nature be in fact 26. Application in physics.
an aggregate, a total, and its apparent behaviour and properties merely a collective effect? Both the kinetic and the atomic view of natural objects and phenomena seem to favour this way of regarding things,— the former showing us in many cases motion and unrest where at the first glance we saw only rest, and the latter dissolving apparently continuous and homogeneous structures into crowds or assemblages of many particles.

Thus the apparently steady pressure of gases is now known to be in reality the violent bombardment of the wall of the containing vessel by their molecules; and the most homogeneous and transparent crystal is revealed, by its optical properties, as an assemblage of very minute particles, held together by forces which may be overcome by mechanical or chemical agencies. Regarded from this point of view, our knowledge of natural objects is merely statistical: it deals with aggregates; it is a collective knowledge. And if we further consider that the sameness of the numberless constituent particles is by no means proved, this collective knowledge turns out to be merely concerned with averages: it is statistical, not individual,

information that we seem to possess; it resembles the knowledge which an economist may possess of the statistics of a society or of the properties of the 'mean" man. If such be the case, the theory of large numbers and the calculus of probabilities must be applicable and useful in dealing with those phenomena which, through their minuteness and great number, elude our detailed examination.

The first to introduce this conception of treating a very large assemblage of moving things by the method of averages was Joule,[1] who, adopting Daniel Bernoulli's conception, calculated the average velocity which a particle of hydrogen gas must possess in order to explain the total effect which shows itself as a definite gas pressure at a definite temperature. His result was that this average speed must be 6055 feet per second in order to be equal to the pressure of one atmosphere at the zero temperature of the Centigrade scale. The speed of the particles, however, cannot be assumed to be equal, owing to continual encounters; and we are indebted to Clausius and

27. Clausius and Clerk Maxwell.

Clerk-Maxwell for introducing the more refined statistical methods of the theory of probabilities. They calculated the mean free path, and showed that former calculations of the average speed were in the main correct. The kinetic theory of gases afforded an opportunity of brilliantly applying the conceptions of averages or means and of the differences of frequencies as the measure of the probability of certain occurrences. In this case—as was first shown by Joule's figures—we

[1] See *supra*, vol. i. p. 434, and vol. ii. p. 110.

have to do with billions and trillions of particles, moving with velocities varying from zero to many thousands of miles per second: we have therefore to do with numbers which practically mean infinity—that is to say, we have to do with that condition of things where alone the laws of probability become strictly correct.[1]

In this case, any deductions which can be made as to the average condition or collective behaviour of an infinitely large assemblage of particles, whose individual members move about with infinitely varying velocities at infinitely varying speeds in infinitely varying directions, must be realised in the well-known laws of gaseous bodies referring to pressure, volume, expansion, molecular structure, and heat, assuming the latter to be merely the sensible effect on our nerves of very numerous impingements of infinitesimally small particles. It is one of the greatest triumphs of the mathematical methods applied in one of the most difficult instances, that the average behaviour and collective properties of

[1] P. G. Tait ('Heat,' 1884, p. 355) says: "It is to Clausius that we are indebted for the earliest approach to an adequate treatment of this question. He was the first to take into account the collisions between the particles, and to show that these did not alter the previously obtained results. He has also the great credit of introducing the statistical methods of the theory of probabilities, and of thus giving at least approximate ideas as to the probable length of the *mean free path*—*i.e.*, the average distance travelled over by a particle before it impinges on another, and thus has its course changed. He thus explains also the slowness of diffusion of gases, and their very small conductivity of heat. Clerk-Maxwell shortly afterwards improved the theory by introducing, also from the statistical point of view, the consideration of the variety of speed at which the different particles are moving; Clausius having expressly limited his investigations by assuming for simplicity that all move with equal speed. Clerk-Maxwell explained gaseous friction, and gave a more definite determination of the length of the mean free path."

such moving crowds turn out to be exactly those laws which Boyle, Charles, Gay-Lussac, Dalton, and Avogadro had found out by direct experiments with gaseous bodies. James Clerk-Maxwell was the first to recognise the great importance of the statistical methods, and to apply them in an exhaustive manner.[1] And we witness here the same spectacle which presented itself in the history of the theory of probabilities. Problems which are to be solved by the mere application of a few rules dictated by common-sense and an exercise of common logic, present in their complexity such a multitude of traps, snares, and pitfalls, that it required the successive application of the highest intellects to free the reasoning from insidious errors, and put the results on

28. Mathematical representation of experimental laws.

[1] The manner in which Joule dealt with the problem of a large crowd of moving particles in his memoir of 1851 was not strictly statistical, inasmuch as he dealt with an average velocity of the molecules, and assumed that all the molecules of a gas moved with the same velocity. Clausius, in his memoir of 1857, made use of assumptions which were more in conformity with nature: he had, accordingly, to employ the calculus of probabilities. Clerk-Maxwell's occupation with the subject dates from the year 1859, when he read his paper, "Illustrations of the Dynamic Theory of Gases," Part I. (published in the 'Phil. Mag.,' 4th series, vol. xix. p. 19, reprinted in 'Scientific Papers,' vol. i.) He showed that "the velocities are distributed among the particles according to the same law as errors are distributed among the observations in the theory of the method of least squares. The velocities range from 0 to ∞, but the number of those having great velocities is comparatively small." If we leave out Joule's imperfect attempt to employ the statistical method, one of the first applications of the method of averages to a physical problem is to be found in Sir G. G. Stokes's paper "On the Composition of Streams of Polarised Light from different Sources" ('Camb. Phil. Trans.,' 1853), where he shows "what will be the average effect of a very great number of special sources of light: thus giving one of the earliest illustrations of the use, in physics, of the statistical methods of probabilities. . . . From this point of view the uniformity of optical phenomena becomes quite analogous to the statistical species of uniformity, which is now found to account for the behaviour of the practically infinite group of particles forming a cubic inch of gas" (P. G. Tait, 'Light,' 2nd ed., 1889, p. 237).

undisputed and indisputable bases.[1] In proportion as this has been done the calculated results have proved to be in closer and closer accord with observed facts. I will here mention only one of the latest achievements in this line of research and reasoning. Assuming—as the atomic and kinetic theories do—that all external phenomena of bodies can be reduced to the collective or mean effect of a practically infinite variety of turbulent movements of a very large number of particles, it must be possible to give a mechanical explanation of that remarkable property of all phenomena of nature, first noticed by Lord Kelvin, that they are essentially irreversible—*i.e.*, that, with very rare exceptions, they take place in a certain direction which we may define as an equalisation of existing differences of level, temperature, electric pressure, and similar inequalities. In order to fix this remarkable property of all natural phenomena, physicists found themselves obliged to introduce, alongside of energy and mass (which are both assumed to conserve or maintain their total quantity), a third something which is the measure of the degree in which an existing distribution of mass and energy can be considered to be capable of external, visible, finite activity

29. Irreversibility of natural processes.

[1] Those who are interested in seeing how difficult it is to link together the common-sense arguments of the theory of probabilities in a consistent chain of unimpeachable logic, should read the report on the various attempts to prove Clerk-Maxwell's law (mentioned in the foregoing note) contained in Prof. O. E. Meyer's 'Kinetische Theorie der Gase' (2nd ed., Breslau, 1899), especially p. 46, &c., and 'Mathematical Appendix,' p. 17; and the great number of memoirs referred to on p. 60 of that book. Nevertheless Tait speaks of the still remaining difficulties in the kinetic theory of gases as having been "greatly enhanced by an apparently unwarranted application of the theory of probabilities on which the statistical method is based." ('Properties of Matter,' 2nd ed., 1890, p. 291.)

—*i.e.*, of its availability to do work.[1] The infinitesimally small motions of an immense crowd may be exerted in such a way as to total up to a finite movement perceptible to our senses and accessible to our handling, or they may so mutually annul each other as to present in their finite sum and aggregate the appearance of rest and inaction, however turbulent their behaviour might appear to an observer gifted with powers of perception millions
30. Lord Kelvin. of times more delicate than ours. Lord Kelvin introduced the conception of the availability of energy,[2] Clausius that of entropy (or energy which is hidden away), to measure this condition of any natural system. Has the statistical view any conception to put at the base of this remarkable property of natural phenomena? It has, and we must assign to Clerk-Maxwell[3] the

[1] See *supra*, chap. vii. p. 128, &c.

[2] Or of "motivity" (*i.e.*, "energy for motive power"), this being "the possession, the waste of which is called dissipation." See *supra*, chap. vii. p. 168; also Thomson (Lord Kelvin), 'Popular Addresses,' vol. i. p. 141.

[3] The contributions of Clerk-Maxwell to this topic are notably two, independently of the larger view which he took of statistical, as compared with historical, knowledge, of which I treat farther on in this chapter. First, in the concluding remarks of his treatise on the 'Theory of Heat' ("On the Limitation of the Second Law of Thermodynamics") he introduced his famous conception of a "sorting demon," the meaning of which fanciful device was, to impress upon the student of the dynamical theory of heat, first, the fact that the loss of availability of the energy of molecular motion is owing to the coarseness of our senses; and second, that the restoration of differences of temperature, or of availability of energy, is simply a matter of arrangement or order, not of an increase of the intrinsic energy of the system. The subject has been frequently referred to, notably by Lord Kelvin, who says ("On the Sorting Demon of Clerk-Maxwell," Royal Institution, February 1879. Reprinted in 'Popular Lectures and Addresses,' vol. i. p. 137, &c.): "Dissipation of energy follows in nature from the fortuitous concourse of atoms. The lost motivity is essentially not restorable otherwise than by an agency dealing with individual atoms; and the mode of dealing with the atoms to restore motivity is essentially a process of assortment, sending this way all of one kind or class, that way all of another kind or class"

credit of having first indicated, and to Prof. Boltzmann[1] —aided by many other eminent natural philosophers —that of having definitely established, this highly suggestive explanation or illustration. The doctrine of chances, to which artifice the statistical view of

(p. 139). "The conception of the 'sorting demon' is merely mechanical, and is of great value in purely physical science. It was not invented to help us to deal with questions regarding the influence of life and mind on the motions of matter, questions essentially beyond the range of mere dynamics" (p. 141). The other contribution through which Clerk-Maxwell's name has become celebrated in this connection is to be found in the so-called Maxwell-Boltzmann law of the distribution of kinetic energy in a mass of moving particles. The discussion of the subject dates from the first memoir of Clerk-Maxwell, quoted above; and, after Prof. Boltzmann had treated of the same subject in 1868, and Mr Watson in 1876, Clerk-Maxwell returned to it in a paper "On Energy in a System of Material Points" ('Camb. Phil. Soc.,' vol. xii.) In the year 1894 Prof. Bryan presented the 2nd part of his Report on "Our Knowledge of Thermodynamics" ('Brit. Assoc. Rep.,' 1894, p. 64, &c.), in which he gives an account of all the different investigations referring to this subject, up to that date. This was followed by a long discussion of the subject in the pages of 'Nature' (vol. li.), in which Messrs Bryan, Boltzmann, Burbury, Culverwell, Larmor, and H. W. Watson took part, and which gave Prof. Boltzmann the opportunity of giving a final expression of his opinion (p. 415).

[1] Prof. Boltzmann's investigations connected with the second law of thermodynamics and the kinetic theory of gases cover the last thirty-five years. He has succeeded in putting the whole problem more and more into a strictly accurate, as also into a popularly intelligible, form. Unfortunately his very numerous contributions are scattered in various periodical publications, and have not yet appeared in a collected edition. Most of them appeared in the Proceedings and Transactions of the Vienna Academy, among which the Address delivered on the 29th May 1886 can be specially recommended. Since then, and after the correspondence in 'Nature' referred to in the last note, he has published his lectures 'Vorlesungen über Gas-Theorie' (2 vols., Leipzig, 1896-98). He there (vol. ii. p. 260, note) gives a list of the most important literature on the subject, and also a general summary regarding the application of the theory of probabilities to the distribution of the kinetic energy of a crowd of moving particles. In this connection he also deals with the consequences of the atomic hypothesis, the irreversibility of all natural processes, and the application of the second law to the history of the universe. He there says (p. 253): "The fact that the closed system of a finite number of molecules, if it had originally an orderly condition, and has then lapsed into a disorderly one, must finally, after the lapse of an inconceivably long period, assume again orderly conditions, is

phenomena reduces us, distinguishes between probable and improbable events or arrangements of a crowd of elements—*i.e.*, between such as are of an average and such as are of an exceptional character. Any highly improbable arrangement—though possible—will be followed by a gradual settling down to more probable or average arrangements. And as in nature you are forced to introduce the conception of availability, so in the calculus of chances you can introduce a certain mathematical quantity which is the measure of the probability. The more improbable, *i.e.*, exceptional, the begin-

not a refutation, but a confirmation, of our theory. But one must not consider the matter thus: as if two gases . . . which were initially unmixed, then became mixed, after a few days again unmixed, then again mixed, &c. We find, rather, that . . . only after a period which, even compared with $10^{10^{10}}$ years, is enormously great, a perceptible unmixing would take place. That this is practically equivalent to never, we see, if we consider that in this period there would be, according to the laws of probability, many years in which, by mere chance, all the inhabitants of a large city would, on the same day, commit suicide, or fire break out in all its buildings; whereas the insurance companies are in so good an agreement with facts that they do not consider such cases at all. If even a much smaller improbability were not practically identical with impossibility, nobody could rely upon the present day being followed by night and the latter again by day." And further (p. 255): "If we, therefore, represent the world under the figure of an enormously large mechanical system, composed of enormously numerous atoms, which started from a very perfectly ordered condition, and exist still mainly in an orderly condition, we arrive at consequences which actually stand in perfect harmony with observed facts"; and (p. 258), "That in nature the transition from a probable to an improbable condition does not happen as frequently as the reverse, can be explained by the assumption of a very improbable initial state of the whole surrounding universe, in consequence of which any arbitrary system of interacting bodies is, in general, in an improbable condition to begin with. But one might say, that here and there the transition from probable to improbable conditions must, after all, be observable. . . . From the numbers regarding the inconceivably great rarity of a transition from probable to improbable conditions, happening in observable dimensions and during an observable period, it is explained how such a process within what we, cosmologically, call a single world, or, specially, our world, is so extremely rare that any experience of it is excluded."

ning you choose, the greater your distance from the average or most probable condition into which, in the long-run, things must settle down; the more play for the equalising and levelling down of coming events. The world—or at least that part of the world accessible to our observation, and the playground of our activity—shows a large amount of available energy, or, expressed in a purely statistical manner, it started from a highly improbable condition, and it is descending or running down into a more probable or average condition. The doctrine of availability or of its reverse, of entropy —*i.e.*, of the loss of availability—turns out to be a theorem of probabilities; and the refined mathematical researches of Prof. Boltzmann and others show that these two conceptions can be made to cover each other.

31. "Availability" a theorem in probability.

Moreover, we can bring home to the popular understanding the difference between the exceptional condition, with its large amount of available energy, and the average condition, with its large amount of self-destructive and wasted energy (or entropy), by the simile of order and disorder. For every arrangement of a crowd of things or beings which is orderly, there are innumerable arrangements which are disorderly; every one knows how easily the orderly arrangement lapses into disorder, and nobody expects by mere haphazard or chance movements to produce order out of disorder. There are thousands of ways by which a stone can fall from the peak of a mountain to the lower levels, but only one direction which would take it up again to the top. A tree has been suggested as the picture of the course that natural movements take: for the one position

in the trunk, where all branches and all roots meet, there are in both directions numberless ways of ramification or dissipation into the twigs or the root-fibres. The statistical view measures the chances of an orderly arrangement compared with disorder, of a commanding unique position compared with the average or mean position, by saying the odds are infinity to one against it. The orderly exceptional position and arrangement of a crowd does not possess more actual energy, but its energy is directed, arranged, it has become available—get-at-able.

32. "Selection" as conceived by Maxwell.

And what is it that changes disorder into order? It is a process of selection. Maxwell imagined a sorting demon endowed with powers of perceiving and dividing the immeasurably small movements of a gaseous body—*i.e.*, of a crowd of particles in turbulent to and fro movement. Such a being could, by mere selection and separation of the slow and fast moving particles, bring order into disorder, converting the unavailable energy into available energy. It would be a process of mere sifting and arranging, such as is apparently carried out in the living creation and by organic structures.[1] And Maxwell went a step further, and conceived the idea

[1] See *supra*, chap. x. p. 437, note, where the selective action of certain organisms is referred to in connection with Prof. Japp's Address to the Brit. Assoc. in 1898. Lord Kelvin says ("On the Dissipation of Energy," 1892, 'Popular Lectures and Addresses,' vol. ii. p. 463, &c.): "It is conceivable that animal life might have the attribute of using the heat of surrounding matter, at its natural temperature, as a source of energy for mechanical effect. . . . The influence of animal or vegetable life on matter is infinitely beyond the range of any scientific inquiry hitherto entered on. Its power of directing the motions of moving particles, in the demonstrated daily miracle of our human free-will, and in the growth of generation after generation of plants from a single seed, are infinitely different from any possible result of the fortuitous concourse of atoms."

that, after all, the whole of our knowledge of natural phenomena and natural things may be only statistical, not historical or individual. "In dealing," he says,[1] "with masses of matter, while we do not perceive the individual molecules, we are compelled to adopt the statistical method of calculation, and to abandon the strict dynamical method in which we follow every motion by the calculus. It would be interesting to inquire how far those ideas about the nature and the methods of science which have been derived from examples of scientific investigation in which the dynamical method is followed, are applicable to our actual knowledge of concrete things, which, as we have seen, is of an essentially statistical nature, because no one has yet discovered any practical method of tracing the path of a molecule, or of identifying it at different times." And elsewhere[2] he says: "The statistical method of investigating social questions has Laplace for its most scientific and Buckle for its most popular

[1] 'Theory of Heat,' 8th ed., p. 329.

[2] 'Life of Clerk-Maxwell by Campbell and Garnett.' Chap. xiv. contains a paper with the title, "Does the progress of Physical Science tend to give any advantage to the opinion of Necessity (or Determinism) over that of the Contingency of Events and the Freedom of the Will?" In it (p. 435) there occurs the following passage: "The doctrine of the conservation of energy, when applied to living beings, leads to the conclusion that the soul of an animal is not, like the mainspring of a watch, the motive power of the body, but that its function is rather that of a steersman of a vessel—not to produce, but to regulate and direct, the animal powers." He then speaks of the "powerful effect on the world of thought" which the developments of molecular science are likely to have, considering the "most important effect on our way of thinking to be that it forces on our attention the distinction between two kinds of knowledge, which we may call for convenience the Dynamical and Statistical." The paper from which the extracts in the text are taken is dated 1873. Clerk-Maxwell was then forty-one years of age.

expounder. Persons are grouped according to some characteristic, and the number of persons forming the group is set down under that characteristic. This is the raw material from which the statist endeavours to deduce general theorems in sociology. Other students of human nature proceed on a different plan. They observe individual men, ascertain their history, analyse their motives, and compare their expectation of what they will do with their actual conduct. . . . However imperfect this study of man may be in practice, it is evidently the only perfect method in principle. . . . If we betake ourselves to the statistical method, we do so confessing that we are unable to follow the details of each individual case, and expecting that the effects of widespread causes, though very different in each individual, will produce an average result on the whole nation, from the study of which we may estimate the character and propensities of an imaginary being called the Mean Man. Now, if the molecular theory of the constitution of bodies is true, all our knowledge of matter is of a statistical kind.

33. Statistical knowledge of nature.

A constituent molecule of a body has properties very different from those of the body to which it belongs. The smallest portion of a body which we can discern consists of a vast number of molecules, and all we can learn about the group of molecules is statistical information. . . . Hence those uniformities which we observe in our experiments with quantities of matter containing millions of millions of molecules are uniformities of the same kind as those explained by Laplace and wondered at by Buckle, arising from the slumping to-

gether of multitudes of cases, each of which is by no means uniform with the others. . . . Much light may be thrown on some of these questions by the consideration of stability and instability. When the state of things is such that an infinitely small variation of the present state will alter only by an infinitely small quantity the state at some future time, the condition of the system, whether at rest or in motion, is said to be stable; but when an infinitely small variation in the present state may bring about a finite difference in the state of the system in a finite time, the condition of the system is said to be unstable. It is manifest that the existence of unstable conditions renders impossible the prediction of future events, if our knowledge of the present state is only approximate and not accurate. It has been well pointed out by Prof. Balfour Stewart that physical stability is the characteristic of those systems from the contemplation of which determinists draw their arguments, and physical instability that of those living bodies, and moral instability[1] that of those developable souls which furnish to consciousness the conviction of free-will."[2]

[1] There is an awkward misprint in the first edition of 'The Life,' which is corrected in the second edition.

[2] Clerk-Maxwell frequently reverts to this subject. In an article on "Molecules," contributed to the ninth edition of the 'Ency. Brit.' (reprinted in 'Scientific Papers,' vol. ii.), he contrasts historical and statistical knowledge as follows (p. 373): "The modern atomists have adopted a method which is, I believe, new in the department of mathematical physics, though it has long been in use in the section of statistics. When the working members of Section F (of the Brit. Assoc.) get hold of a report of the census, or any other document containing the numerical data of economic and social science, they begin by distributing the whole population into groups according to age, income-tax, education, religious belief, or criminal convictions. The number of individuals is far too great to allow of their tracing the

The conceptions involved in the atomic and kinetic views of natural processes, and the statistical manner of dealing with these crowds of moving particles, have thus introduced into natural philosophy two distinct and novel considerations not known to former ages: first, the consideration that our knowledge of things and phenomena in nature is not historical, but that it is that of the mean or average and of the total effects produced by an immensely large number of singly imperceptible events upon our senses which are too coarse to receive or deal with individual occurrences; secondly, the consideration that our knowledge is not purely mechanical, inasmuch

history of each separately, so that, in order to reduce their labour within human limits, they concentrate their attention on a small number of artificial groups. The varying number of individuals in each group, and not the varying state of each individual, is the primary datum from which they work. This, of course, is not the only method of studying human nature. We may observe the conduct of individual men and compare it with that conduct which their previous character and their present circumstances, according to the best existing theory, would lead us to expect. Those who practise this method endeavour to improve their knowledge of the elements of human nature in much the same way as an astronomer corrects the elements of a planet by comparing its actual position with that deduced from the received elements. The study of human nature by parents and schoolmasters, by historians and statesmen, is, therefore, to be distinguished from that carried on by registrars and tabulators, and by those statesmen who put their faith in figures. The one may be called the historical and the other the statistical method. The equations of dynamics completely express the laws of the historical method as applied to matter, but the application of these equations implies a perfect knowledge of all the data. But the smallest portion of matter which we can subject to experiment consists of millions of molecules, not one of which ever becomes sensible to us. We cannot, therefore, ascertain the actual motion of any one of these molecules; so that we are obliged to abandon the strict historical method of dealing with large groups of molecules. The data of the statistical method, as applied to molecular science, are the sums of large numbers of molecular quantities. In studying the relations between quantities of this kind, we meet with a new kind of regularity, the regularity of averages, which we can depend upon quite sufficiently for all practical purposes, but which can make no claim to that character of absolute precision which belongs to the laws of abstract dynamics."

as besides the purely mechanical movements and their summation, it must contain a reference to the nature of our own faculties—a principle which indicates to what extent the elementary movements come under our control or escape it. There must be a principle which measures the availability and usefulness—for our powers—of natural processes, marking off what is orderly for our senses and accessible to our powers, from what is disorderly and inaccessible. This principle the founders of the science of Thermodynamics—Rankine, Clausius, and Thomson—had empirically established; Thomson having foreseen its far-reaching importance in the economy of nature and the applications of industry. The statistical view of natural phenomena forced upon us by atomism and kinetics has shown us that it is not a purely mechanical [1] principle. It is one belonging to the theory of averages and probability. The scientific view of nature is thus, as Clerk-Maxwell says, neither purely historical nor purely mechanical—it is statistical.[2] 34. As opposed to historical and mechanical knowledge.

To this view of the scientific treatment of natural phenomena Clerk-Maxwell has attached a further con-

[1] Clerk-Maxwell, in a review of Tait's "Thermodynamics" ('Scientific Papers,' vol. ii. p. 670): "The truth of the second law is therefore a statistical, not a mathematical, truth, for it depends on the fact that the bodies we deal with consist of millions of molecules, and that we never can get hold of single molecules."

[2] Any one who has had occasion to observe the internal work of any large industrial or manufacturing organisation, will have noticed the twofold way in which important occurrences are looked at by the commercial and the technical chiefs. As regularity is in many instances the condition of success, any break of its routine is carefully examined and criticised. In such cases the technical man will look to the proximate mechanical causes for an explanation, whereas the commercial man, unable to reflect on the technical and mechanical conditions of the special case, will always refer to his statistics of the past as a guide in judging the immediate difficulty that is before him.

sideration, which is interesting inasmuch as it shows that that which I called above the inverse method of statistics does not involve ideas identical with those which the direct method — as applied in ordinary economic and social statistics—involves. In the direct processes of statistics, which we may class under the all-case or enumerative method, we rise, from a large number of individual facts and data which are all different, to the conception of certain uniform averages, to recurring, or continuously and slowly changing, totals, such as we handle daily in sciences like meteorology, in moral, economic, and industrial statistics. The averages are nowhere represented by the individuals, and the regularity of the totals does not appear in dealing with single instances, or with such restricted numbers as come under the personal control of any of us; hence the general uselessness of statistics in handling individual cases or predicting special occurrences. But the statistical view of natural phenomena, as applied to the atomic constitution of bodies, leads us ultimately to the conception that the smallest constituents of matter, the atoms, exhibit a regularity and recurrent uniformity of structure which reminded Sir John Herschel of manufactured articles. The attempt to reduce the somewhat numerous types of these ultimate elements to purely geometrical configurations of the homogeneous elements of one substance has indeed failed, though it is being continually revived. But allowing that there exist some sixty or seventy distinct forms of matter or atomic structures, these structures seem to be alike and stable wherever we meet with

them; our observations ranging over very large distances in space and time, from the particles immediately before us in artificial flames to the vibrations of atoms of distant stars, which must have taken millions of years to reach us. "I do not think," says Clerk-Maxwell,[1] "that the perfect identity which we observe between different portions of the same kind of matter can be explained on the statistical principle of the stability of the averages of large numbers of quantities, each of which may differ from the mean. . . . For if the molecules of some substance, such as hydrogen, were of sensibly greater mass than others, we have the means of producing a separation between molecules of different masses, and in this way we should be able to produce two kinds of hydrogen, one of which would be somewhat denser than the other. As this cannot be done, we must admit that the equality which we assert to exist between the molecules of hydro-

[1] 'Theory of Heat,' p. 329, &c. Cf. also many passages in the articles on "Atom," "Molecule," "Constitution of Bodies," &c., reprinted in the second volume of 'Scientific Papers'; *inter alia*, p. 483: "But the equality of the constants of the molecules is a fact of a very different order. It arises from a particular distribution of matter, a *collocation*, to use the expression of Dr Chalmers, of things which we have no difficulty in imagining to have been arranged otherwise. But many of the ordinary instances of collocation are adjustments of constants, which are not only arbitrary in their own nature, but in which variations actually occur; and when it is pointed out that these adjustments are beneficial to living beings, and are therefore instances of benevolent design, it is replied that those variations which are not conducive to the growth and multiplication of living beings tend to their destruction, and to the removal thereby of the evidence of any adjustment not beneficial. The constitution of an atom, however, is such as to render it, so far as we can judge, independent of all the dangers arising from the struggle for existence. Plausible reasons may, no doubt, be assigned for believing that if the constants had varied from atom to atom through any sensible range, the bodies formed by aggregates of such atoms would not have been so well fitted for the construction of the world as the bodies which actually exist. But as we have no experience of bodies formed of such variable atoms, this must remain a bare conjecture."

gen applies to each individual molecule, and not merely to the average of groups of millions of molecules." And Clerk-Maxwell goes on to show how the fact that the molecules [1] "all fall into a limited number of classes or species with no intermediate links . . . to connect one species with another by uniform gradation, produces that kind of speculation with which we have become so familiar under the name of theories of evolution, it being quite inapplicable to the case of the molecules. The individuals of each species [2] of molecules are like tuning-forks all tuned to concert pitch, or like watches regulated to solar time." [3]

[1] 'Theory of Heat,' p. 330.

[2] Ibid., p. 331.

[3] The passages quoted from Clerk-Maxwell's writings, and the inferences drawn by him, were criticised by Clifford in a lecture delivered in 1874 with the title, "The First and the Last Catastrophe. A Criticism of some recent Speculations about the Duration of the Universe" (reprinted in 'Lectures and Essays,' vol. i. p. 191 *sqq.*); and, quite recently, Prof. Ward has, in his Gifford lectures, reviewed both Maxwell's and Clifford's arguments ('Naturalism and Agnosticism,' vol. i. p. 99, &c.) As Prof. Ward says, the ideas of Herschel and Clerk-Maxwell "are far more due to theological zeal than to the bare logic of the facts." It is, therefore, out of place to discuss here the philosophical consequences of the ideas of the immutability or of the gradual evolution of the ultimate elements of matter. In a former chapter (see pp. 360 *sqq.* and 369, note, of this volume) I referred to the theories of the evolution of the different chemical elements as they have been put forward by various scientific authorities. The interest which attaches to the passages quoted from Clerk-Maxwell is, that in them, for the first time, an instance was given of the application of statistical methods in the domain of abstract science. The reader may gather from a perusal of the writings mentioned above, as also of the present and foregoing chapters of this history, that there is an inherent contradiction (or as Kant would say, antinomy) between the logical methods and the highest objects of scientific reasoning. The methods all tend in the direction of reducing existing differences in the things and phenomena of nature to a small number of data which are easily grasped and calculated, whereas the observation of things natural forces increasingly upon us the existence of ever greater differences, changes, and varieties. The question presents itself, Is it likely that a process the principle of which is unification and simplification, will ever lead to a comprehension of that which increasingly reveals itself to be infinitely complex and varying? Dr Larmor has some remarks which bear on this subject ('Æther and Matter,' p. 288): "The processes by which our conception of the uniformity of Nature is obtained essentially involve averaging of effects, and lose their efficacy long before the individual molecule is reached. Mechanical determinateness thus need not involve molecular determinateness; then why should either of them involve determination in the entirely distinct province of vital activity? . . . Every vital process may conceivably be correlated with a mechanical process, as to its progress, just to that extent to which it is possible experimentally to follow it, without lending any countenance to a theory that would place its initiation under the control of any such system of mechanical relations. In other terms, there is room for complete mechanical co-ordination of all the functions of an organism, treated as an existing material system, without requiring any admission that similar principles are supreme in the more remote and infinitely complex phenomena concerned in growth and decay of structure."

35. Sameness and variation.

The progress of modern science has, however, given a great impetus to the development of statistical or enumerative methods, and notably to the graphical registration of these results, through the importance which the phenomena of variation attained in all theories of evolution, and chiefly in those based upon natural selection. Quetelet had already pointed to the study of the maxima of the possible deviations from the mean and average, as of special interest and value. Nevertheless, the centre of gravity of the aspect unfolded in the writings of Quetelet and his followers was the idea of uniformity and average sameness. The conception of change and development did not fit naturally and logically into their scheme.[1] It was not till after the

[1] A fate overtook the theories and writings of Quetelet and Buckle similar to that which I had occasion to notice above in referring to the great work of A. von Humboldt. Through the influence of the evolutionist movement, prepared by Lamarck, von Baer, Spencer, and others, centring in Darwin, the statical or morphological view had in every department of science to give way to the kinetic or genetic view. This explains why some names, once celebrated, like Humboldt and Buckle, sank rapidly into oblivion. Grant Allen, in his somewhat one-sided but spirited monograph on Darwin ('English Worthies,' 1888), has drawn attention to this. I give here the striking passage, reserving for the sequel of this work the liberty to differ in detail from much in it that is too drastically expressed: "There is no department of human

36. Darwin. publication of the 'Origin of Species' that the phenomena of variation—*i.e.*, of deviation from the existing type or average—forced themselves upon naturalists and statisticians as requiring to be specially observed, described, and accounted for. Since that time a new branch of science has sprung up, unknown before even by name—the study of variation in nature. This, as we have seen in a former chapter, is one of the great and important aspects of nature brought prominently before the thinking naturalist by Darwin's and Wallace's discoveries, and strongly urged forward by the independent arguments of Mr Herbert Spencer. It involves the great problems of Inheritance and Adaptation. What are the facts, and what the causes of variation, of the moving and propelling principle in natural selection and evolution? The latter is a physiological problem—the former is one of statistics.

thought or human action which evolutionism leaves exactly where it stood before the advent of the Darwinian conception. In nothing is this fact more conspicuously seen than in the immediate obsolescence (so to speak) of all the statical pre-Darwinian philosophies which ignored development, as soon as ever the new progressive evolutionary theories had fairly burst upon an astonished world. Dogmatic Comte was left forthwith to his little band of devoted adherents; shadowy Hegel was relegated with a bow to the cool shades of the common-rooms of Oxford; Buckle was exploded like an inflated windbag; even Mill himself,—*magnum et venerabile nomen*,—with all his mighty steam-hammer force of logical directness, was felt instinctively to be lacking in full appreciation of the dynamic and kinetic element in universal nature. Spencer and Hartmann, Haeckel and Clifford, had the field to themselves for the establishment of their essentially evolutionary systems. Great thinkers of the elder generation, like Bain and Lyell, felt bound to remodel their earlier conceptions by the light of the new Darwinian hypotheses. Those who failed by congenital constitution to do so, like Carlyle and Carpenter, were, philosophically speaking, left hopelessly behind and utterly extinguished. Those who only half succeeded in thus reading themselves into the new ideas, like Lewes and Max Müller, lost ground immediately before the eager onslaught of their younger competitors" (*loc. cit.*, p. 197).

The first who seems to have fully grasped the Darwinian problem from this point of view is Mr Francis Galton,[1] who in a series of papers, and notably in his well-known works on 'Hereditary Genius' (1869) and 37. Galton.
on 'Inheritance' (1889), made a beginning in the statistical treatment of the phenomena of Variation. The novel point of view which was thus introduced into natural science was perhaps somewhat obscured by its immediate application to a most difficult and unique problem, which can hardly be discussed without importing what may be called a sentimental bias. This was the question of the connection through descent of those rare occurrences in human nature which we term genius. Mental phenomena had been almost entirely passed over[2] by Darwin. The results which Mr Galton arrives at, so far as the phenomena of genius are concerned, are of minor importance compared with the general methods which he introduced or suggested for dealing with statistics of heredity. In these he combined the ideas of Quetelet with that remarkable

[1] Mr Francis Galton (born 1822, a grandson of Erasmus Darwin) had, like his celebrated cousin, begun his career as a medical student, and then become a well-known traveller and explorer. Subsequently he devoted himself to meteorology, where he drew attention to the existence and theory of anti-cyclones. His first publication, referring not to physical but to human statistics, appeared in 'Macmillan's Magazine' in 1865, in the shape of two articles on "Hereditary Talent and Character." Here he introduced the "theory of hereditary genius," which was "usually scouted." He rightly claims "to be the first to treat the subject in a statistical manner, to arrive at numerical results, and to introduce the 'law of deviation from an average' into discussions on heredity" (Preface to 'Hereditary Genius,' published one year after Darwin's great work in which was put forward the hypothesis of Pangenesis).

[2] As stated by Darwin himself. See 'Animals and Plants under Domestication' (1868), vol. ii. p. 353.

speculation of Darwin's which he put forward at the end of his work on 'The Variation of Animals and Plants under Domestication' (1868)—the theory of

38. "Pangenesis."

"Pangenesis." "This hypothesis implies that the whole organisation, in the sense of every separate atom or unit, reproduces itself. Hence ovules and pollen-grains, the fertilised seed or egg as well as buds, include and consist of a multitude of germs thrown off from each separate atom of the organism."[1] These germs he calls gemmules, and admits that they agree to some extent with Buffon's organic molecules, only that neither in these nor in Spencer's physiological units does it seem clear that each "independent or autonomous" organic unit, say each cell, throws off or contributes its free gemmule (or gemmules), which is capable of reproducing a similar cell.[2]

The theory of Pangenesis has not found much favour with biologists.[3] For their purposes it would be necessary to define somewhat more clearly what those units or gemmules are. This has accordingly been attempted in several other hypotheses put forward about the same time or somewhat later; each thinker having elaborated, when so inclined, his own fanciful picture, following consciously or unconsciously in the line of Spencer's physiological units. We have in Germany Nägeli's micellar theory, Haeckel's kinetic hypothesis, Prof. Weismann's idioplasma theory, and Prof. Pflüger's theory of the compound organic molecule. All these theories attempt to bring biological phenomena into closer connection with the firmly established conceptions current in physics and chemistry, where atomism and kinetics have been so successfully used in analysing and, to a smaller extent, in putting together the complex processes of nature. Of this I treated in former chapters. But the hypothesis of Darwin is capable of another treatment. Wherever we have to deal with a large, an immense number of single elements or units, which in their totality form certain phenomena, there

39. Lends itself to statistical treatment.

[1] *Loc. cit.*, vol. ii. p. 358.

[2] "Physiologists agree that the whole organism consists of a multitude of elemental parts, which are to a great extent independent of each other" (*loc. cit.*, vol. ii. p. 368). Darwin then quotes Claude Bernard (1866) and Virchow (1860) on the doctrine of the "autonomy" of cells: "I assume that the gemmules in their dormant state have a mutual affinity for each other, leading to their aggregation either into buds or into the sexual elements" (p. 374). "Physiologists maintain, as we have seen, that each cell, though to a large extent dependent on others, is likewise, to a certain extent, independent or autonomous. I go one small step farther, and assume that each cell casts off a free gemmule, which is capable of reproducing a similar cell" (p. 377). "As each unit, or group of similar units throughout the body, casts off its gemmules, and as all are contained within the smallest egg or seed, and within each spermatozoon or pollen-grain, their number and minuteness must be something inconceivable" (p. 378).

[3] Grant Allen dismisses the whole speculation in the following words: "The volume on the variation of animals and plants contained also Darwin's one solitary contribution to the pure speculative philosophy of life—his 'Provisional Hypothesis of Pangenesis,' by which he strove to account, on philosophical principles, for the general facts of physical and mental heredity. Not to mince matters, it was his one conspicuous failure, and is now pretty universally admitted as such. Let not the love of the biographer deceive us; Darwin was here attempting a task *ultra vires*. As already observed, his mind, vast as it was, leaned rather to the concrete than to the abstract side: he lacked the distinctively metaphysical and speculative twist. Strange to say, too, his abortive theory appeared some years later than Herbert Spencer's magnificent all-sided conception of 'Physiological Units,' put forth expressly to meet the self-same difficulty. But while Darwin's hypothesis is rudely materialistic, Herbert Spencer's is built up by an acute and subtle analytical perception of all the analogous facts in universal nature. It is a singular instance of a crude and essentially unphilosophic conception endeavouring to replace a finished and delicate philosophical idea" (*loc. cit.*, p. 126). See also many references to the unfavourable criticisms of Pangenesis in the third volume of the 'Life of Charles Darwin.'

is room for the statistical treatment. This treatment entirely ignores the definite nature of the component units, and merely investigates those properties which depend upon aggregation in large numbers, the average or mean results, and the chances of deviations or variations. Now, if organic beings are supposed to be made up of immeasurably large numbers of units transmitted to them by inheritance, and capable of self-multiplication, they must be subject to certain regularities, to regular deviations or recurrent changes; and, under the influence of selection, be it artificial or automatic, to certain developments which can be studied without a precise knowledge of the biological, chemical, or physical nature of these units themselves, or of the mechanism of their movements. Economics, meteorology, the kinetic theory of gases, deal in this way with complex phenomena, the exact individual history of which they are quite incapable of narrating. As in the case of the kinetic theory of gases we had to translate into statistical language the phenomena of pressure, temperature, volume, available or hidden energy, &c., so in dealing statistically with biological phenomena, such as inheritance, on the basis of the theory of Pangenesis, we have to translate into statistical language such phenomena as "types, sports of nature, stability, variation and individuality." "The word man," as Mr Galton says,[1] "when rightly understood, becomes a noun of multitude, because he is composed of millions, perhaps billions, of cells, each of which possesses in some sort an independent life, and is parent of other cells. He is a conscious

[1] 'Hereditary Genius' (1892), pp. 349, 350.

whole, formed by the joint agencies of a host of what appear to us to be unconscious or barely conscious elements. . . . The doctrine of Pangenesis gives excellent materials for mathematical formulas, the constants of which might be supplied through averages of facts."[1] Mr Galton does "not see any serious difficulty in the way of mathematicians in framing a compact formula, based on the theory of Pangenesis, to express the composition of organic beings in terms of their inherited and individual peculiarities, and to give us, after certain constants had been determined, the means of foretelling the average distribution of characteristics among a large multidude of offspring whose parentage was known.[2] . . . In short, the theory of Pangenesis brings all the influences that bear on heredity into a form that is appropriate for the grasp of mathematical analysis."

Evidently in the mind of Mr Galton the problem of heredity divides itself into two distinct problems; and he has himself laboured at the solution of both. We may call the one the "historical" or the "mechanical" problem, the other the "statistical" problem, following the distinction which Maxwell drew when dealing with the kinetics of gases. The historical problem would involve a more detailed account of the nature of those organic units which the theory of Pangenesis, in common with other similar theories, like those of Buffon and Nägeli, assumes, and of the mechanism by which they unite and are transmitted. If this is impossible, or at all events highly hypothetical, the actual following up—by observation and experiment—of the phenomena of 40. Problem of Heredity.

[1] 'Hereditary Genius' (1892), p. 356. [2] Ibid. p. 358.

variation in special instances would at least allow us to accumulate many interesting life-histories of families of living creatures, and might some day lead to important generalisations. Mr Galton has himself made an attempt to modify and further elaborate the hypothesis of Pangenesis;[1] and Mr William Bateson has given us,

[1] Mr Galton in 1871 advanced certain objections to the theory of Pangenesis, based upon experiments made with the transfusion of blood, and tending to show that blood cannot be the carrier of the germs or gemmules. See a paper read before the Royal Society, March 30, 1871. Darwin did not think Pangenesis had "received its deathblow, though from presenting so many vulnerable points, its life is always in jeopardy" ('Life of Darwin,' vol. iii. p. 195). In 1875 Mr Galton published an article in the 'Contemporary Review,' vol. xxvii. p. 80, entitled "A Theory of Heredity," in which he put what may be termed the atomic theory of life and its propagation into a form in which it might serve as a working formula for statistical research. It is a mistake to look upon any such theory as a biological, mechanical, or historical explanation. For statistical purposes only the scantiest data need be borrowed from biology. There is, however, one very important biological conception which Galton introduced, which is not contained in Darwin's "provisional hypothesis," and which somewhat later became celebrated mainly through the writings of Prof. Weismann. This is the distinction between the germ-plasma and the body-plasma, the former preserving the continuity of life and inheritance, whereas the latter forms the character of the individual, and is probably sterile. In fact, Galton, from a purely statistical point of view, anticipated—as several other naturalists did, from various other aspects—the theory of the differentiation of the germinal from the personal portions or aggregates of life units in the "stirp" or sum-total of organic units of some kind which are to be found in the newly fertilised ovum. Prof. J. A. Thomson ('The Science of Life,' p. 147) gives the following succinct statement of the conception of "stirps": "First. Only some of the germs within the stirp attain development in the cells of the 'body.' It is the dominant germs which so develop. Second. The residual germs and their progeny form the sexual elements or buds. The part of the stirp developed into the 'body' is almost sterile. . . . The continuity is kept up by the undeveloped residual portion. Third. The direct descent is not between body and body, but between stirp and stirp. The stirp of the child may be considered to have descended directly from a part of the stirps of each of its parents; but then the personal structure of the child is no more than an imperfect representation of his own stirp, and the personal structure of each of the parents is no more than an imperfect representation of each of their own stirps. This is a definite expression of the notion that the germinal cells of the offspring are in direct continuity with those of the parents. The antithesis between the 'soma' and the chain of sex-cells is emphasised."

in his 'Materials for the Study of Variation,' a remarkable specimen of the historical treatment of the problem. 41. Mr Bateson's historical treatment. But the aspect we are at present specially interested in is the other one which, in the course of Mr Galton's studies, has presented itself to him with increasing clearness, namely, the bearing which the general laws of averages and statistics have on the facts of inheritance. Thus, in his second main contribution to the subject, which appeared in 1889, twenty years after the earlier work, the statistical problem comes out much more clearly, and quite separated from the mechanical or the historical one. The hypothesis of Pangenesis is retained only as a general scheme which suggested "the idea though not the phrase of particulate inheritance." It was felt to be no longer necessary, for the purpose of the problem, "to embarrass ourselves with any details of theories of heredity beyond the fact that descent either 42. "Particulate" descent. was particulate or acted as if it were so."[1] And what is meant by "particulate" (*i.e.*, "bit by bit") is illustrated in the following expressive manner:[2] "Many of the modern buildings in Italy are historically known to have been built out of the pillaged structures of older days. Here we may observe a column or a lintel serving the same purpose for a second time, and perhaps bearing an inscription that testifies to its origin; while as to the other stones, though the mason may have chipped them here and there and altered their shape a little, few if any came direct from the quarry." "This simile gives a rude though true idea of the exact meaning of Particulate Inheritance—namely, that each piece of the new structure

[1] 'Natural Inheritance,' p. 193. [2] Ibid., p. 8.

is derived from a corresponding piece of some older one, as a lintel was derived from a lintel, a column from a column, a piece of wall from a piece of wall. . . . We appear to be severally built up out of a host of minute particles of whose nature we know nothing, any one of which may be derived from any one progenitor, but which are usually transmitted in aggregates, considerable groups being derived from the same progenitor. It would seem that while the embryo is developing itself, the particles more or less qualified for each new post wait, as it were, in competition to obtain it. Also that the particle that succeeds must owe its success partly to accident of position and partly to being better qualified than any equally well-placed competitor to gain a lodgment. Thus the step-by-step development of the embryo cannot fail to be influenced by an incalculable number of small and mostly unknown circumstances."[1]

Now, wherever we have to do with a very large number of unknown elements which combine to produce a result, we are introduced to those conditions with which the theory of averages and probability deals. The curve of error discovered by Laplace and Gauss to picture the distribution of a large number of observations around the average or mean position, which is taken as the most probable or correct one, comes in as a valuable aid, not in studying the errors of natural growth, but as the graphical illustration of the deviations or variations which cluster around what we call the normal, or with Quetelet the mean, figure. Only the interest is now attached not so much to specifying and defining the

[1] 'Natural Inheritance,' p. 9.

homme moyen as to studying the deviations from this ideal standard. "How little," says Mr Galton,[1] "is conveyed by the bald statement that the average income of English families is £100 a-year, compared with what we should learn if we were told how English incomes were distributed." A crowd of data furnish for the astronomer the material out of which he has to choose the most probable, the correct figure; a crowd of observations furnish for the naturalist the material from which he has to learn how nature deviates from her types and exhibits variations which are the factors of change and development. Thus, under the hands of Mr Galton, the Law of Error becomes a Law of Distribution, and the whole machinery of the doctrine of probabilities, "excogitated for the use of astronomers and others who are concerned with extreme accuracy of measurement, and without the slightest idea, until the time of Quetelet, that they might be applicable to human measures,[2] become the only tools by which an opening can be cut through the formidable thicket of difficulties that bars the path of those who pursue 'the science of man.'"

Hence while most people regard statistics as dull, they become for the naturalist and student of human nature "full of beauty and interest";[3] there is scarcely anything so apt to impress the imagination as the wonderful form of cosmic order expressed by the "law of frequency of error." "It would have been personified by the Greeks, and deified if they had known of it."[4]

[1] 'Natural Inheritance,' p. 35.
[2] Ibid., pp. 55, 62.
[3] Ibid., p. 62.
[4] Ibid., p. 66.

43. Application of theory of error.

Every mathematical instrument, when applied to a novel purpose for which it was not originally invented, "derives as much benefit in its development as it confers through being made use of." Thus Mr Galton's application of the theory of error to the facts of distribution and variation not only enabled him to bring method and order into such questions raised by the Darwinian theory[1] as natural selection,

[1] It is perhaps premature to speak with great confidence of the actual results which have been gained by this novel branch of scientific inquiry, or of the practical importance which these results may have in the future with regard to some of the great social questions. Still, in a history of thought it is of importance to note how, through Mr Galton's writings, the problem of Inheritance has acquired quite a new aspect. This finds expression in his famous so-called "law of filial regression," which goes against "the current belief that the child tends to resemble its parents" (p. 104). In fact, all opinions and theories which had been propounded before Galton, either popularly or scientifically, were based upon a one-sided regard to the more visible portion of the ancestry—viz., the parents; whereas, if any general theory like that of "pangenesis," or of "stirps," or of the "differentiation of the germ-plasma and the body-plasma" be made the basis of discussion, the whole ancestral tree must be considered to contribute to the formation of the characters of any individual. In fact, we have before us not one pair, but an endless line of pairs which are, as the terms of a series, connected by the powers of the number two; and it is then easily seen, without going into refinements (which, however, in the further elaboration of the problem, may become very important), that the first term of the series, which represents the parents, contributes only one-half of the whole, that is, each parent one quarter. It is also evident, if each parent only contributes on the average one quarter, that an exceptional bias in any direction communicated by them would be balanced in the long-run by the opposite action of the remaining ancestry, and that, contrary to ordinary belief, inheritance would operate in the direction of bringing each individual back to the average of the whole lineage. Mr Galton first observed this law of regression to the average by definite countings with seeds and "a comparatively small number of observations of human stature"; and he remarks that if it was only by these experiments and observations that the law of regression had been established, it could not have been expected that the truth of the apparent paradox would be recognised. When, however, the rule was once expressed, it was "easily shown that we ought to expect filial regression, . . . two different reasons for its occurrence" existing—"the one connected with our notions of stability of type, the other as follows: the child inherits partly from his parents, partly from his ancestry. In every population that intermarries freely, when the genealogy of any man is traced far backwards, his ancestry will be found to consist of such varied elements that they are indistinguishable from a sample taken at haphazard from the general population." As to the mathematical problem referred to, it was submitted by Mr Galton in a definite form to Mr J. D. H. Dickson, whose solution is given in the appendix to 'Natural Inheritance.' On this solution Mr Galton remarks: "The problem may not be difficult to an accomplished mathematician, but I certainly never felt such a glow of loyalty and respect towards the sovereignty and wide sway of mathematical analysis as when his answer arrived, confirming, by pure mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope, because the data ran somewhat roughly, and I had to smooth them with tender caution. . . . It is obvious from this close accord of calculation with observation, that the law of Error holds throughout with sufficient precision to be of real service, and that the various results of my statistics are not casual and disconnected determinations, but strictly interdependent" (p. 202). Another passage indicating how much the inferences from the law of regression run contrary to popular opinions on inheritance is the following: "The law of Regression tells heavily against the full hereditary transmission of any gift. Only a few out of many children would be likely to differ from mediocrity so widely as their mid-parent, and still fewer would differ as widely as the more exceptional of the two parents. The more bountifully the parent is gifted by nature, the more rare will be his good fortune if he begets a son who is as richly endowed as himself, and still more so if he has a son who is endowed yet more largely. But the law is even-handed; it levies an equal succession-tax on the transmission of badness as of goodness. If it discourages the extravagant hopes of a gifted parent that his children will inherit all his powers, it no less discountenances extravagant fears that they will inherit all his weakness and disease" (p. 106). Prof. Karl Pearson ('The Grammar of Science,' 2nd ed., p. 479) says of the law of ancestral inheritance: "If Darwinism be the true view of evolution—*i.e.*, if we are to describe evolution by natural selection combined with heredity—then the law which gives us definitely and concisely the type of the offspring in terms of the ancestral peculiarities, is at once the foundation-stone of biology and the basis upon which heredity becomes an exact branch of science."

regression, reversion to ancestral types, extinction of families, effect of bias in marriage, mixture of inheritance, latent elements, and generally to prepare the ground for the combined labours of the naturalist and the statistician; he was also able to put novel problems to the mathematician.

To understand this latter point we must realise the

44. Difference in application to living and lifeless units.

great difference which exists between dealing with a vast number of lifeless and of living units. This difference becomes evident if we consider that in the former case the number of units is unalterable and the units are indestructible; in the latter the elements or units are subject to enormous increase and corresponding destruction, generally with a preponderance of the first. In the kinetic theory of gases we have to consider, in every finite system, the conservation or persistence of mass and motion, the two units we deal with. To these two properties of an immensely large crowd we have to reduce the various phenomena of pressure, temperature, volume, available or unavailable energy. In the vast crowd of gemmules which build up a new organism or regenerate an existing one, we have to deal with a continual influx or creation of new units and a continual extinction and ejection of old or dead ones. Without venturing on any theory as to how this state of things has come about, we may see that the mathematics and statistics of such crowds must be different from those referring to stable, lifeless assemblages. The twofold task arises of formulating the new problems and solving them. To the extent that this is possible we shall be able to deal mathematically with the great problem of variability; and for the practical application of these mathematical formulæ we shall have to collect long series of facts and data of measurements—the material which has to be statistically arranged and sifted, and which is to confirm the conclusions and test the results which calculation has brought out.

Mr Galton found ready, or instituted himself, various countings of large numbers, which formed valuable material for his mathematical schemes, and which confirmed them in a surprising degree. Some very elaborate series of measurements of the varying dimensions of individual members in large crowds of animals were published by Prof. Weldon, whose monograph on Crabs will always remain an historical document.[1] It was noticed about the same time that the attempt to bring the measured deviations from the average into a symmetrical arrangement on the sides of more or less was impossible, and the fact had to be realised and mathematically expressed that special influences tending towards change on the intermixing of different varieties produced an asymmetrical distribution or frequency:[2] in fact, nature works with loaded dice, producing a bias in certain directions; this is the favour which, according to Darwin, Wallace, and Lamarck's ideas, must meet the better fitted individuals and exact from them a smaller tribute in the inevitable process of destruction and removal.

45. Prof. Pearson. The mathematical problem.

We owe it to Prof. Karl Pearson to have first grasped clearly and comprehensively the mathematical problem involved, and to have solved it in a manner useful for

[1] See the 'Proceedings of the Royal Society' since 1890, notably vol. lvii., 1895, p. 360 *sqq.*

[2] "An asymmetrical frequency curve may arise from two quite distinct classes of causes. In the first place the material measured may be heterogeneous, and may consist of a mixture of two or more homogeneous materials. . . . The second class of frequency curves arises in the case of homogeneous material when the tendency to deviation on one side of the mean is unequal to the tendency to deviation on the other side" (Karl Pearson, "On the Mathematical Theory of Evolution," 'Trans. Roy. Soc.,' 1895, p. 344).

biological research.[1] He has thus put into the hands of naturalists an instrument wherewith to describe graphically the observed facts of variation and other allied

[1] A considerable literature has already accumulated in this novel branch of exact inquiry. The complete list of it is given in a pamphlet by Georg Duncker, entitled 'Die Methode der Variationsstatistik' (Leipzig, 1899). From this list (p. 60) it will be seen that one of the earliest workers in the field of biological statistics was the botanist F. Ludwig, whose 'Abschnitte der Mathematischen Botanik' have appeared in various periodicals abroad since the year 1883. The philosopher, however, to whom we are most indebted for the mathematical foundations of the whole theory, is, as noted above, Prof. Karl Pearson, whose "Contributions to the Mathematical Theory of Evolution" have been appearing since the year 1893 in the Trans. of the Royal Society. Very helpful abstracts of these contributions, covering a large field of mathematical theory, and containing elaborate discussions of many of the terms recently introduced into biological science, such as regression, reversion, inheritance, panmixia, selection, &c., will be found in the Proceedings of the Royal Society (1893, onwards). Also in his collected essays, 'The Chances of Death and other Studies in Evolution' (2 vols., 1897); and, lastly, in the later chapters of the second edition of his 'Grammar of Science' (1890). From the latter it will be seen what far-reaching inferences may eventually be drawn from the quantitative treatment and mathematical discussion of biological data; notably the results so far gained "lead us to consider variation as a permanent attribute of living forms, which can hardly have been substantially modified since the beginnings of life. In the same manner we find heredity intimately associated with variation in the individual, and not differing very substantially as we pass from one character to a second, or from one to another form of life. We conclude that variation and inheritance rather precede than follow evolution; they are, at present, *one* fundamental mystery of the vital unit" (p. 502). Prof. Pearson, whose training was that of a mathematician and a lawyer, approached the problems of biology from the exact point of view, and it is interesting to see how, in many ways, he comes to results similar to those arrived at by one of the other great representatives of modern biological research, Mr Wm. Bateson. See his 'Materials for the Study of Variation, treated with especial regard to the discontinuity in the Origin of Species' (1894). If I understand him rightly, his researches have led him to the conclusion that variation cannot be the work of natural selection, since he has given "such evidence as to certain selected forms of variations" as to afford "a presumption that the discontinuity of which species is an expression has its origin, not in the environment, nor in any phenomenon of adaptation, but in the intrinsic nature of organisms themselves, manifested in the original discontinuity of variations" (p. 567). This "disposes, once and for all, of the attempt to interpret all perfection and definiteness of form as the work of selection. . . . It suggests, in brief, that the discontinuity of species results from the discontinu-

phenomena, such as correlation, heredity, regression and panmixia, and he has shown how to analyse these graphical tracings so as to indicate the several possible elements out of which they are compounded, representing separate agencies which are at work in nature. The mathematical inventions of Fourier had similarly enabled physicists to analyse the complicated periodicity of tidal curves into their elements, and, under the hands of Ohm and Helmholtz, to resolve the harmonies of music.

We have here arrived at the last stage of the development of the statistical view of nature. It has been variously judged by biologists according to the special views they take of their problems, and also according

ity of variation" (p. 568). Mr Bateson expects great assistance from the statistical methods. "There is," he says, "no common shell or butterfly of whose variations something would not be learnt, were some hundreds of the same species collected from a few places and statistically examined in respect of some varying character. Any one can take part in this class of work, though few do" (p. 574). Notwithstanding the general resemblance noted above between the ideas of Mr Bateson and of Prof. Pearson, they differ so much in detail as to be led to confess that they do not understand one another's languages. Cf. W. Bateson, "Heredity, Differentiation, and other Conceptions of Biology," 'Roy. Soc. Proc.,' vol. lxix. pp. 193-205; K. Pearson, "On the Fundamental Conceptions of Biology," 'Biometrika,' vol. i. pp. 320-344. Prof. Pearson's view is that, for the working out of the theory of evolution, "biological conceptions can be accurately defined, and so defined measured with quantitative exactness" (*loc. cit.*, p. 324). Mr Bateson, on the other hand, regards them as to some extent out of the reach of mathematical definition and measurement. "Discontinuous variation" in Mr Bateson's special sense—by which we may perhaps understand great as distinguished from small but numerous deviations from the average —Prof. Pearson regards as "statistically negligible for the purpose of vital statistics" (pp. 333, 334). He, in fact, holds closer to Darwinism as understood by Darwin, who never looked with much favour on Huxley's view, for example, that "sports," as distinguished from the sum of small differences in individuals, might furnish an appreciable part of the materials for natural selection. Mr Bateson's view found favour with Huxley, as may be observed in the 'Life and Letters.' On the novelty and value of Prof. Pearson's methods, see also the Address by Prof. Weldon to the Zoological Section of the British Association in 1898.

to the degree in which they appreciate and are able to grasp mathematical methods. The subject is still under discussion, and will belong to the History of Thought of a coming age. It is enough to have indicated the latest lines of reasoning which our century has marked out, and to notice how they form a new and remarkable instance of the growth and diffusion of the exact or mathematical spirit in a department of research hitherto almost untouched by it, prepared though it has been for such treatment by one among whose great endowments a grasp of mathematical reasoning hardly formed a distinctive feature. In former chapters I have had occasion to show how Charles Darwin introduced into the science of nature two novel points of view—the genetic view and the process of judicial sifting of evidence. We may now add that he has indirectly, more than directly, furthered quite as much the statistical view of natural phenomena through which we have learned to find and trace law and order in great realms of phenomena and events usually supposed to be governed by what is termed blind chance. The study of this blind chance in theory and practice is one of the greatest scientific performances of the nineteenth century.

46. Statistical knowledge one-sided.

But whilst acknowledging the great importance which the statistical treatment of phenomena has acquired in our age, and the value of the statistical view of many large departments of natural processes which escape almost every other mode of dealing with them, we must not forget that it is essentially one-sided.

Clerk-Maxwell has suggestively opposed it alike to the mechanical and the historical views, of which the former

tries to describe the general mechanism *under* which, the latter the individual steps and incidents *by* which, special events or phenomena proceed and are characterised. Earlier chapters of this narrative attempted to give an account of the former, whilst the essentially historical treatment belongs to another portion of the work. The word history has generally been reserved for records which deal with those events in which human consciousness has played a large, if not an overwhelming, part, and has been able to assist the observer by its own accounts and representations. What should we know of human life and human interests without them, and how helpless—in spite of minutest observation—do we still appear to be in understanding the life of the brute and mute creation, even of the domestic animals, our daily friends and companions? But if history, as opposed to statistics, really seems only possible where the living voice or the surviving narrative of those who have departed helps us to a true understanding of its incidents and its meaning, it also imposes upon us the task of sifting its value and trustworthiness critically. Mathematics, logic, and statistics may do something to exclude the actually impossible or the highly improbable from a vast mass of material; but more delicate criteria are required in dealing with the accumulated testimony of bygone ages. With an unerring instinct of what, in addition to mathematical measurements, may be required in order to accomplish this task, the nineteenth century has not only nursed the scientific spirit and cultivated its methods, but with equal diligence and originality those other methods which lie at the foundation of

47. Critical methods. all recent philosophical thought — the methods of criticism.

And yet, before taking leave of science and entering on a comprehensive appreciation of the workings of the Critical Spirit with which all our thought seems to be permeated, I owe to my readers the attempt to answer one remaining question. If it be true, as the foregoing narrative has abundantly insisted, that through the increasing application of mathematical methods of measuring and calculating, our thought has become truly scientific and our knowledge accurate and useful for describing and predicting phenomena, as also for manifold practical applications, we may be curious to know whether the refined instrument, mathematical thought itself, has been subject to such change and development as has been undergone by the various branches of science to which it has been applied. In fact, we have to ask the question, How has mathe-

48. The instrument of exact research. matical thought itself fared in the course of the nineteenth century? The concluding chapter of the present volume will try to give a reply to this question.

CHAPTER XIII.

ON THE DEVELOPMENT OF MATHEMATICAL THOUGHT DURING THE NINETEENTH CENTURY.

IN venturing upon the last and most abstract portion of the great domain of Scientific Thought of the century, it may be well to remind the reader that it is not a history of science but a history of thought that I am writing. When dealing in the foregoing chapters with manifold discoveries, drawn promiscuously from the various natural sciences, I have done so only to show how the scientific mind has, in the course of the period, come to regard the things of nature from different points of view, and to think and reason on them differently. Such changes have frequently been brought about by the discovery of novel facts, but this alone has not generally sufficed to mark also a change in the manner of reasoning on and thinking about them. The increase in the number of natural species, of the chemical elements or of the smaller planets, has not necessarily made us think differently about these things in themselves: the theory and point of view may change without any change in the object towards which they are directed, 1. History of thought.

for they mark more the attitude of the beholder than the things which he regards. It is true that a very small addition to our actual knowledge of facts, like the sudden appearance of some characteristic feature in a landscape, may sometimes entirely alter the whole aspect, induce us to abandon our accustomed views, and call up suddenly an unforeseen train of ideas; in such a case, perhaps, this insignificant discovery becomes historically interesting, although it is mainly by the altered trains of thought which it has evoked that it has become important to us.

2. Difference between thought and knowledge.

The difference of scientific knowledge and scientific thought is thus owing to the two factors which are involved—the facts of science or nature on the one side and the scientifically thinking mind on the other. Now it might appear as if this difference vanished when we approach the abstract science of mathematics, or at least that of number; for in numbering and counting we have really only to do with a process of thought, and it would seem as if the science of number were itself the science of thought, or at least a portion of it. In fact, the question arises, Is there any difference between mathematical science and mathematical thought? Some considerations might induce us to think that there is not. On the other side, I shall try to show in this chapter that there is, and that the development of mathematics during our period has brought this out very clearly and prominently.

3. Popular prejudices regarding mathematics.

There is an opinion current among many thinking persons who have not occupied themselves with mathematical science, though they may be very efficient in

calculating and measuring, that there is really nothing new in mathematics, that two and two always make four, that the sum of the angles in a triangle always make two right angles, and that all progress in mathematics is merely a question of intricacy, a never-ending process of increased complication by which you can puzzle even the cleverest calculator. To them the history of mathematics would be something analogous to the history of games like whist or chess, the resources and complications of which seem to be inexhaustible. So they think[1] that the intricacies and refinements of elementary and higher mathematics will supply endless material for training the minds of schoolboys or trying the ingenuity

[1] "Some people have been found to regard all mathematics, after the 47th proposition of Euclid, as a sort of morbid secretion, to be compared only with the pearl said to be generated in the diseased oyster, or, as I have heard it described, 'une excroissance maladive de l'esprit humain.' Others find its justification, its *raison d'être*, in its being either the torch-bearer leading the way, or the handmaiden holding up the train of Physical Science; and a very clever writer in a recent magazine article expresses his doubts whether it is, in itself, a more serious pursuit, or more worthy of interesting an intellectual human being, than the study of chess problems or Chinese puzzles. What is it to us, they say, if the three angles of a triangle are equal to two right angles, or if every even number is, or may be, the sum of two primes, or if every equation of an odd degree must have a real root? How dull, stale, flat, and unprofitable are such and such like announcements! Much more interesting to read an account of a marriage in high life, or the details of an international boat-race. But this is like judging of architecture from being shown some bricks and mortar, or even a quarried stone of a public building, or of painting from the colours mixed on the palette, or of music by listening to the thin and screech sounds produced by a bow passed haphazard over the strings of a violin. The world of ideas which it discloses or illuminates, the contemplation of divine beauty and order which it induces, the harmonious connexion of its parts, the infinite hierarchy and absolute evidence of the truths with which it is concerned, these, and such like, are the surest grounds of the title of mathematics to human regard, and would remain unimpeached and unimpaired were the plan of the universe unrolled like a map at our feet, and the mind of man qualified to take in the whole scheme of creation at a glance" (Prof. J. J. Sylvester, Address before Brit. Assoc., see 'Report,' 1869, p. 7).

of senate-house examiners and examinees, without for a moment considering the question whether mathematical thought as distinguished from mathematical problems is capable of and has undergone any radical and fundamental change or development.

4. Use of mathematics.

Closely allied with this is the further question as to the use of mathematics. Two extreme views have always existed on this point.[1] To some, mathematics is only a measuring and calculating instrument,[2] and their interest

[1] Of the two greatest mathematicians of modern times, Newton and Gauss, the former can be considered as a representative of the first, the latter of the second class; neither of them was exclusively so, and Newton's inventions in the pure science of mathematics were probably equal to Gauss's work in applied mathematics. Newton's reluctance to publish the method of fluxions invented and used by him may perhaps be attributed to the fact that he was not satisfied with the logical foundations of the calculus; and Gauss is known to have abandoned his electro-dynamic speculations, as he could not find a satisfactory physical basis (see *supra*, p. 67). Others who were not troubled by similar logical or practical scruples stepped in and did the work, to the great benefit of scientific progress. Newton's greatest work, the 'Principia,' laid the foundation of mathematical physics; Gauss's greatest work, the 'Disquisitiones Arithmeticæ,' that of higher arithmetic as distinguished from algebra. Both works, written in the synthetic style of the ancients, are difficult, if not deterrent, in their form, neither of them leading the reader by easy steps to the results. It took twenty or more years before either of these works received due recognition; neither found favour at once before that great tribunal of mathematical thought, the Paris Academy of Sciences. Newton's early reputation was established by other researches and inventions, notably in optics; Gauss became known through his theoretical rediscovery of Ceres, the first of the minor planets (see above, vol. i. p. 182). The country of Newton is still pre-eminent for its culture of mathematical physics, that of Gauss for the most abstract work in mathematics. Not to speak of living authorities, I need only mention Stokes and Clerk-Maxwell on the one side, Grassmann, Weierstrass, and Georg Cantor on the other.

[2] Huxley said: "Mathematics may be compared to a mill of exquisite workmanship which grinds you stuff of any degree of fineness: but, nevertheless, what you get out depends on what you put in; and as the grandest mill in the world will not extract wheat-flour from peas-cods, so pages of formulæ will not get a definite result out of loose data"; and on another occasion he said that mathematics "is that study which knows nothing of observation, nothing of induction, nothing of experiment, nothing of causation." The former statement was endorsed by Lord Kelvin ('Pop. Lectures,' &c., vol. ii. p. 102); the latter was energetically repudiated by Sylvester in his famous Address to the first section of the British Assoc. at Exeter (1869, 'Report,' &c., p. 1, &c.)

ceases as soon as discussions arise which cannot benefit those who use the instrument for the purposes of application in mechanics, astronomy, physics, statistics, and other sciences. At the other extreme we have those who are animated exclusively by the love of pure science. To them pure mathematics, with the theory of numbers[1] at the head, is the one real and genuine science, and the applications have only an interest in so far as they contain or suggest problems in pure mathematics. They are mainly occupied with examining and strengthening the foundations of mathematical reasoning and purifying its methods, inventing rigorous proofs, and testing the validity and range of applicability of current conceptions. We may say that the former are led by practical, the latter by philosophical, interests, and these latter may be either logical or ontological,[2]

[1] Gauss considered mathematics to be "the Queen of the Sciences, and arithmetic the Queen of Mathematics. She frequently condescends to do service for astronomy and other natural sciences, but to her belongs, under all circumstances, the foremost place" (see 'Gauss zum Gedächtniss,' by Sartorius von Waltershausen, Leipzig, 1856, p. 79). Cayley's presidential Address to the British Association, 1883, has been frequently quoted: "Mathematics connect themselves on one side with common life and the physical sciences; on the other side with philosophy in regard to our notions of space and time and the questions which have arisen as to the universality and necessity of the truths of mathematics, and the foundation of our knowledge of them. I would remark here that the connection (if it exists) of arithmetic and algebra with the notion of time is far less obvious than that of geometry with the notion of space" ('Mathematical Papers,' vol. xi. p. 130). In addition to founding higher arithmetic, Gauss occupied himself with the foundations of geometry, and, as he expected much from the development of the theory of numbers, so he placed "great hopes on the cultivation of the *geometria situs*, in which he saw large undeveloped tracts which could not be conquered by the existing calculus" (Sartorius, *loc. cit.*, p. 88).

[2] To this might be added the psychological interest which attaches to mathematical conceptions. The late Prof. Paul Du Bois-Reymond occupied himself

inasmuch as number and form are considered to be the highest categories of human thought, or likewise as the ultimate elements of all reality. These two interests existed already in antiquity,[1] as the word "geometry"

5. Twofold interest in mathematics.

much with the question. See the following works: 'Die Allgemeine Functionentheorie,' part i., Tübingen, 1882; 'Ueber die Grundlagen der Erkenntniss in den exacten Wissenschaften,' Tübingen, 1890; and his paper "Ueber die Paradoxien des Infinitärcalcüls" ('Mathematische Annalen,' vol. ix. p. 149). In addition to the two main interests which attach to mathematical research, and which I distinguish as the practical and the philosophical, a third point of view has sprung up in modern times which can be called the purely logical. It proposes to treat any special development of mathematical research with the aid of a definite, logically connected complex of ideas, and not to be satisfied to solve definite problems with the help of any methods which may casually present themselves, however ingenious they may be. In this way the great geometrician, Jacob Steiner, *e.g.*, refused the assistance of analysis in the solution of geometrical problems, conceiving geometry as a complete organism which should solve its problems by its own means. This view has been much strengthened by the development in modern times of the theory of Groups; a group of operations being defined as a sequence of such operations as always lead back again to operations of the same kind. Mathematical rigorists in this sense would look upon the use of mixed methods or operations not belonging to the same group with that kind of disfavour with which we should regard an essayist who could not express his ideas in pure English, but was obliged to import foreign words and expressions. It is interesting to see that the country which has offended most by the importation of foreign words—namely, Germany—is that in which this purism in mathematical taste has found the most definite expression. (See, *inter alia*, Prof. Friedrich Engel's Inaugural Lecture, "Der Geschmack in der neueren Mathematik," Leipzig, 1890, as also Prof. F. Klein's suggestive tract, 'Vergleichende Betrachtungen über neuere Geometrische Forschungen,' Erlangen, 1872.)

[1] The literature of this subject is considerable. I confine myself to two works. The late eminent mathematician, Hermann Hankel, of whom more in the sequel of this chapter, besides showing much originality in the higher branches of the science, took great interest in its philosophical foundations and historical beginnings. In 1870 he published a small but highly interesting volume, 'Zur Geschichte der Mathematik in Alterthum und Mittelalter' (Leipzig, Teubner). We have, besides, the great work of Prof. Moritz Cantor, 'Vorlesungen über Geschichte der Mathematik,' in three large volumes (Leipzig, Teubner). It brings the history down to 1758. Referring to the two interests which led to mathematical investigations, Hankel says (p. 88): "From the moment that Greek philosophers begin to attract our attention through their mathematical researches, the aspect which mathematics present changes radically. Whilst among the earlier civilised nations we only meet with routine and practice, with empirical rules which served practical purposes in an isolated manner, the Grecian mind on the other side recognised, from the first moment when it became acquainted with this matter, that it contained something which transcended all those practical ends, but which was worthy of special attention, and which could be expressed in a general form, being, in fact, an object of science. This is the high merit of the Greek mathematicians; nor need one fear that this merit should be diminished by admitting that they borrowed the new material from the ancient Egyptian civilisation."

and the well-known references to mathematical ideas in the schools of Pythagoras and Plato indicate. An ancient fragment[1] which enumerates briefly the Grecian mathematicians, says of Pythagoras, "He changed the occupation with this branch of knowledge into a real science, inasmuch as he contemplated its foundation from a higher point of view, and investigated the theorems less materially and more intellectually;"[2] and of Plato it says that "He filled his writings with mathematical discussions, showing everywhere how much of geometry attaches itself to philosophy."[3]

This twofold connection of mathematical with other pursuits has, after the lapse of many centuries, come prominently forward again in the nineteenth century. We have already had to record a powerful stimulus to mathematical thought in almost every chapter in which we dealt with the fruitful ideas which governed scientific work, and we have now no less to draw attention to the philosophical treatment which has been bestowed upon the foundations of science and the inroad of mathemati-

[1] The fragment referred to is preserved by Proclus, and is given in full in Cantor's work (vol. i. p. 124 *sqq.*) He calls it an ancient catalogue of mathematicians. It is generally attributed to Eudemus of Rhodes, who belonged to the peripatetic school of philosophy, and was the author of several historical treatises on geometry and astronomy (Cantor, vol. i. p. 108).

[2] Cantor, vol. i. p. 137.

[3] Ibid., p. 213.

cal into philosophical thought;[1] so much so that this closing chapter on the development of mathematical thought forms a fitting link with the next great department of our subject—the Philosophy of the Century.

6. Origin of mathematics. We are told that mathematics among the Greeks had its origin in the Geometry invented by the ancient Egyptians for practical surveying purposes. The first mathematical problems arose in the practice of mensuration. Modern mathematical thought received in an analogous manner its greatest stimulus through the Uranometry of Kepler, Newton, and Laplace: through the mechanics and the survey of the heavens new methods for solving astronomical problems were invented in the seventeenth and eighteenth centuries, and the nineteenth century can be said to have attempted to perform towards this new body of doctrine the same task that Euclid, three hundred years before the Christian era, performed towards the then existing mathematics. As Proclus tells us, "putting together the elements, arranging much from Eudoxus, furnishing much from Theætetus, he, moreover, subjected to rigorous proofs what had been negligently demonstrated by his predecessors."[2] What one man, so far as we know, did for the Grecian science, a number of great thinkers in

[1] Thus, for instance, the recent investigations and theories of the "manifold," as they have been set forth by Prof. Georg Cantor of Halle, constitute, as it were, a new chapter in mathematical science, whereas they were formerly a subject merely of philosophical interest. See a remark to this effect by B. Kerry at the end of his very interesting article on Cantor's doctrine in the 9th vol. of Avenarius's 'Zeitschrift für wissenschaftliche Philosophie' (1885), p. 231, where he refers to Kant's comparison of philosophy to a Hecuba "tot generis natisque potens."

[2] Quoted by Cantor, vol. i. p. 247. See also Hankel, *loc. cit.*, p. 381 *sqq.*

our century, among whom I only mention Gauss, Cauchy, and Weierstrass, attempted to do for the new science which was created during the two preceding centuries. As Prof. Klein says, "We are living in a critical period, similar to that of Euclid."[1]

[1] See 'The Evanston Colloquium, Lectures on Mathematics delivered in August and September 1893,' by Felix Klein, notably Lecture vi. In this lecture Prof. Klein explains his view (to which he had given utterance in his address before the Congress of Mathematics at Chicago: 'Papers published by the American Mathematical Society,' vol. i. p. 133. New York, 1896) on the relation of pure mathematics to applied science. This view is based upon the distinction between what he calls the "naïve and the refined intuition." . . . "It is the latter that we find in Euclid; he carefully develops his system on the basis of well-formulated axioms, is fully conscious of the necessity of exact proofs, clearly distinguishes between the commensurable and the incommensurable, and so forth. . . . The naïve intuition, on the other hand, was especially active during the period of the genesis of the differential and integral calculus. Thus we see that Newton assumes without hesitation the existence, in every case, of a velocity in a moving point, without troubling himself with the inquiry whether there might not be continuous functions having no derivative."

In the opinion of Prof. Klein "the root of the matter lies in the fact that the naïve intuition is not exact, while the refined intuition is not properly intuition at all, but arises through the logical development from axioms considered as perfectly exact."

In the sequel Prof. Klein shows that the naïve intuition imports into the elementary conceptions elements which are left out in the purely logical development, and that this again leads to conclusions which are not capable of being verified by intuition, no mental image being possible. Thus, for instance, the abstract geometry of Lobatchevsky and Riemann led Beltrami to the logical conception of the pseudo-sphere of which we cannot form any mental image. Similar views to those of Prof. Klein have been latterly expressed by H. Poincaré in his suggestive volume 'La Science et l'Hypothèse' (Paris, 1893). He there says (p. 90): ". . . L'expérience joue un rôle indispensable dans la genèse de la géométrie; mais ce serait une erreur d'en conclure que la géométrie est une science expérimentale, même en partie. . . . La géométrie ne serait que l'étude des mouvements des solides; mais elle ne s'occupe pas en réalité des solides naturels, elle a pour objet certains solides idéaux, absolument invariables, qui n'en sont qu'une image simplifiée et bien lointaine. . . . Ce qui est l'objet de la géométrie c'est l'étude d'un 'groupe' particulier; mais le concept général de groupe préexiste dans notre esprit au moins en puissance. . . . Seulement, parmi tous les groupes possibles, il faut choisir celui qui sera pour ainsi dire l'étalon auquel nous rapporterons les phénomènes naturels." This distinction between the mathematics of intuition and the mathematics of logic has also been forced upon us from quite a different quarter. The complica-

7. Gauss. It is right to place the name of Gauss at the head, for his investigations regarding several fundamental and critical questions in arithmetic and geometry date from the last years of the eighteenth century, long before Cauchy's influence made itself felt. This is now abundantly clear through the publication of Gauss's works, and from much of his correspondence with personal friends, notably with the astronomer Bessel. We can now understand how those who knew him regarded him as a kind of mathematical oracle to whom "nothing in theory existed that he had not looked at from all sides,"[1] and who anticipated in his own mind the development which mathematical thought was to take for a long time after him. And yet it was not to him primarily that the great change was due which came over mathematical reasoning during the first half of the century. Gauss was not a great teacher. In fact, there existed in the first quarter of the period only one great training school in advanced mathematics, and that was Paris.

8. Cauchy. There it was that Augustin Cauchy—first as lecturer,

tion of modern mathematics and the refinement of the modern theories have brought about the desire "to create an abridged system of mathematics adapted to the needs of the applied sciences, without passing through the whole realm of abstract mathematics" (Klein, *loc. cit.*, p. 48). In this country Prof. Perry has made a beginning by publishing his well-known work, 'Calculus for Engineers,' which has been welcomed by Prof. Klein in Germany, and which has led to an extensive correspondence in the pages of 'Nature'; it being recognised by many that a quicker road must be made from the elements to the higher applications of mathematics in the natural sciences than the present school system, beginning with Euclid, admits of. The separation of the logical and practical treatment of any science, as likewise the independent development in Germany of the polytechnic school alongside of the university, has, however, its dangers, as is recognised by Prof. Klein ('Chicago Mathematical Papers,' p. 136).

[1] See Bessel's letter to Gauss, 27th December 1810, in 'Briefwechsel zwischen G. and B., Leipzig, 1880, p. 132.

then as professor—exerted his great influence in the famous École Polytechnique, in the Sorbonne, in the Collége de France.[1] In contrast with Gauss—who was self-contained, proud, and unapproachable, whose finished and perfect mathematical tracts were, even to those who worshipped him, an abomination,[2] owing to their unintelligible and novel enunciation, who hated lecturing—Cauchy possessed the enthusiasm and patience of the teacher,[3] spent hours with his pupils, and published his lectures on the foundations of the Calculus for the benefit of the rising mathematical generation. Thus he has the merit of having created a new school of mathematical thought—not only in France but also abroad, where the greatest intellects, such as that of Abel,[4] expressed themselves indebted to him for having pointed out the only right road of progress. It will be useful to define somewhat more closely wherein this new school differed from that preceding it, which culminated in the great names of Euler, Lagrange, and Laplace.

The great development of modern as compared with ancient mathematics may be stated as consisting in the in-

[1] See Valson, 'La Vie et les Travaux du Baron Cauchy,' Paris, 1868, vol. i. p. 60 *sqq.*

[2] "On disait que sa manière d'exposer était mauvaise, ou encore qu'il faisait comme le renard, qui efface avec sa queue les traces de ses pas sur le sable. Crelle dit, selon Abel, que tout ce qu'écrit Gauss n'est qu'abomination (Gräuel), car c'est si obscur qu'il est presque impossible d'y rien comprendre" (Bjerknes, 'Niels Henrik Abel,' Trad. française, Paris, 1885, p. 92).

[3] "C'est que Cauchy alliait au génie des Euler, des Lagrange, des Laplace, des Gauss, des Jacobi, l'amour de l'enseignement porté jusqu'à l'enthousiasme, une rare bonté, une simplicité, une chaleur de cœur qu'il a conservées jusqu'à la fin de sa vie" (Combes, quoted by Valson, vol. i. p. 63).

[4] See Bjerknes, 'N.-H. Abel,' p. 48 *sqq.*; p. 300. Cauchy's 'Cours d'Analyse' appeared in 1821; the 'Résumé des leçons sur le calcul infinitésimal,' to which Abel refers in a letter to Holmboe, dated 1826, appeared in 1823.

troduction of algebra or general arithmetic, in the application of this to geometry and dynamics, and in the invention of the infinitesimal methods, through which the rigorous theorems of the older geometricians which referred to the simpler figures—such as straight lines, circles, spheres, cones, &c.—became applicable to the infinite variety of curves and surfaces in which the objects and phenomena of nature present themselves to our observation. Logically speaking, it was a grand process of generalisation, based mostly on inference and induction, sometimes merely on intuition.[1] Such a process of generalisation has a twofold effect on the progress of science.

9. Process of generalisation.

The first and more prominent result was the greatly increased power of dealing with special problems which the generalised method affords, and the largely increased field of research which it opened out. We may say that the century which followed the inventions of Descartes, Newton, and Leibniz, was mainly occupied in exploring the new field which had been disclosed, in formulating and solving the numberless problems which presented themselves on all sides; also, where complete and rigorous solutions seemed unattainable, in inventing methods of approximation which were useful for practical purposes. In this direction so much had to be done, so much work lay ready to hand, that the second and apparently less practical effect of the new generalisations receded for a time into the background. We may term

[1] "On se reportait inconsciemment au modèle qui nous est fourni par les fonctions considérées en mécanique et on rejetait tout ce qui s'écartait de ce modèle ; on n'était pas guidé par une définition claire et rigoureuse, mais par une sorte d'intuition et d'obscur instinct" (Poincaré, "L'œuvre math. de Weierstrass," 'Acta Mathematica,' vol. xxii. p. 4).

this second and more hidden line of research the logical side of the new development. It corresponds to the work which Euclid performed in ancient geometry, the framing of clear definitions and of unambiguous axioms; proceeding from these by rigorous reasoning to the theorems of the new science.[1] But the translation of geometrical and mechanical conceptions into those of generalised arithmetic or algebra brought with it a logical problem of quite a novel kind which has given to modern mathematics quite a new aspect. This new problem is the retranslation of algebraical—*i.e.*, of general—formulæ into geometrical conceptions—the geometrical construction of algebraical expressions. It is the inverse operation of the former. In this inversion of any given operation lies the soul and principle of all mathematical progress, both in theory and in application.[2] The invention of

10. Inverse operations.

[1] Referring specially to the definition of a "function" or mathematical dependence, a conception introduced by Euler, but not rigorously defined by him, M. Poincaré says, *loc. cit.*: "Au commencement du siècle, l'idée de fonction était une notion à la fois trop restreinte et trop vague. . . . Cette définition, il fallait la donner : car l'analyse ne pouvait qu'à ce prix acquérir la parfaite rigueur." In its generality this task was performed in the last third of the century by Weierstrass, but the necessity of this criticism of the formulæ invented by modern mathematics dates from the appearance of Cauchy's 'Mémoire sur la théorie des intégrales définies' of 1814, which Legendre reported on in this sense, but which was not published till 1825.

[2] The operations referred to are generally of two kinds: first, there is the operation of translating geometrical relations, intuitively given, into algebraical relations; and, secondly, the operation of extending algebraical relations by going forward or backward in the order of numbers, usually given by indices. In each case the new relations arrived at require to be interpreted, and this interpretation leads nearly always to an extension of knowledge or to novel conceptions. A simple example of the first kind presents itself in the geometrical construction of the higher powers of quantity. Having agreed to define by a the length of a line, by a^2 an area, what is the meaning of a^3 a^4 . . . a^n? Can any geometrical meaning be attached to these symbols? An example of the

the seventeenth century afforded two grand occasions for such progress, and the creation through it of novel mathematical ideas. The translation of geometrical con-

second class is the following: having defined the symbols

$$\frac{dy}{dx}, \frac{d^2y}{dx^2} \cdots \frac{d^ny}{dx^n},$$

an operation suggests itself in the inverse order, the indices or their reciprocals (inversions) being taken negatively. Can any meaning be attached to these latter symbols? Further, if the operation denoted by going on from one of these symbols to the next is known and feasible, how can the inverse operation be carried out? In the first class of problems we proceed from an intuitively given order to a purely logical order, and have in the sequel to go back from the purely logical order to an intuitive order of ideas. In the second case, having followed a certain logical order, we desire to know what the inversion of this order will produce and how it can be carried out. The view that the direct and indirect processes of thought form the basis of all mathematical reasoning, and an alternation of the two the principle of progress, has been for the first time consistently expounded by Hermann Hankel in his 'Theorie der Complexen Zahlen-Systeme,' Leipzig, 1867. But it had already been insisted on by George Peacock in his "Report," &c., contained in the 3rd vol. of the 'Reports of the Brit. Assoc.,' 1833, where he says (p. 223): "There are two distinct processes in Algebra, the direct and the inverse, presenting generally very different degrees of difficulty. In the first case, we proceed from defined operations, and by various processes of demonstrative reasoning we arrive at results which are general in form though particular in value, and which are subsequently generalised in value likewise; in the second, we commence from the general result, and we are either required to discover from its form and composition some equivalent result, or, if defined operations have produced it, to discover the primitive quantity from which those operations have commenced. Of all these processes we have already given examples, and nearly the whole business of analysis will consist in their discussion and development, under the infinitely varied forms in which they will present themselves."

It is extraordinary how little influence this very interesting, comprehensive, and up-to-date report on Continental mathematics, including the works of Gauss, Cauchy, and Abel, seems to have had on the development of English mathematics. But the latter have through an independent movement—viz., the invention of the Calculus of Operations—led on to the radical change which has taken place in recent mathematical thought. This change, which can be explained by saying that the science of Magnitude must be preceded by the doctrine of Forms or Relations, and that the science of Magnitude is only a special application of the science of Forms, was independently prepared by Hermann Grassman, of whom Hankel says (*loc. cit.*, p. 16): "The idea of a doctrine of Forms which should precede a doctrine of Magnitude, and of considering the latter from the point of view of the former, . . . remained of little value for the development

ceptions into algebraical language suggested the inverse operation of interpreting algebraical terms by geometrical conceptions, and led to an enormous extension of geometrical knowledge.[1] Further, the infinitesimal methods through which curves and curved surfaces were conceived as being made up of an infinite number of infinitesimally small, straight—*i.e.*, measurable—lines, led to the inverse problem; given any algebraical operations which obtain only in infinitesimally small dimensions—*i.e.*, at the limit—how do they sum up to finite quantities and

of mathematics, so long as it was only used to prove theorems which besides being already known, were sufficiently though merely empirically proved. It was H. Grassmann who took up this idea for the first time in a truly philosophical spirit and treated it from a comprehensive point of view." Hankel also refers to Peacock as well as to De Morgan, whose writings, however, he was insufficiently acquainted with (ibid., p. 15). In quite recent times Mr A. N. Whitehead has conceived "mathematics in the widest signification to be the development of all types of formal, necessary, deductive reasoning," and has given a first instalment of this development in his 'Treatise on Universal Algebra' (vol. i., Cambridge, 1898). See the preface to this work (pp. 6, 7).

[1] A good example of the use of the alternating employment of the intuitive (inductive) and the logical (deductive) methods is to be found in the modern doctrine of curves. The invention of Descartes, by which a curve was represented by an equation, led to the introduction of the conception of the "degree" or "order" of a curve and its geometrical equivalent; whereas the geometrical conception of the tangent to a curve led to the distinction of curves according to their "class," which was not immediately evident from the equation of the curve but which led to other analytical methods of representation where the tangential properties of curves became more evident. A third method of studying curves was introduced by Plücker (1832), who started from "the singularities" which curves present, defined them, and established his well-known equations. A further study of these "singularities" led to the notion of the "genus" or "deficiency" (Cayley) of a curve. The gradual development of these and further ideas relating to curves is concisely given in an article by Cayley on "Curve" in the 6th vol. of the 'Encyclopædia Britannica,' reprinted in Cayley's collected papers, vol. xi. This article furnishes also a good example of the historical treatment of a purely mathematical subject by showing, not so much the progress of mathematical knowledge of special things, as the development of the manner in which such things are looked at —*i.e.*, of mathematical thought.

figures? What are the properties of these finite figures as inferred from the properties of their infinitesimally small parts? The infinitesimal methods evidently corresponded with the atomistic view of natural objects, according to which the great variety of observable phenomena, the endlessly complicated properties of natural objects, could be reduced to a small number of conceivable properties and relations of their smallest parts, and could then be made intelligible and calculable.

The general reader who is unacquainted with the numberless problems and intricate operations of higher mathematics can scarcely realise how in these few words lie really hidden the great questions of all the modern sciences of number and measurement; the trained mathematical student will recognise in a process of inversion not only the rationale of such extensive doctrines as the integral calculus, the calculus of variations, the doctrine of series, the methods of approximation and interpolation, but also the application of analysis to geometry, the theory of curves of higher order, the solution of equations, &c. All these various branches were diligently cultivated by the great mathematicians of the eighteenth century, mostly, however, with the object of solving definite problems which were suggested by the applied sciences,[1]

[1] In general it can be stated that the impetus given to mathematical research by the problems set by the applied sciences has been immeasurably greater than that which can be traced to the abstract treatment of any purely mathematical subject. We have a good example of this at the beginning of the nineteenth century in the great work of Laplace as summed up, for the most part, in the 'Mécanique Celeste' and the 'Théorie des Probabilités,' which contain the beginnings and the development of a great number of purely mathematical theories suggested by problems in astronomy, physics, and statistics. On the other side we have at the same time the so-called "Combinational School" in Germany, whose members and

notably astronomy—not infrequently also as objects of mere curiosity without any practical purpose whatever. In the latter part of the eighteenth century the need was felt of putting the new science into a comprehensive system. The attempts to do this—notably the great text-books of Leonhard Euler in Germany and of Lacroix in France—revealed how uncertain were the foundations and how paradoxical some of the apparent conclusions of the reasoning which, in the hands of the great inventors and masters, had led to such remarkable results.

As in other cases which we dealt with in former chapters of this work, so also in the present instance we may find a guide through the labyrinth of modern mathematical thought in the terms of language around which cluster the more recent doctrines. Two terms present themselves which were rare or altogether absent in older treatises: these terms are the "complex quantity" and the "continuous." To these we can add a third term which we meet with on every page of the writings of mathematicians since Newton and Leibniz, but which has only very recently been subjected to careful analysis and rigorous definition,—the term "infinite." Accordingly we may say that the range of mathematical thought during

11. Modern terms indicative of modern thought.

their labours are almost forgotten, although in their elaborate treatises there are to be found many formulæ which had to be rediscovered when, fifty years later, the general theory of forms and substitutions began to be systematically developed, and proved to be an indispensable instrument in dealing with many advanced mathematical problems. See on the latter subject an article by Major MacMahon on "Combinational Analysis" ('Proc., London Math. Soc.,' vol. xxviii. p. 5, &c.), as also the chapters on this subject and on "Determinants" in the first vol. of the 'Encyclopädie der Mathematischen Wissenschaften' (Leipzig, 1898). Also, *inter alia*, a note by J. Muir in 'Nature,' vol. lxvii., 1903, p. 512.

the last hundred years has grown in proportion to the methodical study and stricter definition of the notions of the complex quantity, of the continuous, and of the infinite. And these conceptions indicate three important logical developments which characterise modern mathematical reasoning. The conception of the complex quantity or the complex unit introduces us to the possible extension of our system of counting and measuring, retaining or modifying, the fundamental rules on which it is based. The conception of the continuous and its opposite, the discontinuous, introduces us to the difference of numbers and quantity, numbers forming a discontinuous series, whilst we conceive all natural changes to be made up of gradual—*i.e.*, of imperceptibly small—changes, called by Newton fluxions. The discussion, therefore, of the continuous leads us ultimately to the question how our system of counting can be made useful for dealing with continuously variable quantities—the processes of nature. The conception of the infinite underlies not only the infinitesimal methods properly so called, but also all the methods of approximation by which—in the absence of rigorous methods—mathematical, notably astronomical, calculations are carried out.

12. Complex quantities.

13. The continuous.

14. The infinite.

Problems involving one or more of these conceptions presented themselves in large number to the analysts of the eighteenth century: there were notably two great doctrines in which they continually occur—the general solution of equations,[1] and the theory of

[1] As it may not be immediately evident how the ideas of continuity have to do with the general solution of equations, I refer to the first publication by Gauss, in 1799, containing a proof of the fundamental theorem of algebra, and its republication fifty years later (see Gauss,

infinite series. The solution of an equation being called finding its roots, it was for a long time assumed that every equation has as may roots as are indicated by its degree. A proof of this fundamental theorem of algebra was repeatedly attempted, but was only completed by Gauss in three remarkable memoirs, which prove to us how much importance he attached to rigorous proofs and to solid groundwork of science. The second great doctrine in which the conceptions of the continuous and the infinite presented themselves was the expansion of mathematical expressions into series. In arithmetic, decimal fractions[1] taken to any number of terms were quite familiar; the infinite series presented itself as a generalisation of this device. A very general formula

15. Doctrine of series. Gauss.

'Werke,' vol. iii. pp. 1 and 71). A very good summary of this proof is given by Hankel ('Complexe Zahlen-Systeme,' p. 87). A purely algebraical demonstration of the same theorem, not involving considerations of continuity and approximations, was also given by Gauss in the year 1816, and reproduced by others, including George Peacock, in his 'Report,' quoted above, p. 297. Hankel (*loc. cit.*, p. 97) shows to what extent Gauss's proof supplemented the similar proofs given by others before and after.

[1] Decimal fractions seem to have been introduced in the sixteenth century. Series of other numbers, formed not according to the decimal but to the dyadic, duodecimal, or other systems, were known to the ancients, and continued in use to the middle ages. The dyadic system was much favoured by Leibniz. It was also known that every rational fraction could be developed into a periodical decimal fraction. Prominent in the recommendation of the use of decimal fractions was the celebrated Simon Stevin, who, in a tract entitled 'La Disme' attached to his 'Arithmétique' (1590, translated into English, 1608), described the decimal system as "enseignant facilement expédier par nombres entiers sans rompus tous comptes se rencontrans aux affaires des hommes." Prof. Cantor ('Gesch. der Math.,' vol. ii. p. 616) says, "We know to-day that this prediction could really be ventured on—that indeed decimal fractions perform what Stevin promised." At the end of his tract he doubts the speedy adoption of this device, connecting with it the suggestion of the universal adoption of the decimal system. The best account of the gradual introduction of decimal fractions is still to be found in George Peacock's 'History of Arithmetic' ('Ency. Metrop.,' vol. i. p. 439, &c.)

of this kind was given by Brook Taylor, and somewhat modified by Maclaurin. It embraced all then known and many new series, and was employed without hesitation by Euler and other great analysts. In the beginning of the century, Poisson, Gauss, and Abel drew attention to the necessity of investigating systematically what is termed the convergency[1] of a series. As a specimen of this kind of research, Gauss published, in 1812, an investigation of a series of very great generality and importance.[2] We can say that through these two isolated memoirs of Gauss, the first of the three on equations, published in 1799, and the memoir on the series of 1812, a new and more rigorous treatment of the infinite and the continuous as mathematical conceptions was introduced into analysis, and that in both he showed the necessity of extending the system of numbering and measuring by the conception of the complex quantity. But it cannot be maintained that Gauss succeeded in impressing the new line of thought upon the science of

[1] A very good account of the gradual evolution of the idea of the convergency of a series will be found in Dr R. Reiff's 'Geschichte der unendlichen Reihen' (Tübingen, 1899, p. 118, &c.) Also in the preface to Joseph Bertrand's 'Traité de Calcul Différentiel' (Paris, 1864, p. xxix, &c.) According to the latter Leibniz seems to have been the first to demand definite rules for the convergency of Infinite Series, for he wrote to Hermann in 1705 as follows: "Je ne demande pas que l'on trouve la valeur d'une série quelconque sous forme finie; un tel problème surpasserait les forces des géomètres. Je voudrais seulement que l'on trouvât moyen de décider si la valeur exprimée par une série est possible, c'est-à-dire convergente, et cela sans connaître l'origine de la série. Il est nécessaire, pour qu'une série indéfinie représente une quantité finie, que l'on puisse démontrer sa convergence, et que l'on s'assure qu'en la prolongeant suffisamment l'erreur devient aussi petite que l'on veut." In spite of this, Leibniz, through his treatment of the series of Grandi, $1-1+1-1$, &c., the sum of which he declared to be $\frac{1}{2}$, seems to have exerted a baneful influence on his successors, including Euler (see Reiff, *loc. cit.*, pp. 118, 158).

[2] The memoir on the Hypergeometrical series.

mathematics in general. This was done about fifteen or twenty years after Gauss had begun to publish his isolated memoirs, in a comprehensive treatment of the subject by Cauchy, who, before 1820, delivered lectures on Analysis at the École Polytechnique and in other colleges, and commenced their publication in 1821. In this course of lectures the discussion of the notions of the infinite, of the continuous, of the convergence of series, and of the extension of our conception of quantity beyond the ordinary or real quantities of algebra, is put in the foreground, and the illicit habit of using the generalisations of algebra without defining the conditions of their validity severely criticised.[1] It is also evident, from the extensive notes which Cauchy added to the "cours" of 1821, that he felt the necessity of a revision of the fundamental notions of algebra. The publication of 1821 was followed by others on the Calculus, and it is through these treatises mainly that a new spirit was infused into general mathematical literature, first in

16. Cauchy's Analysis.

[1] The earliest labours of Cauchy were geometrical, and he evidently acquired through them an insight into the contrast between the rigour of the older geometrical and the looseness of the modern algebraical methods. In this regard he says: "J'ai cherché à leur donner toute la rigueur qu'on exige en géometrie, de manière à ne jamais recourir aux raisons tirées de la généralité de l'algèbre. Les raisons de cette espèce, quoique assez communément admises, surtout dans le passage des séries convergentes aux séries divergentes, et des quantités réelles aux expressions imaginaires ne peuvent être considérés, ce me semble, que comme des inductions propres à faire pressentir quelque fois la vérité, mais qui s'accordent peu avec l'exactitude si vantée des sciences mathématiques. On doit même observer qu'elles tendent à faire attribuer aux formules algébriques une étendue indéfinie, tandis que, dans la réalité, la plupart de ces formules subsistent uniquement sous certaines conditions, et pour certaines valeurs des quantités qu'elles renferment. En déterminant ces conditions et ces valeurs, et en fixant d'une manière précise le sens des notations dont je me sers, je fais disparaître toute incertitude" ('Cours d'Analyse,' 1821, Introd., p. ii).

France, somewhat later also in England and Germany. In the latter country, the highly original writings of Abel, and the independent labours of Jacobi, opened out an entirely new branch of higher mathematics, beginning with the discovery of the property of double periodicity of certain functions.[1] This extensive and fruitful province of analysis for a time retarded the revision and extension of the groundwork of mathematical reasoning which Cauchy had begun, and upon which Gauss evidently desired to make the extension of higher mathematics proceed.[2]

[1] Before the discovery of the functions with a double period, functions with one period were known: the circular and exponential functions—the former possessing a real, the latter an imaginary, period. The elliptic functions turned out to "share simultaneously the properties of the circular functions and exponential functions, and whilst the former were periodical only for real, the latter only for imaginary, values of the argument, the elliptic functions possessed both kinds of periodicity." This great step became clear when it occurred to Abel and Jacobi independently to form functions by inversion of Legendre's elliptic integral of the first kind. The two fundamental principles involved in this new departure were thus the process of inversion and the use of the imaginary, as a necessary complement to the real, scale of numbers. The share which belongs independently to Abel and Jacobi has been clearly determined since the publication of the correspondence of Jacobi with Legendre during the years 1827-32 (reprinted in Jacobi's 'Gesammelte Werke,' ed. Borchardt, vol. i., Berlin, 1881), and of the complete documents referring to Abel, which are now accessible in the memorial volume published in 1902. A very lucid account is contained in a pamphlet by Prof. Königsberger, entitled 'Zur Geschichte der Theorie der Elliptischen Transcendenten in den Jahren 1826-29' (Leipzig, 1879).

[2] Of the four great mathematicians who for sixty years did the principal work in connection with elliptic functions — viz., Legendre (1752-1833), Gauss (1777-1855), Abel (1802-29), and Jacobi (1804-51), each occupied an independent position with regard to the subject, —suggested originally by Euler, and important for the practical applications which it promised. Legendre during forty years, from 1786 onward, worked almost alone: he brought the theory of elliptic integrals, which had occurred originally in connection with the computation of an arc of the ellipse, into a system, and to a point beyond which the then existing methods seemed to promise no further advance. This advance was, however, secured by the labours of Jacobi through the introduction of the novel principles referred to in the last note. Two years before Jacobi's publication commenced, Abel had already approached the subject from an entirely different and much more general point of view. "Abel," as Monsieur L. Sylow says ('Mémorial des études d'Abel,' p. 14), "était avant tout algébriste. Il a dit lui-même que la théorie des équations était son sujet favori, ce qui d'ailleurs apparaît clairement dans ses œuvres. Dans ses travaux sur les fonctions elliptiques, le traitement des diverses équations algébriques dont cette théorie abonde est mis fortement en évidence, et dans le premier de ces travaux, la résolution de ces équations est même indiquée comme étant le sujet principal. Qui plus est, la théorie des équations était entre se mains l'instrument le plus éfficace. Ce fut ainsi sans aucun doute la résolution de l'équation de division des fonctions elliptiques qui tout d'abord le conduisit à la théorie de la transformation. Elle joue encore un rôle capitale dans sa démonstration du théorème dit théorème d'Abel, et dans les recherches générales sur les intégrales des différentielles algébriques qui se trouvent dans son dernier mémoire le 'Précis d'une Théorie des fonctions elliptiques.'" But whilst Abel certainly took a much more general view than either Legendre or Jacobi, both of whom came to a kind of deadlock on the roads they had chosen (Jacobi, when he attempted to extend the theory of the periodicity of functions), it is now quite clear that Gauss viewed the whole subject almost thirty years before Abel and Jacobi entered the field from a still more general point of view. Already, in 1798, when he was only twenty-one, he must have recognised the necessity of enlarging and defining the fundamental conceptions of algebra and of functionality or mathematical dependence; and it is very likely that the magnitude of the undertaking, for which his astronomical labours left him no time, debarred him from publishing the important results which he had already attained, and which covered to a great extent the field cultivated in the meantime by Abel and Jacobi, leaving only the celebrated theorem of the former (referring to the algebraical comparison of the higher non-algebraical functions) and the discovery of a new function on the part of Jacobi (his Theta function) as the two great additions which we owe to them in this line of research (see Königsberger, *loc. cit.*, p. 104). In this recognition of the fundamental change which mathematical science demanded, and its bearing upon these special problems here referred to, Gauss must have for a long time stood alone; for his great rival Cauchy, to whom we are mainly indebted for taking the first steps in this direction, did not for many years apply his fundamental and novel ideas to the theory of elliptic functions, which up to the year 1844, when Hermite entered the field, were almost exclusively cultivated by German and Scandinavian writers (see R. L. Ellis, "Report on the recent Progress of Analysis," Brit. Assoc., 1846; reprinted in 'Mathematical and other Writings,' p. 311). Nor could it otherwise be explained how Cauchy could keep the manuscript of Abel's great memoir without ever occupying himself with it, and thus delay its publication for fifteen years after it had been presented to the Academy. (See the above-mentioned correspondence between Legendre and Jacobi, 1829; also Sylow, p. 31.)

That such a revision had become necessary was seen, 17. Revision of fundamentals.
slowly if in many quarters, but it did not become generally recognised till late in the century, when thinkers of

the highest rank, who for some time had lived apart in the secluded regions of sublime analysis, descended again into the region of elementary science, both pure and applied, where they speedily remodelled the entire mode of teaching. England possessed very early a writer of great eminence who represented this tendency, and whose merits were only partially recognised in his day—Augustus de Morgan.

18. Extension of conception of number.

It will now be necessary to explain more definitely what is meant by the extension of our conception of number and quantity through the introduction of complex numbers or complex quantities. This extension first forced itself on analysts in the theory of equations, then in the algebraical treatment of trigonometrical quantities — *i.e.*, in the measurement of angles, or, as it is now called, of direction in geometry. The first extension of the conception of number lay in the introduction of negative numbers. These admitted of comparatively easy representation arithmetically by counting backward as well as forward from a given datum; practically in the conception of negative possessions, such as debts, geometrically by the two opposite directions of any line in space. In algebra, where the simple operations on quantities are usually preserved in the result and not lost in the simple numerical value of the result as in arithmetic, compound quantities were looked upon as generated by the processes of addition, resulting in the binomial (of which the polynomial was an easy extension), and further by the multiplication with each other of different binomials or polynomials, through which process expressions of higher order or

degree were arrived at. The forward or direct process was easy enough, though even here assumptions or arbitrary rules were included which escaped notice for a long time; but the real labour of the analysts only began with the inverse problem—viz., given any compound quantity, similar in structure to those directly produced by multiplication of binomials, to find the factors or binomials out of which it can be compounded. Now it was found that as in the arithmetical process of division, the invention of fractional quantities; as in that of extraction of roots, the irrational quantities had to be introduced: so in the analysis of compound algebraical expressions into binomial factors, a new quantity or algebraical conception presented itself. It was easily seen that this analysis could be carried out in every case only by the introduction of a new unit, algebraically expressed by the square root of the negative unity. There was no difficulty in algebraically indicating the new quantity as we indicate fractions and irrational quantities; the difficulty lay in its interpretation as a number. Since the time of Descartes geometrical representations of algebraical formulæ had become the custom, and it was therefore natural when once the new, or so-called imaginary, unit was formally admitted, that a geometrical meaning should be attached to it.

19. The geometrical and the logical problems.

Out of the scattered beginnings of these researches two definite problems gradually crystallised: the one, a purely formal or mechanical one — viz., the geometrical representation of the extended conception of quantity, of the complex quantity; the other, a logical

or philosophical one—viz., the clearer definition of the assumptions or principles which underlie arithmetical and algebraical reasoning. And if algebraical, then also geometrical reasoning. Both problems seem to have presented themselves to the youthful mind of Gauss, as is evident from his correspondence with Bessel [1] and Schumacher, and from his direct influence on Bolyai,[2] Möbius, and Von Staudt, perhaps also indirectly on Lobatchevsky.[3] It does not, however, appear as if he

[1] See especially the letters of Gauss to Bessel, dated November and December 1811 and May 1812 ('Briefwechsel,' Leipzig, 1880, p. 151 *sqq.*)

[2] Bolyai, the elder (1775-1856), was a student friend of Gauss in the years 1797 to 1799, and kept up a correspondence with him during half a century. This correspondence has now been published by F. Schmidt and P. Stäckel, Leipzig, 1899, with a supplement containing some information about this extraordinary man. His son, Johann Bolyai (1802-60), is the author of the celebrated "Appendix, scientiam spatii absolute veram exhibens," which was attached to his father's 'Tentamen, juventutem . . . in elementa matheseos puræ . . . introducendi,' 1832. The tract seems to have been written in 1823. A translation, with introduction, has been published by Dr G. Bruce Halsted ('Neomonic Series,' vol. iii. 4th ed., Austin, Texas, 1896). When the elder Bolyai sent to Gauss in the year 1831 to 1832 a copy of his son's tract and of his own work on Geometry, Gauss expressed great surprise at the contents of the former. (See his letter of March 6, 1832.) His remarks that the younger Bolyai had anticipated some of his own ideas on the subject, remind one of a similar remark which he made, May 30, 1828, to Schumacher with reference to Abel's "Memoir on Elliptic Functions" in vol. ii. of Crelle's 'Journal' (see Gauss, 'Werke,' vol. iii. p. 495). In both cases he felt himself relieved from the necessity of publishing his own results, though, so far as those referring to the foundations of geometry are concerned, it does not appear that his ideas had arrived at that state of maturity which the publication of his posthumous papers has proved to have been attained in his treatment of the higher functions. Indeed little or nothing of prime importance has been found among his papers referring to the principles of geometry; and he stated to Bolyai that though he had intended to commit his views to paper, so that they should not be lost, he had not intended to publish anything during his lifetime.

[3] It is doubtful whether Gauss's speculations had any influence on the younger Bolyai's theory, and still more so as regards Lobatchevsky, whose first tract appeared in the 'Kazan Messenger,' 1829 to 1830, but dates back probably to 1826. Inasmuch, however, as the younger Bolyai must have become acquainted through his father with the speculations of the youthful Gauss, and as Lobatchevsky was a pupil of another student friend of Gauss in the person of Prof. Bartels, it is not unlikely that the interest which these thinkers took in the subject can be originally traced to the same source. (See Dr Halsted's address on Lobatchevsky, 'Neomonic Series,' vol. i., 1894.) A complete bibliography of the earlier papers, referring to the so-called "non-Euclidean" literature down to 1878, is given by Dr Halsted in the first two vols. of the 'American Journal of Mathematics': the most recent publications are those of the Hon. B. A. W. Russell in his work, 'The Foundations of Geometry' (1897) and his excellent article on "Non-Euclidean Geometry" in the 28th vol. of the 'Ency. Brit.' See also Klein's lithographed lectures on 'Nicht-Euklidische Geometrie,' Göttingen, 1893.

had arrived at any finality in his speculations, and, beyond occasional hints which have only subsequently become intelligible, the love of finish exhibited in all his published writings prevented him from giving to the world the suggestive ideas which evidently formed the groundwork of his mathematical labours. There is no doubt that—like Goethe in a very different sphere—Gauss anticipated individually the developments in the sphere of mathematical thought down to the end of the century. The interpretation of the complex quantity had been given by Wessel, Buée, and Argand [1] in the early years of the century; but it remained unnoticed till it received the sanction of Gauss in a celebrated memoir referring to the theory of numbers, and until in

[1] The first somewhat exhaustive historical statement as to the geometrical representation of the complex or imaginary quantity was given by Hankel in the above-mentioned work (see above, note, p. 645), p. 82. He there says, after discussing the claims of others,—notably of Gauss,—that Argand in his 'Essai' of the year 1806 (re-edited by Hoüel, 1874) "had so fully treated of the whole theory that later nothing essentially new was added, and that, except a publication of still earlier date were found, Argand must be considered the true founder of the representation of complex quantities in the plane." Such an earlier publication has indeed been met with in a tract by Caspar Wessel, which was presented to the Danish Academy in 1797, and published in 1799. Having been overlooked, like Argand's 'Essai,' it has now been republished at Copenhagen, 1897, with the title 'Essai sur la représentation de la direction' (see 'Encyk. Math. Wissenschaften,' vol. i. p. 155).

this country the labours of De Morgan and of Sir William Rowan Hamilton gave the matter a further and very important extension.[1] It was also in this country that the second problem, the critical examination of the principles which underlie the process of legitimate generalisation of algebra, received distinct attention. To George Peacock, and to the school of algebraists which followed him, is due the merit of having brought out clearly the three fundamental laws of symbolical reasoning now generally admitted in text-books on the subject—the associative, distributive, and commutative principles. That these principles were to a great extent conventional, or empirically adopted from ordinary arithmetic, and in consequence not necessarily indispensable for a consistent system of symbolical reasoning, has been generally admitted ever since Sir William Rowan Hamilton, after ten years of labour, succeeded in establishing a new calculus—the method of quaternions, in which the commutative principle of multiplication is dropped. This

20. Quaternions.

[1] Far more important than the suggestions or artifices mentioned in the foregoing note, and which since the time of Argand and Gauss have been variously modified, is the conception that our common numbers do not form a complete system without the addition of the imaginary unit, but that with the introduction of a second unit "numbers form a universe complete in itself, such that, starting in it, we are never led out of it. There may very well be, and perhaps are, numbers in a more general sense of the term; but in order to have to do with such numbers (if any) we must start with them" (Cayley in art. "Equation," 'Ency. Brit.'; 'Coll. Works,' vol. xi. p. 503). There seems little doubt that this conception was first clearly established in the mind of Gauss, and that none of the contemporary writers can be shown to have had a similarly clear insight. Since this has become generally recognised—and we owe this recognition probably to the independent labours of Grassmann and Riemann—the discussion of the whole subject has been raised to a much higher level, as may be seen by comparing the Report of Peacock, quoted above, with the discussion of Hankel (*loc. cit.*), and still more with the exhaustive article by Prof. E. Study in vol. i., 'Encyk. Math. Wiss.,' pp. 147-184.

calculus was shown to be of special use in expressing the relations of spherical trigonometry. Two terms expressing definite notions special to geometry, by which science has been enriched and practical application greatly simplified, are an outcome of this line of research. These are the terms "vector," to express the notion of directed magnitude—*i.e.*, of direction and magnitude combined as distinguished from magnitude and position alone; and the notion of an "operator" which changes direction and magnitude as an ordinary multiplier changes magnitude only.[1] It was shown by Argand and others that the

[1] These two notions, which have their origin in the writings of Hamilton on the one side and the Calculus of Operations on the other, belong to this country and to a period during which mathematical researches were carried on in a fragmentary manner, and much out of contact with the contemporary mathematics of the Continent. Both the Calculus of Quaternions of Hamilton and the Calculus of Operations were looked upon for a long time as curiosities (as was also the Barycentric Calculus of Möbius in Germany). Gradually, however, the valuable ideas which were contained in them became recognised as much from the practical as from the theoretical point of view. In the former interest the application of Vector Analysis or the Algebra of Directed Quantities received a great impetus when the need was felt of having an algebra of "physical quantities." This found expression in the writings of Clerk-Maxwell. (See his 'Treatise on Electricity and Magnetism,' vol. i. p. 8, 2nd ed., as also his paper on "The Mathematical Classification of Physical Quantities," 1871. 'Coll. Papers,' vol. ii. p. 257.) In the practical application of electrical theories these notions have since become indispensable, and the subject has received increasing attention, notably in America, which holds a foremost place in the development of electrical science and its application. Mathematicians of the first order, such as J. Willard Gibbs, have published text-books on the subject, whilst other electricians of eminence, such as Mr Oliver Heaviside, have elaborated special forms of the Directional Calculus to serve their purposes. In Dynamics the Dublin School, represented after the death of Hamilton by Sir Robert S. Ball (in his 'Theory of Screws,' 1876), has had an important influence in the introduction of novel and more appropriate methods which have gradually permeated the general treatment of the subject. Whilst there is no doubt that for a long time the Calculus of Quaternions was the only methodical elaboration of these novel and useful ideas, it was overlooked that simultaneously and quite independently H. Grassmann of Stettin (see above, vol. i. p. 243) had worked out a much more comprehensive and fundamental calculus, of which the method of quaternions and all the different forms of Vector Analysis can be

arithmetic based upon two units instead of one—*i.e.*, the arithmetic of couples or complex quantities—could be completely and consistently represented by choosing as axes whereon the separate units were counted, the two perpendicular axes of Cartesian geometry. An attempt to extend this geometrical representation into space led Hamilton to the invention of his method, Gauss having very early satisfied himself that within the limits of ordinary algebra no further extension was necessary or possible.

The examination into fundamental principles was not limited in the mind of Gauss to those of algebra: he early applied himself likewise to those of geometry and of dynamics. The great French mathematicians, such
21. Foundations of geometry. as Legendre and Lagrange, were also occupied with such speculations. They have been carried on all through the century, but have only towards the end of the period been brought into connection and shown to be of importance for the general progress of mathematics. The secluded, and for a long time unappreciated, labours of isolated but highly original thinkers have accordingly

considered as merely special instances. This has now been abundantly proved through the writings of mathematicians in all countries, among whom I will only mention Hankel and Dr V. Schlegel in Germany, Clifford, Prof. Henrici, and latterly Mr Whitehead in England, Prof. Peano in Italy, and M. Burali Forti in France. See on the whole subject, on the fate of Grassmann and of his great work, V. Schlegel, 'Die Grassmann'sche Ausdehnungslehre,' Leipzig, 1896; also, by the same author, a short biography of Grassmann (Leipzig, Brockhaus, 1878). A complete edition of Grassmann's works is being published by Teubner. Those who are interested in seeing how the notions underlying the directional calculus are gradually becoming clarified, and the terminology and notation settled, may read with profit the controversy carried on in the pages of 'Nature,' vols. xlvii. and xlviii., between Prof. Macfarlane, Willard Gibbs, Mr O. Heaviside, Mr A. M'Aulay, and Dr Knott; also Dr Larmor's review of Hayward's 'Algebra of Coplanar Vectors' (vol. xlvii. p. 266), and Sir R. S. Ball's reference to the 'Ausdehnungslehre' of Grassmann (vol. xlviii. p. 391, 1893).

received tardy recognition. Such speculations can be carried on either as fascinating exercises of mere ingenuity, or for practical purposes to improve the refined instruments of mathematical calculation, or in the philosophical interest of arriving at the fundamental processes of human thought and intuition.[1] Many persons think that only the second of these three in-

[1] Already Euler had remarked on the different interests that prompted mathematical research. Referring to the writings of Count Fagnano, he says in the introduction to the first of his memoirs on Elliptic Integrals (1761, quoted by Brill & Nöther in 'Bericht der Deutschen Mathematiker-Vereinigung,' vol. iii. p. 206): "If one looks at mathematical speculations from the point of view of utility, they can be divided into two classes: first, those which are of advantage to ordinary life and other sciences, and the value of which is accordingly measured by the amount of that advantage. The other class comprises speculations which, without any direct advantage, are nevertheless valuable because they tend to enlarge the boundaries of analysis and to exercise the powers of the mind. Inasmuch as many researches which promise to be of great use have to be given up owing to the inadequacy of analysis, those speculations are of no little value which promise to extend the province of analysis. Such seems to be the nature of observations which are usually made or found *a posteriori*, but which have little or no chance of being discovered *a priori*. Having once been established as correct, methods more easily present themselves which lead up to them, and there is no doubt that through the search for such methods the domain of analysis may be considerably extended." The school of mathematicians headed by Abel and Jacobi pursued mathematics from purely scientific interest, and was criticised on this ground by eminent contemporary mathematicians in France: see a letter of Jacobi to Legendre, dated July 2, 1830, in which he refers to a Report of Poisson on his great work, but adds: "M. Poisson n'aurait pas dû reproduire dans son rapport une phrase peu adroite de feu M. Fourier où ce dernier nous fait des reproches, à Abel et à moi, de ne pas nous être occupés de préférence du mouvement de la chaleur. Il est vrai que M. Fourier avait l'opinion que le but principal des mathématiques était l'utilité publique et l'explication des phénomènes naturels; mais un philosophe comme lui aurait dû savoir que le but unique de la science, c'est l'honneur de l'esprit humain et que sous ce titre, une question de nombres vaut autant qu'une question du système du monde." In the sequel he adds: "Je crois entrevoir que toutes ces transcendantes" (*i.e.*, the elliptic and Abelian functions) "jouissent des propriétés admirables et inattendues auxquelles on peut être conduit par le théoréme d'Abel. . . . J'ai réfléchi aussi de temps en temps sur une méthode nouvelle de traiter les perturbations célestes, méthode dans laquelle doivent entrer les théories nouvelles des fonctions elliptiques."

ducements is likely to prove fruitful for the progress of science; they look upon the first as an amusing pastime, and upon the third as empty and not devoid of danger. In recognition of the partial correctness of this view, I will follow up the practical stimulus in its fruitful influence upon the development of the lines of mathematical research.

This stimulus came in the closing years of the preceding century through the lectures of Gaspard Monge at the École Normale, and has become popularly known
22. Descriptive Geometry. through his invention of Descriptive Geometry, the first modern systematic application of purely graphical methods in the solution of mathematical problems. As Cauchy was the founder of the modern school of analysts, so Monge, together with Carnot, founded the modern school of geometricians; Dupin, Poncelet, and Chasles being among his most illustrious pupils. The aim of this school was to give to geometrical methods, such as had been practised by the ancients,[1] the same generality and systematic unity which characterised the analytical methods introduced by Descartes.

Not long after the introduction of the latter, Leibniz

[1] These methods had been largely used in this country by Newton, Robert Simson, and Stewart. They were systematised by L. N. M. Carnot. Chasles ("Discours d'inauguration, &c.," 1846, 'Géométrie Supérieure,' p. lxxvii) says: "Dans le siècle dernier, R. Simson et Stewart donnaient, à l'instar des Anciens, autant de démonstrations d'une proposition, que la figure à laquelle elle se rapportait présentait de formes différentes, à raison des positions relatives de ses diverses parties. Carnot s'attacha à prouver qu'une seule démonstration appliquée à un état assez général de la figure devait suffire pour tous les autres cas; et il montre comment, par des changements de signes de termes, dans les formules démontrées par une figure, ces formules s'appliquaient à une autre figure ne différant de la première, commes nous l'avons dit, que par les positions relatives de certaines parties. C'est ce qu'il appela le 'Principe de corrélation des figures.'"

had foretold[1] the possibility and necessity of such an independent development of pure geometry, in which the relations of position in space, as opposed to those of measure, magnitude, or quantity, would be placed in the foreground. Projection, as practised in the drawing of maps, and perspective, as practised in the fine and descriptive arts, had already revealed a number of remarkable properties of figures in the plane and in space. By continuous motion of points or lines, by artifices like throwing of shadows, by sections of solids with lines and surfaces, a vast number of problems had been solved and isolated theorems established. The method here practised was that of construction, as in analysis the method was that of calculation with subsequent interpretation. All this purely constructive work was to be brought together and systematically combined in a whole. It was evidently a distinct line of research, based upon intellectual processes other than the purely analytical method — a line which, as it seemed to its followers, had been unduly neglected and pushed into the background. Although Monge became the founder of this purely descriptive or constructive branch of geometry, he was himself equally great as an analyst; in fact, the fusion in his mind of the two methods was the origin of much of his greatest work. In attempting to carry out more thoroughly the separation or independent development of the constructive or descriptive method, his great pupil,
J. V. Poncelet—whilst deprived of all literary resources 23. Poncelet.

[1] See the quotations from his letters to Huygens and others given above, vol. i. p. 103 note.

in the prisons of Russia—meditated on the real cause of the power which algebraical analysis possessed, on the reason why geometry proper was deprived of it, and what might be done to give it a similar generality. In pursuing this line of thought he was led to discover the cause of the existing limitation of purely geometrical reasoning in its rigidity, inasmuch as it was arrested as soon as its objects ceased to have a positive or absolute, that is a physical, existence.[1] Opposed to this limitation was the freedom of the analytical method, which, operating with indeterminate symbols, could, by letting them change gradually, include not only what was explicitly given, but also that which was merely implied; not only the finite, but likewise, the infinite; not only the real, but likewise the fictitious or imaginary. In order to gain a similar generality in purely geometrical or descriptive science, a similar flexibility would have to be introduced. Poncelet was thus led to the enunciation of his celebrated and much-criticised "principle or law of continuity."[2]

[1] See the "Introduction" to the 1st volume of the 'Traité des Propriétés projectives des figures,' pp. xi, xii. I quote from the 2nd edition of 1865. The 1st was published in 1822. The researches date from 1813, the year of Poncelet's imprisonment. See "Préface de la première édition."

[2] Ibid., Introduction, p. xiv. On the principle of continuity in geometry, see an article in vol. xxviii. "Ency. Brit.' by the Rev. Charles Taylor, and the references given therein; also Prof. E. Kötter's Report on the "Development of Synthetic Geometry" in vol. v. of the 'Jahresbericht der Deutschen Mathematiker Vereinigung,' p. 122, &c.: "Originally the expositions referring to the principle of continuity were intended to occupy much greater space. . . . In consequence of correspondence with Terquem, Servois, and Brianchon, Poncelet desisted from the publication of it. . . . However cautiously Poncelet advanced his principle"—in the 'Essai sur les propriétés projectives des sections coniques' (presented to the Academy in 1820)—"it nevertheless aroused the doubts of Cauchy, who in his report on Poncelet's paper warns against the too hasty application of the principle. Gergonne accompanied the reprint of this report with notes, in which he characterised the principle as a valuable instrument for the discovery of new truths, which nevertheless did not make stringent proofs superfluous." Cauchy's report seems to have aroused Poncelet's indignation. Hankel ('Elemente der Projectivischen Geometrie,' 1875, p. 9) says: "This principle, which was termed by Poncelet the 'Principle of Continuity,' inasmuch as it brings the various concrete cases into connection, could not be geometrically proved, because the imaginary could not be represented. It was rather a present which pure geometry received from analysis, where imaginary quantities behave in all calculations like real ones. Only the habit of considering real and imaginary quantities as equally legitimate led to that principle which, without analytical geometry, could never have been discovered. Thus pure geometry was compensated for the fact that analysis had for a long time absorbed the exclusive interest of mathematicians; indeed it was perhaps an advantage that geometry, for a time, had to lie fallow." Kötter continues: "Von Staudt was the first who succeeded in subjecting the imaginary elements to the fundamental theorem of projective geometry, thus returning to analytical geometry the present which, in the hands of geometricians, had led to the most beautiful results."

Analytical geometry, by substituting an algebraical expression for a geometrical figure—say a curve,—could apply to it all the artifices of abstract analysis. By varying the co-ordinates you can proceed along the whole extent of the curve and examine its behaviour as it vanishes into infinity, or discover its singular points at which there occurs a break of continuity: you can vary its constants or parameters, and gradually proceed from one curve to another belonging to the same family, as is done in grouping together all curves of the second order, or—as was done in the calculus of variation, invented by Euler and Lagrange—you can vary the form of the equation, proceeding from one class of curve to another. Now clearly all this operating on equations and symbolic expressions was originally abstracted from geometry, including the mechanical conception of motion; in particular the ideas which underlie the method of fluxions were suggested by the motion of a point in space. The conception of continuous motion in space—

of motion of points, lines, planes—corresponded accordingly to the notion of variability in analysis. The introduction of motion, gradual and continuous, would give to purely geometrical or descriptive reasoning the same flexibility which analysis had acquired in the calculus of fluxions and of variations. Figures would lose their rigidity and isolation and limited nature and become movable, related to each other, filling the whole of space instead of a restricted and confined area or region.

24. Character of modern geometry.

It is the peculiarity of the modern as opposed to the older geometry, never to let figures become motionless or rigid,[1] never to consider them in their isolation, but always in their mutual relations; never to have regard only to a finite portion of a line or surface, but to conceive of it in its infinite extension. By a reaction of analysis and geometry on each other, freedom and generality have been gradually acquired.

But this moving about of figures in space in order to learn their properties and mutual relations must be according to some method; otherwise it will not lead to scientific and exact knowledge. Poncelet, in considering how the two successful methods in geometry — the Cartesian and the Descriptive—had attained to their perfection, discovers a general principle which underlies their proceedings, and which is capable of great extension: this is the principle of projection.[2]

[1] See, *inter alia*, what Geiser says of Jacob Steiner's method in his pamphlet 'Zur Erinnerung an Jacob Steiner,' Schaffhausen, 1874, p. 27.

[2] 'Traité des Propriétés projectives,' vol. i. p. xviii: "En réfléchissant attentivement à ce qui fait le principal avantage de la Géométrie descriptive et de la méthode des coordonnées, à ce qui fait que ces branches des Mathématiques offrent le caractère d'une véritable doctrine, dont les principes, peu nombreux, sont liés et enchaînés d'une manière nécessaire et par une marche uniforme, on ne tarde pas à reconnaître que cela tient uniquement à l'usage qu'elles font de la projection."

25. Method of projection.

Of this principle of projection, which Poncelet at once introduces in the more general form as conical or central projection, two signal applications existed in the treatises on Conic Sections handed down from antiquity, and in the practical methods and Rules of Perspective invented by Lionardo da Vinci and further developed by various geometricians. The results, which lay scattered in many books and memoirs, Poncelet collected in a systematic form, bringing them, by the application of the law of continuity, under a few general and eminently useful points of view or principles. By the method of projection or perspective he "transformed figures which are very general into others which are particular, and *vice versâ*." He established the principle of "homology" in figures, and by showing how figures apparently very different could be described by the process of projection from the same original figure, he showed that there existed a peculiar relation among figures—viz., their "reciprocity."[1]

[1] The properties of figures, called by Poncelet "homology" and "reciprocity," refer to the correspondence of certain elements of one figure to those of another figure. In the case of "homology," we have to do with corresponding points or corresponding lines—*i.e.*, with the correspondence of the same elements. In the case of "reciprocity," we have to do with correspondence of points or lines in the one figure, with lines or points in the other—*i.e.*, with the correspondence of different elements. The idea of placing figures in an homologous relation was got by the device of making two planes, which contained figures in perspective, fall together into one plane; upon which the section of the two original planes became the "axis," and the eye-point the "centre" of homology—all situated in one and the same plane. Poncelet had already conceived of the possibility of reducing the two planes in Monge's 'Descriptive Geometry,' which represent the plan and elevation of a figure in one plane, on which the elevations were marked by what are now called "contour lines." The idea of the correspondence of figures by what is called "reciprocity" was sug-

26. Law of continuity. By the law of continuity he showed how in pure geometry it became necessary to introduce the consideration of points and lines which vanish into infinity or which become imaginary, establishing by their invisible elements the continuous transition from one geometric form to another; just as in algebra these conceptions had forced themselves on the attention of analysts. Ideal elements were thus made use of to lead to the discovery of real properties.

The consideration of lines and points which vanish or lie at infinity was familiar to students of perspective from the conception of the "vanishing line"; but the inclusion of ideal points and lines was, as Hankel says, a gift which pure geometry received from analysis, where imaginary (*i.e.*, ideal or complex) quantities behave
27. Ideal elements. in the same way as real ones. Without the inclusion of these ideal or invisible elements the generality or continuity of purely geometrical reasoning was impossible.

The geometrical reasoning of Monge, Carnot, and Poncelet was thus largely admixed with algebraical or analytic elements. It is true that Monge's descriptive geometry was a purely graphical method, and that

gested to Poncelet by the property, known already to De la Hire ("Sectiones Conicæ," 1685), that in the plane of a conic section every point corresponds to a straight line called its "polar," that to every straight line corresponds a point called its "pole," that the "polars" corresponding to all the points of a straight line meet in one and the same point, and *vice versa* that the "poles" corresponding to all lines going through one and the same point lie on a straight line; the line and point in question standing in both cases in the relation of pole and polar to each other. Poncelet uses "this transformation of one figure into its reciprocal polar systematically as a method for finding new theorems: to every theorem of geometry there corresponds in this way another one which is its 'polar,' and the whole of geometry was thus split up into a series of truths which run parallel and frequently overlap each other" (Hankel, *loc. cit.*, p. 20).

Poncelet's method of central projection attacked geometrical problems from a purely constructive point of view. Nevertheless the frequently expressed object of the later writings of Monge, as well as those of Carnot and Poncelet, was to introduce into geometrical reasoning the generality and continuity which analysis possessed, and this was largely attained by the interpretation of nations taken over from analysis. Their endeavours were, however, in the sequel crowned by the discovery of a purely geometrical property, the understanding of which has ever since formed the basis of what may be termed modern geometry.

This remarkable property, which may be regarded as revealing the very essence of extension in space or of the "space-manifold,"—inasmuch as it brings the different elements of space into mutual relation,—is the so-called principle of "duality" or of "reciprocity." The 28. Principle of duality.
principle of duality is now usually defined to mean that in geometry on the plane or in space, "figures coexist in pairs, two such coexisting figures having the same genesis and only differing from one another in the nature of the generating element."[1] The elements of plane geometry are the point and the line; the elements of solid geometry are the point and the plane. By interchanging these correlative terms, correlative propositions may be written down referring to plane and to space geometry. In projective geometry there are two processes which are correlative or complementary to each other—the process of projection and the process of section. We can project

[1] Cremona, 'Elements of Projective Geometry,' transl. by Leudesdorf. Oxford, 1885, p. 26.

from a point drawing lines or rays on the plane and in space, and we can cut these by lines in a plane or by planes in space. And it can be shown that "if one geometric form has been derived from another by means of one of these operations, we can conversely, by means of the complementary operation, derive the second from the first."[1]

The projective geometry of Poncelet contains the twofold origin of the principle of duality in his method of projection and section, and in his theory of the reciprocity of certain points and lines in the doctrine of conic sections,
29. Reciprocity. called the theory of reciprocal polars. But the mathematician who first expressed the principle of duality in a general—though not in the most general—form was Gergonne, who also recognised that it was not a mere geometrical device but a general philosophical principle, destined to impart to geometrical reasoning a great simplification. He sees in its enunication the dawn of a new era in geometry.[2]

[1] Cremona, *loc. cit.*, p. 33.

[2] The principle of Duality seems to have been first put forward in its full generality by Gergonne, inspired probably by the theory of Reciprocal Polars (see note, p. 663) enunciated by Poncelet, who many years afterwards carried on a voluminous polemic as to the priority of the discovery. "Gergonne saw that the parallelism (referred to above) is not an accidental consequence of the property of conic sections, but that it constitutes a fundamental principle which he termed the 'principle of duality.' The geometry which is usually taught, and in which a line is considered to be generated by the motion of a point, is opposed by another geometry equally legitimate in which a point is generated by the rotation of a line. Whereas in the first case the line is the locus of the moving point, in the latter case the point is the geometrical intersection of the rotating line. In this generality the principle of duality has been incorporated into modern geometry" (Hankel, *loc. cit.*, p. 21). Gergonne says of the new principle (1827, see Supplement to vol. ii. 2nd ed. of Poncelet's 'Traité,' p. 390): "Il ne s'agit pas moins que de commencer pour la géométrie, mal connue depuis près de deux mille ans qu'on s'en occupe, une ère tout-à-fait nouvelle ; il s'agit d'en mettre tous les anciens traités à peu près au rebut, de leur substituer des traités d'une forme tout-à-fait différente, des traités vraiment philosophiques qui nous montrent enfin cette étendue, réceptacle universel de tout ce qui existe, sous sa véritable physionomie, que la mauvaise méthode d'enseignement adoptée jusqu'à ce jour ne nous avait pas permis de remarquer ; il s'agit, en un mot, d'opérer dans la science une révolution aussi impérieusement nécessaire qu'elle a été jusqu'ici peu prévue."

It must, however, in all fairness be stated that about the period from 1822 to 1830 this great simplification and unification of geometric science was as it were in the air—that it had presented itself to various great thinkers independently, being suggested from different points of view. The beginnings can no doubt be traced in the beautiful theorems of older French mathematicians, such as Pascal and De la Hire, and more generally in the suggestive methods of Monge and Poncelet; its first formal enunciation is in the memoirs of Gergonne: but the comprehensive use of it—the rewriting of geometry
from this point of view—was the idea of Jacob Steiner, 30. Steiner.
who, in his great but unfinished work on the "Systematic Development of the Dependence of Geometric Forms" (1830), set himself the great task "of uncovering the organism by which the most different forms in the world of space are connected with each other." "There are," he says, "a small number of very simple fundamental relations in which the scheme reveals itself, by which the whole body of theorems can be logically and easily developed." "Through it we come, as it were, into possession of the elements which Nature employs with the greatest economy and in the simplest manner in order to invest figures with an infinite array of properties."[1]

[1] See the Preface to the 'Systematische Entwickelung, &c.,' in Jacob Steiner's 'Gesammelte Werke' (ed. Weierstrass), vol. i. p. 229. "In the beautiful theorem that a conic section can be generated by the intersection of two projective pencils (and the dually

The labours of Poncelet and Steiner introduced into geometry a twofold aspect, and accordingly, about the middle of the century, we read a good deal of the two kinds of geometry which for some time seemed to develop independently of each other. The difference has been defined by the terms "analytic or synthetic," "calculative or constructive," "metrical or projective." The one operated with formulæ, the other with figures; the one studied the properties of quantity (size, magnitude), distances, and angles, the other those of position.

The projective method seemed to alter the magnitude of lines and angles and retain only some of those of position and mutual relation, such as contact and intersection. The calculating or algebraical method seemed to isolate figures and hide their properties of mutual interdependence and relation.

31. Mutual influence of metrical and projective geometry.

These apparent defects stimulated the representatives of the two methods to investigate more minutely their hidden causes and to perfect both. The algebraical formula had to be made more pliable, to express more naturally and easily geometrical relations; the geometrical method had to show itself capable of dealing with quantitative problems and of interpreting geometrically those modern notions of the infinite and the complex which the analytic aspect had put promi-

correlated theorem referring to projected ranges), Steiner recognised the fundamental principle out of which the innumerable properties of these remarkable curves follow, as it were, automatically with playful ease. Nothing is wanted but the combination of the simplest theorems and a vivid geometrical imagination capable of looking at the same figure from the most different sides in order to multiply the number of properties of these curves indefinitely" (Hankel, *loc. cit.*, p. 26; see also Cremona, 'Projective Geometry,' p. 119).

nently into the foreground. The latter was done by the geometric genius of Von Staudt, who succeeded in giving a purely geometrical interpretation of the imaginary or invisible elements[1] which algebra had introduced, whilst Steiner astonished the mathematical world by the fertility of the methods by which he solved the so-called isoperimetrical problems—*i.e.*, problems referring to largest or smallest contents contained in a given perimeter or *vice versâ*, problems for which Euler and Lagrange had invented a special calculus.[2] In spite of

[1] The geometrical interpretation of the imaginary elements is given by Von Staudt in a sequel to his 'Geometrie der Lage' (1847), entitled 'Beiträge zur Geometrie der Lage' (1856-60); and after having been looked upon for a long time as a curiosity or a "hair-splitting abstraction," it has latterly, through the labours of Prof. Reye ('Geometrie der Lage,' 1866-68) and Prof. Lüroth ('Math. Annalen,' vol. xiii. p. 145), become more accessible, and is systematically introduced into many excellent text-books published abroad. The simplest exposition I am acquainted with is to be found in the later editions of Dr Fiedler's German edition of Salmon's 'Conic Sections' (6th Aufl., vol. i. p. 23, &c., and p. 176, &c.) In 1875, before the great change which has brought unity and connection into many isolated and fragmentary contributions had been recognised, Hankel wrote with regard to Von Staudt's work, and in comparison with that of Chasles, as follows: "The work of Von Staudt, classical in its originality, is one of those attempts to force the manifoldness of nature with its thousand threads running hither and thither into an abstract scheme and an artificial system: an attempt such as is only possible in our Fatherland, a country of strict scholastic method, and, we may add, of scientific pedantry. The French certainly do as much in the exact sciences as the Germans, but they take the instruments wherever they find them, do not sacrifice intuitive evidence to a love of system nor the facility of method to its purity. In the quiet town of Erlangen, Von Staudt might well develop for himself in seclusion his scientific system, which he would only now and then explain at his desk to one or two pupils. In Paris, in vivid intercourse with colleagues and numerous pupils, the elaboration of the system would have been impossible" (*loc. cit.*, p. 30).

[2] See the lecture delivered by Steiner in the Berlin Academy, December 1, 1836, and the two memoirs on 'Maximum and Minimum' (1841), reprinted in 'Gesammelte Werke,' vol. ii. p. 75 *sqq.*, and 177 *sqq.*, especially the interesting Introductions to both, in which he refers to his forerunner Lhuilier (1782), deploring that others had needlessly forsaken the simple synthetical methods adopted by him. Some of Steiner's expositions in these matters were apparently so easy that non-mathematical listeners

these marvellous works of genius, science is probably indebted for its greatest advances to those mathematicians who, like Plücker in Germany, Chasles in France, and Cayley in England, employed the analytic and constructive methods alternately and with equal mastery.

It is impossible—and it is not my object—to allot to each of these original thinkers the special ideas introduced by him into modern science; but for the purpose

like Johannes Müller could not understand how such simple things could be brought before the Academy of Sciences, whereas the great mathematician Dirichlet was full of praise of the ingenuity of the method by which problems were solved which the Calculus of Variations attacked long after Steiner, and then only in ways which the synthetical method had indicated (see Geiser, 'Zur Erinnerung an Jacob Steiner,' p. 28). It must not be supposed, however, that Steiner was an extreme purist so far as geometrical methods were concerned, for he says himself "that of the two methods neither is entitled to exclude the other; rather both of them will, for a long time, have plenty to do in order to master the subject to some extent, and then only can an opinion as to their respective merits be formed" ('Ges. Werke,' vol. ii. p. 180). An instance of a celebrated problem being treated alternately by synthetic and analytic methods is that of the Attraction of Ellipsoids, in which the Theorem of Maclaurin had created quite a sensation. In spite of the admiration which it evoked, both Legendre and Poisson expressed the opinion that the resources of the synthetic method are easily exhausted. The latter, whilst admitting "que la synthèse ait d'abord devancé l'analyse," nevertheless concludes that "la question n'a été enfin résolue complètement que par des transformations analytiques. . . auxquelles la synthèse n'aurait pu suppléer." This expression of opinion was falsified when Chasles presented to the Academy, in the year 1837, a memoir in which, through the study of confocal surfaces, the Theory of Maclaurin was synthetically proved in its full generality. Poinsot, who reported on this memoir, attached the following remarks: "Ce mémoire remarquable nous offre un nouvel exemple de l'élégance et de la clarté que la géométrie peut répandre sur les questions les plus obscures et les plus difficiles. . . . Il est certain qu'on ne doit négliger ni l'une ni l'autre; elles sont au fond presque toujours unies dans nos ouvrages, et forment ensemble comme l'instrument le plus complet de l'esprit humain. Car notre esprit ne marche guère qu'à l'aide des signes et des images; et quand il cherche à pénétrer pour la première fois dans les questions difficiles, il n'a pas trop de ces deux moyens et de cette force particulière qu'il ne tire souvent que de leur concours. C'est ce que tout le monde peut sentir, et ce qu'on peut reconnaître dans le Mémoire même." (Chasles, 'Rapport sur les progrès de la géometrie,' 1870, p. 105, &c.)

of bringing some order into the tangled web of mathematical speculation, mainly represented by these, I shall identify the name of Plücker with the great advance which has taken place in geometry through the change in our ideas as to the elements of space construction and the generalisation of our ideas of co-ordinates: with Chasles I shall specially connect the modern habit in geometry of combining figures in finite space with their infinitely distant elements, and with Cayley the application to geometrical science of the novel and comprehensive methods of modern algebra. Let us dwell for a moment on each of these three great departures.

32. Plücker, Chasles, Cayley.

The elements of any science are a very different thing from the elements of the special object with which that science is concerned. The elements of chemistry are not the chemical elements. The latter are, we suppose, something existing in nature, something fixed and unalterable, which science aims at finding out; the former are certain conceptions from which we find it convenient to start in teaching, expounding, and building up the science of chemistry. The latter are artificial, the former are natural. The same remark obtains in geometrical science. The elements of geometry have an historical, a practical beginning: the elements of space form a conception which gradually emerges in the progress of geometrical science. In every science there is a tendency to replace the casual and artificial elements by the natural or real elements, and to build up the historical traditional body of doctrine anew, using the very elements which Nature herself, as it were, employs in producing her actual forms and objects. As the pass-

33. Historical and logical foundations.

age quoted above shows, such an idea must have been before the mind of Jacob Steiner when he wrote the 'Systematische Entwickelung.' Through Euclid geometricians had learnt to begin with the straight line of definite—not indefinite—length, the triangle, the circle, advancing to more complicated figures; practice had made geometry a science of mensuration, involving number; the convenience of practice in astronomy, geodesy, and geography had introduced the artifice of referring points and figures in space to certain arbitrarily chosen data—points and lines. The terms "right ascension" and "declination," "altitude" and "azimuth," "latitude" and "longitude," led to the co-ordinates of Descartes and to analytical geometry. In this older and modern geometry, the beginnings were arbitrary, and many conceptions were introduced which were foreign to the object of research. It was through a slow process that in quite recent times—notably during the nineteenth century — mathematicians became aware how artificial were their methods, and with how many foreign elements they had encumbered the objects of their study. To replace the artificial by natural conceptions, and to open the eyes of geometricians to the advantage of not confining themselves to the point (its motion and distances) as the element in their space construction, no one did more than Julius Plücker of Bonn. We have now not only a point-geometry, but likewise a line-geometry — *i.e.*, we have a geometry in which the line is the primary element, the point being the secondary element, defined by the intersection of two lines. This conception, which

can be applied also to geometry in space, the point being conceived as generating a plane by its motion, or three planes defining a point by their intersection, leads us to the same idea of dual correspondence or reciprocity which Poncelet and Gergonne had arrived at by entirely different considerations. Plücker's was an analytical mind, and with him the principle of duality at once assumes an analytical form. He saw that the same equation lent itself to a twofold interpretation, accordingly, as we adopt point co-ordinates or line co-ordinates —*i.e.*, according as we refer our geometrical figure to the point or the line as the moving and generating space element. Through this step the idea of co-ordinates 34. Generalised co-ordinates.
was generalised, and the dualistic conception of figures in space received an analytical expression. It was the junction of analytical and descriptive methods on a higher level, from which an entirely novel and fertile development of geometry became possible.

Whilst the labours of Plücker lay in the direction of making analytical formulæ more natural, better adapted to the expression of geometrical forms and relations, and of reading out of these remodelled formulæ novel geometrical properties, the French school, with Michel Chasles [1]

[1] In addition to numerous valuable memoirs, Chasles published, among others, two works of paramount importance, inasmuch as they for a long time dominated purely geometrical research, not only in France but also in Germany and England,—the 'Aperçu historique sur l'origine et le développement des méthodes en géométrie' (1837), and the 'Traité de géométrie supérieure' (1852). These works, through their brilliant style, not only threw into the shade for a time the labours of contemporary German mathematicians, such as Möbius, Steiner, Plücker, and Von Staudt, but also obscured some of the single discoveries of the author himself. The 'Aperçu' was early translated into German; whereas in this country it was the Dublin school, notably Townsend and Dr Salmon, who spread a knowledge of Chasles's work.

as its leader and centre, laboured at the introduction into pure geometry of those ideas which were peculiar to the analytical method, and which gave to that method its unity, generality, and comprehensiveness. Two ideas presented themselves as requiring to be geometrically dealt with: the infinite and the imaginary—*i.e.*, the elements of a figure which lie at infinity and those which are ideal or invisible, which cannot be construed. It is usually supposed that the consideration in geometry of imaginary or invisible elements in connection with real figures in space or on the plane has been imported from algebra; but the necessity of dealing with them must have presented itself when constructive geometry ceased to consider isolated figures rigidly fixed, when it adopted the method of referring figures to each other, of looking at systems of lines and surfaces, and of moving figures about or changing them by the processes of projection and perspective. The analytical manipulations applied to an equation, which according to some system or other expressed a geometrical figure, found its counterpart in projective geometry, where, by perspective methods,—changing the centre or plane of projection,—certain elements were made to move away into infinity, or when a line that cut a circle moved away outside of it, seemingly losing its connection with it. By such devices, implying continuous motion in space, Poncelet introduced and defined points, lines, and other space elements at
35. Ideal elements. infinity, and brought in the geometrical conception of ideal and imaginary elements. "Such definitions," he says, "have the advantage of applying themselves at once to all points, lines, and surfaces whatsoever; they

are, besides, neither indifferent nor useless, they help to shorten the text and to extend the object of geometrical conceptions; lastly, they establish a point of contact, if not always real, at least imaginary, between figures which appear—*prima vista*—to have no mutual relation, and enable us to discover without trouble relations and properties which are common to them."[1] It was the principle of geometrical continuity which led Poncelet to the consideration of infinite and imaginary elements.

As we saw above, the projective methods of Poncelet had introduced into geometrical reasoning a remarkable distinction among the properties of figures. In general it was recognised that, in the methods of central and parallel projection or in drawing in perspective, certain properties or relations of the parts of a figure remain unaltered, whereas others change, become contorted or out of shape. Poncelet called the former projective or descriptive, the latter metrical, properties. This distinction introduced into all geometry since his time several most important and fundamental points of view; it divided geometrical research into two branches, which we may term positional and metrical geometry—the geometry of position and that of measurement. We know that ancient geometry started from problems of mensuration: modern geometry started, with Monge, from problems of representation or graphical description. It has thus become a habit to call ancient geometry metrical, modern geometry projective. This habit has led to an unnecessary separation of views, but in the further course of development also

[1] 'Traité des Propriétés projectives,' vol. i. p. 28.

to a unification on a higher level. But the distinction mentioned above led to another most remarkable line of thought and research which tends more and more to govern mathematical doctrine. The methods of projection are based upon the motion or upon the transformation of figures. Under such a process some relations remain unaltered or invariant, others change. As analytical methods in the hands of Plücker and others began to accommodate themselves more closely to geometrical forms, as an intimate correspondence was introduced between the figure and the formula, it became natural to study the unalterable properties of the figure

36. Invariants.

in the invariant elements of the formula. This is the origin and meaning of the doctrine of Invariants.[1] It is the great merit of the English school of mathematicians, headed by Boole, Cayley, and Sylvester, both to have first conceived the idea of a doctrine of invariant

[1] "In any subject of inquiry there are certain entities, the mutual relations of which, under various conditions, it is desirable to ascertain. A certain combination of these entities may be found to have an unalterable value when the entities are submitted to certain processes or are made the subjects of certain operations. The theory of invariants in its widest scientific meaning determines these combinations, elucidates their properties, and expresses results when possible in terms of them. Many of the general principles of political science and economics can be expressed by means of invariantive relations connecting the factors which enter as entities into the special problems. The great principle of chemical science which asserts that when elementary or compound bodies combine with one another the total weight of the materials is unchanged, is another case in point. Again, in physics, a given mass of gas under the operation of varying pressure and temperature has the well-known invariant, pressure multiplied by volume and divided by absolute temperature. Examples might be multiplied. In mathematics the entities under examination may be arithmetical, algebraical, or geometrical; the processes to which they are subjected may be any of those which are met with in mathematical work. It is the principle which is valuable. It is the idea of invariance that pervades to-day all branches of mathematics" (Major P. A. MacMahon, Address, Brit. Assoc., 1901, p. 526).

forms, and to have foreseen its importance and corresponding significance when applied to a great variety of scientific problems, notably to the projective processes in geometry. These were known to them mainly through the classical treatises of Poncelet and Chasles, the leading ideas of which had been introduced to British students by the labours of the Dublin school.[1]

The investigations referred to mark the junction of two important lines of mathematical research, which had been carried on independently in earlier times, or only united for special purposes or for the solution of special problems. The history of the progress of geometry during the nineteenth century has already shown us the use and interest which belong to two different aspects of the common object, of which the one relies mainly on processes of measurement, including number, the other mainly on processes of description, in-

[1] The history of the doctrine of invariants has been written by Dr Franz Meyer, and is published in the first volume of the 'Jahresbericht der Deutschen Mathematiker Vereinigung' (p. 79 *sqq.*) The fact that this formed the first of the several Reports which the German Mathematical Society has undertaken to publish, testifies to the great importance which belongs to this doctrine in the history of recent mathematics. A concise summary with copious references is given by the same author in the first volume of the 'Encyklopädie der Math. Wissenschaften,' p. 320 *sqq.* How necessary the form and perfection of algebraic operations was for the development of the geometrical conceptions which are laid down, *e.g.*, in the works of Plücker, can be seen in the work of Otto Hesse, who introduced elegance and conciseness into many of the expositions which, for want of this formal development, appear cumbrous in the writings of Plücker. "The analytical form in which Plücker's Researches present themselves is frequently wanting in that elegant form to which we have become accustomed, specially through Hesse. Plücker's calculations frequently bear the stamp of mere aids for representing geometrical relations. That algebraical connections possess an interest in themselves, and require an adequate representation, was realised only by a generation which habitually employed methods that had been largely devised by Plücker himself" (A. Clebsch, 'Zum Gedächtniss an Julius Plücker,' 1872, p. 8. See also Gustav Bauer, 'Gedächtnissrede auf Otto Hesse,' München, 1882).

cluding arrangement. The same difference of views can be established with regard to many other things which form the objects of other sciences. In geometry this difference obtrudes itself, as it were, in its naked form. Thus in all the natural, and even the social, sciences we have become accustomed to look first at the constituent elements or parts of things, to count and measure them, then afterwards to look at their possible arrangement, or existence together in the actual world of nature or society. Astronomy, crystallography, chemistry, geology, the natural history sciences, economics and statistics, the doctrine of chances,—all furnish, especially in their systematic development during the last hundred or hundred and fifty years, examples of the twofold aspect just referred to. The progress of these sciences, as we have abundantly seen, has depended largely upon the application of mathematical methods. As the analysis into elements or parts, and the possible synthesis of such elements in complicated structures, has become everywhere the order of study, so there must exist in the abstract science of mathematics—*i.e.*, in the framework of our scientific reasoning—not only the theory of measurement and number, but also that of combination, form or arrangement, and order.

37. Theory of forms.

The doctrine of forms in the well-known problems of permutations and combinations begins with modern mathematics in the seventeenth century, and received scientific recognition mainly in connection with the doctrine of chances at the hands of James Bernoulli abroad, and of De Moivre in this country. The process of multiplication of binomials and polynomials leads to the formation of combinations, and

where the factors are the same, as in Newton's binomial theorem, to combinations with permutation; and consequently the doctrine of chances and of arrangements in triangular, pyramidal, or other figures is closely connected with the doctrine of series and algebraical expressions. In this country the interest in the subject has been stimulated and kept alive by isolated problems and puzzles in older popular periodicals, such as the 'Gentleman's Magazine' and the 'Ladies' Diary'; in Germany—as we noticed before—a school of mathematicians arose who attempted a systematic treatment of the whole subject, which, owing to its barrenness in practical results, brought this line of research somewhat into disrepute. What was wanted was a problem of real scientific interest and a method of abbreviation and condensation. Both were supplied from unexpected [1]

[1] The theory of arrangement or of order, also called the "Ars Combinatoria," has exerted a great fascination on some master minds, as it has also given endless opportunities for the practical ingenuity of smaller talents; among the former we must count in the first place Leibniz, and in recent times J. J. Sylvester, who conceived the "sole proper business of mathematics to be the development of the three germinal ideas—of which continuity is one, and order and number the other two" ('Philosophical Transactions,' vol. clix. p. 613). This idea has been dwelt on by Major MacMahon in his address (Brit. Assoc., 1901, p. 526), who says: "The combinatorial analysis may be described as occupying an extensive region between the algebras of discontinuous and continuous quantity. It is to a certain extent a science of enumeration, of measurement by means of integers as opposed to measurement of quantities which vary by infinitesimal increments. It is also concerned with arrangements in which differences of quality and relative position in one, two, or three dimensions are factors. Its chief problem is the formation of connecting roads between the sciences of discontinuous and continuous quantity. To enable, on the one hand, the treatment of quantities which vary *per saltum*, either in magnitude or position, by the methods of the science of continuously varying quantity and position, and, on the other hand, to reduce problems of continuity to the resources available for the management of discontinuity. These two roads of research should be regarded as penetrating deeply into the domains which they connect."

quarters—the one purely theoretical, the other practical. Accordingly the doctrine of forms and arrangements has during the last century been developed by mathematicians in two distinct interests, which only quite lately seem to approach and assist each other.

38. Theory of numbers. The purely abstract or theoretical interest came from the side of the theory of numbers, a branch of research which was revived by Legendre in France and by the youthful genius of Gauss in Germany; the more practical one came from the theory of equations, notably in its application to problems of geometry. The methods by which these subjects were treated had in the early part of the nineteenth century undergone a great change. The older inductive method in both branches—namely, in the solution of equations and in the investigation of the properties of numbers—relied mainly on ingenious devices which were mostly of special, not of general, value. Theorems were found by induction, and had afterwards to be proved by rigorous logical deduction. Success depended on the degree of care with which the mind operated with mathematical symbols, and rested frequently on the intuition, if not the inspiration, of genius. Two of the greatest mathematical minds—Fermat[1] in France and Newton[2] in England—stood

[1] Pierre Fermat (1601-65) prepared an edition of the Treatise of Diophantus, and his marginal notes contain many theorems referring to the properties of numbers which have been the subject of much comment and examination by mathematicians of the first rank down to the present day. In letters to contemporaries he referred to many of these discoveries, and to his proofs, which he did not communicate. Some of these proofs seem not to have satisfied him, being deficient in rigour. In spite of the labours of Euler, Lagrange, Cauchy, Dirichlet, Kummer, and others, one of these theorems still awaits proof. A full account of Fermat's theorems is given in Cantor's 'Geschichte der Mathematik,' vol. ii. 2nd ed., p. 773 *sqq.* Also in W. Rouse Ball's 'History of Mathematics,' p. 260 *sqq.*

[2] Newton, in his 'Universal

foremost in having with unrivalled fertility propounded theorems which were as difficult to prove as the manner in which they had been arrived at was mysterious. The great analytical genius of Euler, who possessed unequalled resources in the solution of single problems, spent much time and power in unravelling the riddles of Fermat. In the theory of equations the general solution beyond the fourth degree baffled the greatest thinkers. The time had come when in both branches a systematic study of the properties had to be attempted. This was done for the theory of numbers by Gauss, for that of equations by Abel. Every great step in advance of this kind in mathematics is accompanied by, and dependent on, skilful abbreviations, and an easy algorithm or mathematical language. An assemblage of elements held together by the simplest operations or signs of arithmetic — namely, those of addition and multiplication — is much easier to deal with if it can be arranged with some regularity, and accordingly methods were invented by which algebraical expressions or forms were made symmetrical and homo- 39. Symmetry.
geneous;[1] the latter property signifying that each term

Arithmetic,' gave an interesting theorem by which the number of imaginary roots of an equation can be determined; he left no proof, and the theorem was discussed by Euler and many other writers, till at last Sylvester in 1866 found the proof of it in a more general theorem. In more recent times Jacob Steiner published a great number of theorems referring to algebraical curves (see Crelle's 'Journal,' vol. xlvii.) which have been compared by Hesse with the "riddles of Fermat." Luigi Cremona succeeded at last in proving them by a general synthetical method.

[1] The introduction of homogeneous expressions marks a great formal advance in algebra and analytical geometry. The first instance of homogeneous co-ordinates is to be found in Möbius's "Barycentric Calculus" (1826), in which he defined the position of any point in a plane by reference to three fundamental points, considering each point as the centre of gravity of those points when weighted. "The idea of co-ordinates appears here for the first time in a new

contained the same number of factors. Such forms could be written down on the pattern or model of one of their terms by simple methods of exchange or permutation of the elements. It would then not be necessary to write down all the terms but only to indicate them by their elements, these also being abbreviated by the use of indices. Rows and columns or arrangements in squares suggested themselves as easy and otherwise well-known artifices by which great masses of statistics and figures are marshalled and controlled. Out of these manifold but simple devices there grew an algebra of algebra, a symbol for denoting in a very general way symmetrical and homogeneous algebraical expressions.[1]

40. Determinants.

Gauss termed such expressions Determinants: they turned up in his 'Disquisitiones Arithmeticæ' as they had done half a century before in Cramer's 'Analyse des lignes courbes algébriques.' Just as common fractions can be

garb, which soon led to a more general conception. The Barycentric co-ordinates were the first instance of homogeneous co-ordinates, . . . and already with Möbius the advantages become evident through the symmetry and elegance of his formulæ" (Hankel, 'Project. Geom.,' p. 22).

[1] Determinants were first used by Leibniz for the purpose of elimination, and described by him in a letter to the Marquis de l'Hospital (1693). The importance of his remarks was not recognised and the matter was forgotten, to be rediscovered by Cramer in the above-named work (1750, p. 657). It is interesting to note that the same difficulty of the process of elimination induced Plücker to resort to geometrical interpretation of analytical expressions, and that whilst he "saw the main advantage of his method in avoiding algebraical elimination through a geometrical consideration, Hesse showed how, through the use of Determinants, algebraical operations could receive that pliability the absence of which was the reason for Plücker to discard it." (See the account of Clebsch's work in 'Math. Ann.,' vol. vii. p. 13.) Through this invention the combinatorial analysis, which, in the hands of the school in Germany, had led into a desert, was raised again into importance. It has become still more important since the general theory of forms and of groups began to play an increasing part in modern analysis.

dealt with as if they were special things having special properties, though the latter depend only on the properties of the numbers they are made up of and their mode of connection; as powers and surds are separately examined; so the arrangements called determinants can be subjected to a special treatment, their properties ascertained, and themselves subjected to the ordinary operations of arithmetic. This doctrine, which constitutes the beginning and centre of the theory of algebraical forms or "quantics" and of algebraical operations or "tactics," was pretty fully worked out and first introduced into the course of teaching by Cauchy in France; then largely adopted by Jacobi in Germany, where Otto Hesse, trained in the ideas of Plücker, first showed its usefulness in his elegant applications to geometry. In France it was further developed by Hermite, who, together with Cayley and Sylvester in England, proclaimed the great importance of it as an instrument and as a line of mathematical thought.[1] In the latter country the idea of abbreviating and summarising algebraical operations had become quite familiar through another device which has not found equal favour abroad—namely, the Calculus of

[1] "For what is the theory of determinants? It is an algebra upon algebra; a calculus which enables us to combine and foretell the results of algebraical operations, in the same way as algebra enables us to dispense with the performance of the special operations of arithmetic. All analysis must ultimately clothe itself under this form." In this connection Sylvester ('Phil. Mag.,' 1851, Apl., p. 301) refers to Otto Hesse's "problem of reducing a cubic function of three letters to another consisting only of four terms by linear substitutions—a problem which appears to set at defiance all the processes and artifices of common algebra," as "perhaps the most remarkable indirect question to which the method of determinants has been hitherto applied."

41. Calculus of Operations.

Operations, the idea of treating algebraical operations and their symbols as quantities, and of subjecting them to arithmetical treatment separately from the material operated on. The genius of Arthur Cayley was specially fertile in this direction, as was that of Sylvester in the nomenclature or language of the doctrine of forms.[1] The merit, however, of having brought together the new ideas which emanated from the schools of Poncelet and Chasles in France, of Cayley and Sylvester in England, into a connected doctrine, and of having given the impetus to the fundamental re-

[1] The theory of invariants was gradually evolved from many independent beginnings. In 1864 Sylvester wrote ('Phil. Trans.,' p. 579), "As all roads are said to lead to Rome, so I find, in my own case at least, that all algebraical inquiries, sooner or later, end at the Capitol of Modern Algebra, over whose shining portal is inscribed the Theory of Invariants." About the same time (1863) Aronhold developed the principal ideas which lay at the foundation of the theory in organic connection and in complete generality, hereby domiciling in Germany the doctrine which had previously owed its development mainly to English, French, and Italian mathematicians (see Meyer, 'Bericht,' &c., p. 95). The different roads which Sylvester refers to can be traced, first, in the love of symbolic reasoning of Boole, who was "one of the most eminent of those who perceived that the symbols of operation could be separated from those of quantity and treated as distinct objects of calculation, his principal characteristic being perfect confidence in any result obtained by the treatment of symbols in accordance with their primary laws and conditions, and an almost unrivalled skill and power in tracing out these results" (Stanley Jevons in article "Boole," 'Ency. Brit.'); secondly, in the independent geometrical labours of Hesse in Germany (whose mathematical training combined Plücker's and Jacobi's teaching) and Dr Salmon in Dublin (who, after having transplanted Poncelet and Chasles to British soil, recognised the importance of Cayley's and Sylvester's work, and introduced in the later editions of his text-book modern algebraical methods); thirdly, in the independent investigations belonging to the theory of numbers of Eisenstein in Germany and Hermite in France. In full generality the subject was taken up and worked out by Sylvester in the 'Cambridge and Dublin Mathematical Journal' (1851-54), and by Cayley in the first seven memoirs upon Quantics (1854-61), which "in their many-sidedness, together with the exhaustive treatment of single cases, remain to the present day, for the algebraist as well as for the geometrician, a rich source of discovery" (Meyer, *loc. cit.*, p. 90).

modelling of the text-books and school-books of algebra and geometry in this country and in Germany, belongs undeniably to Dr Salmon of Dublin.[1] The conception of a form—be this geometrical or algebraic—suggests the investigation of the change, the recurrence of forms. How do forms under the process of geometrical or algebraical manipulation alter or preserve their various properties? The processes of projection practised by Monge, Poncelet, and Chasles in France had already led to a distinction between descriptive and metrical properties of geometrical figures. A corresponding examination of algebraical forms, which are all capable of geometrical representation or interpretation, would lead to the extensive and fundamental doctrine of the invariants of these forms—*i.e.*, of such arrangements of the elements as remain absolutely or proportionally unaltered during the processes of change and combination. Notably instead of the geometrical process of projection by central perspective we may employ in our algebraic formulæ a corresponding process, that which is known as linear substitution. And at the time when it was recognised that geometrical transformation had its

[1] Of Dr Salmon, whose 'Lessons introductory to the Modern Higher Algebra' appeared in 1859 (4th ed., 1855; 1st German ed. by Fiedler, 1863), Meyer says: "Recognising how the special results in this domain gradually acquired a considerable bulk, we must the more gratefully acknowledge the work of Salmon—who had already, in the direction of algebra as well as of geometry, furnished valuable contributions of his own—in undertaking the labour of collecting the widely-scattered material in a concise monograph. For the promulgation in Germany we have to thank Fiedler both for his edition of Salmon, and for having already given an independent introduction to the subject, in which especially he made Cayley's applications to projective geometry generally accessible. About the same time (1862) there appeared likewise an edition by Brioschi, which gained many adherents for the theory of Invariants in Italy."

counterpart in the transformation of algebraical forms by the processes of substitution, these latter had already been extensively studied for their own sakes in the theory of algebraical equations, which in the first quarter of the century had undergone a great development under the hands of two brilliant mathematical talents both lost to science at an early age—the Norwegian Abel and the Frenchman Évariste Galois.[1]

42. Principle of substitution.

Like all algebraical expressions, those termed equations were originally invented and commanded attention

[1] Évariste Galois is held to have been one of the greatest mathematical geniuses of modern times, who, if he had lived, might have been a rival of Abel: he was born in 1811, and died before he was twenty-one, in consequence of a duel. For a long time his writings remained unpublished and unknown, till Liouville published them in the 11th vol. of his 'Journal' (1846). Liouville was also the first to recognise the importance and absolute correctness of Galois's method, which, when submitted to the Academy in the year 1831, and reported on by Lacroix and Poisson, had appeared almost unintelligible. On the eve of his death Galois addressed a letter to his friend Auguste Chevalier, which is a unique document in mathematical literature, forming a kind of mathematical testament. He desires this letter to be published in the 'Revue Encyclopédique,' referring publicly the "importance," not the "correctness," of his discoveries to the judgment of Jacobi and Gauss, and expressing the hope that some persons would be found who would take the trouble to unravel his hieroglyphics. The first attempt to make Galois's ideas generally accessible is to be found in Serret's 'Algèbre Supérieure' (3rd ed., 1866), but it was not till after the publication of Camille Jordan's 'Théorie des Substitutions' (1870) that the short papers of Galois were recognised as containing the germs and beginnings of an entirely novel and comprehensive mathematical theory —viz., the "Theory of Groups." The relation between the writings of Abel and Galois is exhaustively treated in Prof. Sylow's Paper on Abel's work, contained in the 'Memorial Volume,' 1892, p. 24. He there says: "Le mérite de Galois ne consiste pas essentiellement dans ses propositions, mais dans la généralité de la méthode qu'il appliqua. C'est son admirable théorème fondamental qui a donné à la théorie des équations sa forme définitive, et d'où est sortie, en outre, la théorie des groupes généralisée, qui est d'une si grande importance, on peut le dire, pour toutes les branches des mathématiques, et qui déjà, entre les mains de Jordan, de Klein, de Lie, de Poincaré et d'autres, a enrichi la science d'une longue suite de découvertes importantes." The memoirs of Abel and Galois referring to the Theory of Equations have been conveniently edited, in a German translation, by H. Maser, 1889. See also Cayley's article on "Equation" in the 'Ency. Brit.,' § 32.

as instruments or devices for the solution of definite problems in arithmetic, geometry, and mechanics. The solution of the equation—*i.e.*, the expression of the unknown quantity in terms of the known quantities—served a practical end. Gradually as such solutions became more and more difficult, owing to the complexity of the formulæ, the doctrine divided itself into two distinct branches, serving two distinct interests. The first, and practically the more important one, was to devise methods by which in every single case the equations which presented themselves could be solved with sufficient accuracy or approximation; this is the doctrine of the numerical solution of equations. The other more scientific branch looked upon equations as algebraical arrangements of quantities and operations which possessed definite properties, and proposed to investigate these properties for their own sake. The question arose, How many solutions or roots an equation would admit of, and whether the expression of the unknown quantity in terms of the known quantities was or was not possible by using merely such operations as were indicated by the equation itself—*i.e.*, the common operations and the ordinary numbers of arithmetic? This doctrine of the general properties of equations received increasing attention as it became empirically known that equations beyond the fourth degree could not be solved in the most general form.[1] Why could they not be solved,

43. General solution of equations.

[1] Since the researches regarding the solubility of Equations have led on, through Galois and the French analysts, to the same line of reasoning as other researches mentioned before—viz., toward the development of the theory of groups—the history of the whole subject has aroused special interest. The earlier beginnings and the labours of forgotten analysts have been un-

and what were the conditions—*i.e.*, the special properties—of an equation which rendered it soluble? These were some of the questions which the great mathematicians, such as Gauss, Abel, and Galois, placed before themselves during the earlier part of the century. There are other unsolved problems which the nineteenth century inherited from preceding ones, where the same line of reasoning was adopted—*i.e.*, where the question was similarly reversed. Instead of trying to solve problems as yet unsolved, it was proposed to prove their general insolubility, and to show the reason of this; also to define the conditions which make a solution possible.

earthed and placed in their correct historical perspective. Prof. Burkhardt of Göttingen, to whom we also owe the chapter on this subject in the first volume of the 'Encyklopädie,' &c., contributed in the year 1892 a most interesting historical paper, "Die Anfänge der Gruppentheorie und Paolo Ruffini" ('Abhandl. zur Gesch. der Math.,' 6 Heft). In this paper he also goes back to other earlier analysts, among them Prof. Waring of Cambridge, who during his lifetime used to complain that he knew of no one who read his mathematical tracts. It appears that during nearly the last thirty years of the eighteenth century nothing had been added regarding the general theory of equations, and that Ruffini was the first to begin a new epoch in the year 1799, with the distinct assertion that a general solution of algebraic equations beyond the fourth degree, by means of radicals, was impossible, and with an attempt to prove this. His researches were therefore contemporaneous with those of Gauss, who published his 'Dissertation' (see note p. 644) in the same year, and his great arithmetical work in 1801. Although Gauss seems to have arrived at the same conclusion, and perhaps even to have anticipated much later attempts to solve thegeneral equation of the fifth degree by other than algebraical operations (see Sylow, *loc. cit.*, p. 16), his published researches rather took the line of the study of a definite class of soluble equations which were connected with the celebrated problem of the division of the circle; a satisfactory proof of Ruffini's statement being withheld till Abel published his celebrated memoir in the year 1825 in the first volume of Crelle's 'Journal.' With this memoir the theory of equations entered a new phase, towards which the labours of Ruffini were preparatory. As in so many other cases, so also in this, the solution of the problem depended upon stricter definitions of what was meant by the solution of an equation, and by "algebraical" and other ("transcendental") functions and operations. We know that both Abel and Galois began their research by futile attempts to find a solution of the general equation of the fifth degree.

In following this altered course of investigation, an enormous amount of mathematical knowledge was gained, and problems were solved which had previously never been thought of. Especially through the theory of equations the abstract doctrine of algebraical forms was created and greatly advanced long before it was generally recognised that it had peculiar importance through the correspondence or parallelism which existed between algebraical expressions and geometrical configurations.

44. Theory of groups.

Out of these earlier algebraical and later combined algebraical and geometrical investigations, a novel and very useful point of view has been gradually gained which represents the most general conception of mathematical tactics. This centres in the notion of a group of elements. These elements may be quantities or operations, so that the theory of Groups embraces not only the doctrines which deal with quantities but also those which deal with arrangements and their possible changes. The older combinatorial analysis dealt mainly with assemblages of a quantity of separate elements, their number, their variety: the modern theory of groups deals rather with the processes and operations by which different arrangements can be transformed one into the other. It is an algebra of operations. The methods of transformation which presented themselves first of all were the methods known in algebra as substitution. Accordingly the first comprehensive treatise on the theory was the 'Treatise on Substitutions,' published in 1870 by M. Camille Jordan. This book forms a landmark in modern mathematics; it brought into a system

the beginnings of the new and comprehensive calculus of operations which were contained in the writings of Lagrange, Abel, Cauchy, and Galois, and established the terminology and the algorithm. A group of substitutions is defined as having the property that each two or more operations belonging to it and successively applied can be replaced by another single operation contained in the same group. Succeeding operations are symbolically represented by the product of two or more letters. This product has certain algebraical properties, and in analogy with common products it has factors, a degree, an index; the substitution may be cyclical and symmetric, and may have many other remarkable properties which the theory[1]

[1] The "Theory of Groups" has now grown into a very extensive doctrine which, according to the late Prof. Marius Sophus Lie (1842-99), is destined to occupy a leading and central position in the mathematical science of the future. "The conception of Group and Invariant was for him not only a methodical aspect from which he intended to review the entire older region of mathematics, but also the element which was destined to permeate and unify the whole of mathematical science" (M. Nöther, 'Math. Ann.,' vol. liii. p. 39). But though it is an undoubted fact that the largest systematic works on the subject emanate from that great Norwegian mathematician, and that his ideas have won gradual recognition, especially on the part of prominent French mathematicians, notably M. Picard ('Traité d'Analyse,' 1896, vol. iii.) and M. Poincaré, the epoch-making tract which pushed the novel conception into the foreground was Prof. F. Klein's 'Erlangen Programme' (1872), entitled "Vergleichende Betrachtungen über neuere geometrische Forschungen." To those who read and re-read this short but weighty treatise, it must indeed have been like a revelation, opening out entirely new avenues of thought into which mathematical research has been more and more guided during the last generation. The tract, which has now been translated into all the important modern languages, remained for a long time comparatively unnoticed, and, twenty years after its publication, was reprinted by the author in the 43rd volume of the 'Math. Annalen,' with some introductory remarks which indicate the changes that had taken place in the interval as regards the scope of the idea. The main result of the dissertation is this: That, primarily, for all geometrical investigations, the characteristic properties of any manifold (or arrangement) is not the element out of which it is composed, but the group, the transformations of which reveal its invariantive properties. There are, accordingly, as many different ways of studying any manifold (*e.g.*, such as projective geometry, line geometry, geometry of reciprocal radii, Lie's sphere geometry, analysis situs, &c.) as there are continuous groups of transformations that can be established; and there are as many invariant theories (see 'Ency. Math. Wiss.,' vol. ii. p. 402; Nöther, *loc. cit.*, p. 22). From that date onward the different kinds of groups have been defined and systematically studied, notably by Klein and Lie and their pupils. In this country, although many of the relevant ideas were contained in the writings notably of Cayley and of Sylvester, the systematic treatment of the subject was little attended to before the publication (1897) of Prof. Burnside's 'Theory of Groups of Finite Order,' and latterly of his article on the whole Theory of Groups in the 29th volume of the 'Ency. Brit.' It has been remarked by those who have studied most profoundly the development of the two great branches of mathematical tactics —viz., "The Theory of Invariants" and the "Theory of Groups"—that the progress of science would have been more rapid if the English school had taken more notice of the general comprehensive treatment by Lie, and if Lie himself had not refrained from entering more fully into the special theories of that school (see Dr F. Meyer, 'Bericht,' &c., p. 231).

of groups investigates. Its immediate application, and the purpose for which it was elaborated, was the theory of Equations. Every equation constitutes an arrangement in which a finite number of independent elements, called constants or coefficients, is presented under a certain algebraical form. The solution of the equation means the finding of such an arrangement as when substituted in the equation for the unknown quantity, will satisfy the equation.

45. Continuo and discontinuous groups.

The conception of a group of operations standing in the defined relations is, however, capable of a great and fundamental extension into that region of mathematics which deals, not with fixed or constant, but with variable or flowing quantities; not with elements which are disconnected or discontinuous, but with such as are continuous. To understand the development of modern mathematical thought, it is accordingly necessary to go back somewhat and review the progress which the

conception of the variable[1] has undergone in the course of the last hundred years. Here we come upon a term which was introduced into mathematical language mainly through the writings of Euler—the term function. It is used to denote the mathematical dependence of two or more variable quantities on each

[1] To the theory of equations in algebra there corresponds the theory of differential equations in analysis; and as the theory of algebraical equations had gradually emerged in a complete form out of investigations of special equations, or sets of equations, so likewise in analysis a general theory of differential equations is gradually being evolved out of the scattered and very extensive investigations of special differential equations which presented themselves notably in the application of analysis to astronomical and physical problems. It is claimed by those who have grasped the abstract ideas of Sophus Lie, that he has taken a great step forward in the direction of a general theory of differential equations, by applying methods which suggested themselves to him through the general theory of algebraic forms and its connection with geometry. Accordingly, the theories of Lie can be termed an algebraical theory of differential equations, depending upon transformations analogous to those which had been established in the general theory of forms or quantities of which I treated above. Prof. Engel, in his obituary notice of Sophus Lie ('Deutsche Math. Ver.,' vol. viii. p. 35), tells us that in the year 1869-70, when Lie met Prof. Klein in Berlin, the former was occupied with certain partial differential equations which exhibited, under certain transformations, invariantive properties, and that Klein then pointed out "that his procedure had a certain analogy with the methods of Abel. The suggestion of this analogy became important for Lie, as he was generally intent upon following up more closely the analogies with the theory of algebraical equations." Dr H. F. Baker, in his recent article on Differential Equations in the 'Ency. Brit.' (vol. xxvii. p. 448), roughly distinguishes two methods of studying differential equations, which he names respectively "transformation theories" and "function theories," "the former concerned to reduce the algebraical relation to the fewest and simplest forms, eventually with the hope of obtaining explicit expressions of the dependent in terms of the independent variables; the latter concerned to determine what general descriptive relations among the quantities are involved by the differential equations, with as little use of algebraical calculations as may be possible." For the history of thought and connection of ideas, it is interesting to learn, through Prof. Engel, that it was not purely algebraical work,—such as is represented by Galois and Jordan, to which Lie was early introduced by Prof. Sylow,—but the study of Poncelet's and Plücker's methods which led Lie to his original conceptions, and that he was fond of calling himself a pupil of Plücker, whom he had never seen (Engel, *loc. cit.*, p. 34).

other. The question arises, What are we to understand under this term? What is a mathematical function or dependence? The question was approached by the great analysts of the second half of the eighteenth century. A preliminary answer which served the requirements of a very wide field of practical application was given by Fourier at the beginning of the nineteenth century. Since that time the question has been independently treated by two schools of Continental mathematicians. Of these the first was founded by Cauchy in France, and is mainly represented by Bernhard Riemann and his numerous pupils in Germany; the other centres in the Berlin school, headed by Weierstrass, and goes back to the work of Lagrange.

The interests which have led to this modern branch of mathematical research[1] are various, but we can 46. Theory of Functions.

[1] The literature suitable for introducing the student of mathematics to the modern theory of functions—which plays in analysis, *i.e.*, the doctrine of variable quantity, a part of similar importance to that which the theory of forms plays in algebra—is so enormous, the subject being approached from so many sides by different writers, that it seems worth while to refer to two expositions which may be read with profit, and which do not require extensive mathematical knowledge. First and foremost I would recommend Cayley's article on "Functions" in vol ix. of the 'Ency. Brit.' Then there is the chapter on "Foundations of the General Theory of Functions," contained in the 2nd volume of the German 'Mathematical Encyclopedia,' written by Prof. Pringsheim. Cayley's article introduces the general theory after giving a short summary of the more important "known" functions, including those which presented themselves in the first half of the nineteenth century, and which I referred to in dealing with the work of Abel and Gauss (see note, p. 648). The treatment of these latter functions, which had been brought to a certain degree of perfection by Jacobi, had made it evident that more general aspects had to be gained and broader foundations laid. But ever since the middle of the eighteenth century another development of mathematical ideas had been going on which started from the solution of a problem in mathematical physics—namely, that of vibrating strings, which led in the sequel to

distinguish two which are very prominent, and are roughly represented by the two schools just referred to. In the first place, a function can be formally defined as an assemblage of mathematical symbols, each of which denotes a definite operation on one or more quantities. These operations are partly direct, like addition, multiplication, &c.; partly indirect or inverse, like subtraction, division, &c. Now, so far as the latter are concerned, they are not generally and necessarily practicable, and the question arises, When are they practicable, and if they are not, what meaning can we connect with the mathematical symbol? In this way we arrive at definitions for mathematical functions which cannot immediately be reduced to the primary operations of arithmetic, but which form special expressions that become objects of research as to their properties and as to the relation they bear to those fundamental operations upon which all our methods of calculation depend. The inverse operations, represented by negative, irrational, and imaginary quantities; further, the operations of integration in its definition as the

a certain finality when Fourier introduced his well-known series and integrals, by which any kind of functionality or mathematical dependence, such as physical processes seem to indicate, could be expressed. The work of Fourier, which thus gave, as it were, a sort of preliminary specification under which a large number of problems in physical mathematics could be attacked and practically solved, together with the stricter definitions introduced by Lejeune Dirichlet, settled for a time and for practical purposes the lengthy discussions which had begun with Euler, Daniel Bernoulli, d'Alembert, and Lagrange. The abovenamed chapter, written by Prof. Pringsheim, gives an introduction to the subject showing the historical genesis of the conception of function and the various changes it was subjected to, and then proceeds to expositions and definitions mostly taken from the lectures of Weierstrass (see p. 8), whereas Cayley's article introduces us to the elements of the general theory of functions as they were first laid down by Riemann in the manner now commonly accepted.

inverse of differentiation,—led early to investigations of the kind just mentioned. The experience that ordinary fractions might be expressed by decimal fractions—*i.e.*, by finite or infinite series—led to the inverse problem of finding the sum of such series and many other answerable and apparently unanswerable problems. The older method of research consisted in treating these problems when and as they arose: new chapters were accordingly added to the existing chapters of the text-books, dealing with special functions or mathematical expressions. It was only towards the end of the eighteenth century, and at the beginning of the nineteenth, that Lagrange, Gauss, and Cauchy felt and proclaimed the necessity of attacking the question generally and systematically; the labours of Euler having accumulated an enormous mass of analytical knowledge, a great array of useful formulæ, and amongst them not a few paradoxes which demanded special attention. I have already had occasion to refer to the problem of the general solution of equations as an instance where, in the hands of Abel, the tentative and highly ingenious attempts of earlier analysts were replaced by a methodical and general treatment of the whole question. Another chapter of higher mathematics, the investigation of expressions which presented themselves in the problems of finding the length of the arc of an ellipse, and which opened the view into the large province of the so-called higher transcendents, gave Abel further occasion of laying new foundations and of creating a general theory of equations or of forms.

But yet another interest operated powerfully in the

direction of promoting these seemingly abstract researches. Nature herself exhibits to us measurable and observable quantities in definite mathematical dependence;[1] the conception of a function is suggested by all the processes of nature where we observe natural phenomena varying according to distance or to time.

47. Physical analogies.

[1] Nearly all the "known" functions have presented themselves in the attempt to solve geometrical, mechanical, or physical problems, such as finding the length of the arc of the ellipse (elliptic functions); or answering questions in the theory of attraction (the potential function and other functions, such as the functions of Legendre, Laplace, and Bessel, all comprised under the general term of "harmonic functions"). These functions, being of special importance in mathematical physics, were treated independently before a general theory of functions was thought of. Many important properties were established, and methods for the numerical evaluation were devised. In the course of these researches other functions occurred, such as Euler's "Gamma" function and Jacobi's "Theta" function, which possessed interesting analytical properties. These functions, suggested directly or indirectly by applications of analysis, did not always present themselves in a form which indicated definite analytical processes, such as processes of integration or the summation of series. Very frequently they presented themselves, not in an "explicit" but in an "implicit" form; their properties being expressed by certain conditions which they had to fulfil. It then remained a question whether a definite symbol, indicating a set of analytical operations, could be found. This arises from the fact that the solution of most problems in mechanics and physics starts from the assumption that, though the finite observable phenomena of nature are extremely intricate, they are, nevertheless, compounded out of comparatively simple elementary processes, which take place between the discrete atoms, or the elementary but continuous portions of matter. Mathematically expressed, this means that the relations in question present themselves in the form of differential equations, and that the solution of them consists in finding functions of finite (observable) quantities which satisfy the special conditions. A comparatively small number of differential equations has thus been found empirically to embrace very large and apparently widely separated classes of physical phenomena, suggesting physical relations between those phenomena which might otherwise have remained unnoticed. The physicist or astronomer thus hands over his problems to the mathematician, who has either to integrate the differential equations, or, where this is not possible, at least to infer the properties of the functions which would satisfy them—in fact, the differential equation becomes a definition of the function or mathematical relation. In consequence of this the theory of differential equations is, as Sophus Lie has said, by far the most important branch of mathematics.

The attraction of the heavenly bodies varies with the distance, the velocity of a falling stone or the cooling of a hot body varies with the interval of time which has lapsed or flown. We are now so much accustomed to represent such dependence by curves drawn on paper, that we hardly realise the great step in advance towards definiteness and intelligibility that this device marks in all natural sciences and in many practical pursuits. But the representation of the natural connections of varying quantities by curves also forms the connecting link with the other class of researches just mentioned. Descartes had shown how to represent algebraical formulæ by curves in the plane and in space; and at the beginning of the nineteenth century this method was modified by Gauss and Cauchy so as to deal also with the extended conception of number which embraced the imaginary unit. Two questions arise, Is it possible to represent every arbitrary dependence such as we meet with in the graphical description of natural phenomena by a mathematical formula—*i.e.*, by a formula denoting several specified mathematical operations in well-defined connections? and the inverse question, Is it possible to represent every well-defined arrangement of symbols denoting special mathematical operations graphically by curves in the plane or in space? The former question is one of vital importance in the progress of astronomy, physics, chemistry, and many other sciences, and has accordingly occupied many eminent analysts ever since Fourier gave the first approximative answer in his well-known series: the latter question can only be answered by much stricter defini-

tions of all the more advanced and of some even of the elementary operations which analysts had become accustomed to use without a previous knowledge of the range of their validity. All applications of mathematics consist in extending the empiricial knowledge which we possess of a limited number or region of accessible phenomena into the region of the unknown and inaccessible; and much of the progress of pure analysis consists in inventing definite conceptions, marked by symbols, of complicated operations; in ascertaining their properties as independent objects of research; and in extending their meaning beyond the limits they were originally invented for,—thus opening out new and larger regions of thought.

48. The potential.

A brilliant and most suggestive example of this kind of reasoning was afforded by a novel mode of treating a large class of physical problems by means of the introduction of a special mathematical function, termed by George Green, and later by Gauss, the "Potential" or "Potential function."[1] All the problems of Newtonian attraction were concentrated in the study of this formula: and when the experiments of Coulomb and Ampère showed the analogy that existed between electric and magnetic forces on the

[1] See vol. i. p. 231 of this work. The history of the subject has been written by Todhunter ('History of the Theories of Attraction and the Figure of the Earth,' 2 vols., 1873) for the earlier period down to 1832. For the later period see Bacharach's 'Abriss der Geschichte der Potentialtheorie,' Göttingen, 1883; for the connection of the theory with Riemann's mathematical methods, especially Prof. F. Klein's tract, 'Ueber Riemann's Theorie der algebraischen Functionen' (Leipzig, 1882, trans. by F. Hardcastle, Cambridge, 1893); Prof. Carl Neumann's 'Untersuchungen über das Logarithmische und Newtonische Potential' (Leipzig, 1877); Dr Burkhardt's 'Memorial Lecture on Riemann' (Göttingen, 1892); and jointly with Dr Franz Meyer, the same author's chapter on "Potentialtheorie" in the 2nd volume (p. 464) of the 'Encyclopädie der Math. Wiss.,' 1900.

one side, and Newtonian forces on the other; still more when Fourier, Lamé, and Thomson (Lord Kelvin) pointed to the further analogy which existed between the distribution of temperature in the stationary flow of heat and that of statical electricity on a conductor, and extended the analogy to hydrostatics and hydrodynamics,—it became evident that nature herself pointed here to a mathematical dependence of the highest interest and value. Many eminent thinkers devoted themselves to the study of this subject, but it was reserved for Bernhard Riemann to generalise the mode of reasoning peculiar to these researches into a fundamentally novel method for the explanation and definition of mathematical function or dependence.[1]

[1] Although Riemann's original method of dealing in a general way with algebraical functions is here introduced as a generalisation of certain ideas suggested by mathematical physics, it was not in this way that they were introduced to the mathematical world. This was done in his very abstract and difficult memoir, 'Theorie der Abel'schen Functionen' (published in 1857 in vol. liv. of Crelle's 'Journal'). In this memoir the connection which existed with mathematical physics was not patent, and it took a long time before his methods, which seemed to be a development of Cauchy's earlier researches, were understood and fully appreciated. It was only after he had lectured repeatedly on the subject, and initiated a number of younger mathematicians, who now occupy many of the chairs at the German universities, that the discoveries and inventions of Riemann received their deserved appreciation. Even in his own lectures on mathematical physics—notably on partial differential equations (including harmonics) and the theory of the potential—he did not lead up to the fundamental ideas which he developed in his lectures on the theory of the Abelian functions. Some light is thrown on the subject of the genesis of Riemann's ideas by his dissertation written in the year 1851, though even the biographical notice attached to the 1st edition of his works (1876) did not deal with the origins of his theory. It seems, therefore, correct to date the adequate recognition of Riemann's work in wider circles from the publication in 1882 of Prof. F. Klein's tract mentioned above. Like several other short treatises of this eminent living mathematician, it must have thrown quite a new light upon the subject; and, like several of his other writings, it revealed connections between regions of thought which to many students must have appeared isolated. "Through the treatment initiated by Klein, the theory of

49. Riemann. The peculiarity of such dependence, as exemplified in the phenomena of the steady flow of heat or of electric distribution, consisted in this, that if at certain points or in certain regions of space the thermal or electrical conditions were defined and known by actual observation, then the whole distribution in other points and regions was completely determined. Those boundary conditions could therefore be regarded as the necessary and sufficient definition of the whole existing distribution. Translated into mathematical language, this means that functions exist which are completely defined by boundary values and singularities—*i.e.*, values at single points. Nature herself had shown the way to define and calculate measured relations when through their intricacy they evaded the grasp of the ordinary operations of algebra.[1] Plücker had already in geometry (following in the lines of Newton), when attacking the problem of the infinite variety of higher curves, suggested the method of classifying them according to their characteristic properties or singularities. What had been done by geometers and physicists in isolated cases with the expenditure of much ingenuity and skill, Riemann and his school elevated to the rank of a general method and doctrine.

functions acquires a great degree of clearness and connectedness, which is mainly gained by conceptions derived from the (physical) theory of the potential, and thus exhibits the intimate relationship of these theories" (Bacharach, 'Geschichte der Potentialtheorie,' Göttingen, 1883, p. 71).

[1] On this subject see Burkhardt's 'Memorial Lecture on Riemann' (Göttingen, 1892), p. 5. &c.; Bacharach (*loc. cit.*), p. 30, &c. The latter especially with reference to the theorem called by Clerk-Maxwell "Thomson's theorem" ('Cambridge and Dublin Mathematical Journal,' 1848, or 'Reprint of Papers on Electro-statics,' &c., p. 139); and abroad 'Dirichlet's Principle,' after Riemann (1857). Further, Brill and Nöther's "Bericht" ('Math. Ver.,' vol. iii. p. 247); and lastly, a very suggestive address by Prof. Klein ("On Riemann's Influence on Modern Mathematics") to the meeting of the German Association in Vienna in 1804 ('Report,' p. 61).

It is a process of generalisation and simplification. Moreover, Riemann's manner of proceeding brought with it the gain that he could at once make the various theorems of the doctrine of the potential useful for purely mathematical purposes: the equation which defined the potential in physics became the definition of a function in mathematics.[1]

[1] "One may define Riemann's developments briefly thus: that, beginning with certain differential equations which the functions of the complex variable satisfy, he is enabled to apply the principles of the potential theory. His starting-point, accordingly, lies in the province of mathematical physics" (Klein, 'Vienna Report,' *loc. cit.*, p. 60). By starting with physical analogies Prof. Klein evades certain difficulties which the purely mathematical treatment had to encounter. In the preface to his tract of the year 1882, quoted above,—in introducing his method of explaining Riemann's theory,—he says: "I have not hesitated to make exactly these physical conceptions the starting-point of my exposition. Instead of them, Riemannn, as is well known, makes use in his writings of Dirichlet's principle. But I cannot doubt that he started from those physical problems, and only afterwards substituted Dirichlet's principle in order to support the physical evidence by mathematical reasoning. Whoever understands clearly the surroundings among which Riemann worked at Göttingen, whoever follows up Riemann's speculations as they have been handed down to us, partly in fragments, will, I think, share my opinion." And elsewhere he says: "We regard as a specific performance of Riemann in this connection the tendency to give to the theory of the potential a fundamental importance for the whole of mathematics, and further a series of geometrical constructions or, as I would rather say, of geometrical inventions" ('Vienna Report,' p. 61). Klein then refers to the representation on the so-called "Riemann surface," which is historically connected, as Riemann himself points out, with the problem which Gauss first attacked in a general way—viz., the representation of one surface on another in such a manner that the smallest portions of the one surface are similar to those of the other: a problem which is of importance in the drawing of maps, and of which we possess two well-known examples in the stereographic projection of Ptolemy and the projection of Mercator. This method of representation was called by Gauss the "Conformal Image or Representation." His investigations on this matter were suggested by the Geodetic Survey of the kingdom of Hanover, with which he was occupied during the years 1818 to 1830. (See Gauss, 'Werke,' vol. iv., also his correspondence with Schumacher and Bessel.) A very complete treatise on this aspect of Riemann's inventions is that by Dr J. Holtzmüller, 'Theorie der Isogonalen Verwandschaften' (Leipzig, 1882). On the historical antecedents of Riemann's conception, which for a long time appeared somewhat strange, not to say artificial, see Brill and Nöther's frequently quoted "Report" ('Bericht der Math. Verein.,' vol. iii.), p. 256 *sqq.*

In the investigation of those higher functions which the purely analytical methods of Abel and his followers had forced upon the attention of mathematicians, the methods of Riemann proved to be eminently useful and suggestive. But these novel methods themselves had been imported into the pure science from the side of its application in physics. The value of such ideas has always been questioned by another class of thinkers who aim at building up the edifice of the science by rigorous logic, without making use of practical devices which could only be legitimately employed when once their validity had been thoroughly proved and its limits defined. The merit of having done this in the whole domain of those conceptions which, since the age of Descartes, Newton, and Leibniz, had been introduced as it were from the outside into analysis, belongs to the school of mathematicians headed in Germany by Karl Weierstrass.

50. Weierstrass.

Riemann had grown up in the traditions of the school of mathematical thought which was inspired by Gauss and Weber in Göttingen. Geometrical representation and physical application, including the immediate evidence of the senses, formed a large and important factor in the body of arguments by which scientific discovery and invention was carried on in that school; though Gauss himself made logical rigour the final test of maturity in all his published writings, abstaining in many cases from communicating his results when they had not satisfactorily passed that test in his own mind. Through this self-imposed restriction he had permitted important discoveries, which led to large increase of mathematical knowledge, to be anticipated by others.

The cases of Cauchy, Abel, and Jacobi are the best-known instances. Through their labours an entirely new field had been prospected and partially cultivated. It was to this that Weierstrass, the other great leader in modern theory, was attracted. He made the clear definition and logical coherence of the novel conceptions which it involved his principal aim. Gauss had laboured without assistance at similar problems, making many beginnings which even his colossal intellect could not adequately develop. Weierstrass early gathered around him a circle of ardent and receptive pupils and admirers,[1] to whose care and detailed elaboration he

[1] The researches of Weierstrass (1815 to 1897) began somewhat earlier than those of Riemann, but only became generally known and appreciated in their fundamental originality through his pupils—his academic influence dating from the year 1861. Some account of Weierstrass's activity is given by Emil Lampe in the 6th volume (1899) of the 'Bericht der Math. Verein.,' p. 27, &c. The genesis of his ideas is traced by Brill and Nöther in the Report quoted in the last note, and by M. Poincaré in 'Acta Math.,' vol. xxii. The former divides his Researches roughly into two periods, during the first of which (1848-56) he dealt with what Cayley would call "known" functions; progress during this period depending not so much upon fundamentally new ideas as upon an investigation of special problems and great analytical skill. The second period begins in the year 1869, and is devoted to nothing less than the building up of the entire structure of mathematical thought from the very beginning upon altered definitions, through which the dilemmas and paradoxes would be obviated that had shown themselves ever since the middle of the eighteenth century in consequence of a too confident application and extension of conventional ideas suggested mainly by practical problems. The elements of this grand edifice are now largely accepted, not only in Germany, but also in France, Italy, and England. In Germany Prof. O. Stolz, through his works on General Arithmetic, 2 vols. (1885 and 1886), and the Calculus, 3 vols. (1893 to 1899), has probably done more than any other academic teacher to utilise the new system of mathematical thought for the elementary course of teaching. It seems of importance to state, however, that outside of the circle of Weierstrass's influence, and quite within the precincts of Riemann's school, the necessity was felt of strengthening the foundations on which research in higher mathematics was carried on, by going back to the fundamental ideas of arithmetic. The principal representative of this line of research was Hermann Hankel (1839-73), a pupil of Riemann's, who, in the

confided many separate and lengthy investigations. It was through one of these that a test-case, in which existing mathematical definitions broke down, was published in 1872. It forms a kind of era in the history of

middle of the sixties, delivered lectures at the University of Leipsic upon "Complex numbers and their functions," starting in a characteristic manner with that extended algebra which Cauchy and Riemann had used to such good purpose. The first part of these lectures was published in 1867. In the preface Hankel says: "In the natural sciences we witness in recent times the distinct tendency to ascend from the world of empirical detail to the great principles which govern everything special and connect it into a whole—*i.e.*, the desire for a philosophy of nature, not forced upon us from outside, but naturally evolved out of the subject itself. Also in the domain of mathematics a similar want seems to make itself generally felt —a want which has always been alive in England." Had the author not been prematurely taken away, there is no doubt that he would have still more largely contributed to the revolution of mathematical ideas now in progress. As it is, he made one further important contribution, of which more hereafter. In Italy Prof. Ulisse Dini began to lecture in the year 1871 to 1872 on the theory of functions, and published his lectures in 1878. A translation was brought out in Germany (1892) by Prof. Lüroth and Mr A. Schepp, in which many of the modern developments are utilised. In France we owe to M. Jules Tannery a valuable introduction to the theory of functions of one variable, based upon a series of lectures delivered in the École Normale in 1883, in which, as he says (Preface, p. vii), he collected the labours of Cauchy, Abel, Lejeune Dirichlet, Riemann, Ossian Bonnet, Heine, Weierstrass, and others; after which he considers that nothing essential need be added in the way of elucidation of the foundations of the theory. M. Émil Borel published in 1898 'Lectures on the Theory of Functions,' the first of a series of text-books dealing with various aspects of the theory of functions, in which he largely refers to the labours of Weierstrass. Before Weierstrass's theory had become known, however, M. Méray had already entered upon an exposition of the foundations of analysis on lines which had much analogy with those adopted by Weierstrass. In England the late Prof. Clifford had occupied himself in various memoirs with the theories of Riemann; but we owe the first comprehensive treatise, embracing the work of Riemann as well as that of Weierstrass, to Prof. Forsyth ('Theory of Functions of a Complex Variable,' Cambridge, 1893). Almost simultaneously Professors Harkness and Morley published a 'Treatise on the Theory of Functions,' and in 1898 an 'Introduction to the Theory of Analytic Functions,' in which they in the main adopted the point of view of Weierstrass. A very original thinker, whose independent researches reach back to the year 1872, and who played an important part in the investigation of many obscure points, was the late Prof. Paul Du Bois-Reymond, who published in 1882 the first part of his 'Allgemeine Functionentheorie,' containing the

mathematical thought. Up to that time "one would have said that a continuous function is essentially capable of being represented by a curve, and that a curve has always a tangent. Such reasoning has no mathematical value whatever; it is founded on intuition, or rather on a visible representation. But such representation is crude and misleading. We think we can figure to ourselves a curve without thickness; but we only figure a stroke of small thickness. In like manner we see the tangent as a straight band of small thickness, and when we say that it touches the curve, we wish merely to say that these two bands coincide without crossing. If that is what we call a curve and a tangent, it is clear that every curve has a tangent; but this has nothing to do with the theory of functions. We see to what error we are led by a foolish confidence in what we take to be visual evidence. By the discovery of this striking example Weierstrass has accordingly given us a useful reminder, and has taught us better to appreciate the faultless and purely arithmetical methods with which he more than any one has enriched our science." [1]

"metaphysics and theory of the fundamental conceptions in mathematics: quantity, limit, argument, and function" (Tübingen). This work touches the borderland of mathematics and philosophy, as does the same author's posthumous work, 'Über die Grundlagen der Erkenntniss in den exacten Wissenschaften' (Tübingen, 1890), and will occupy us in another place.

[1] M. Poincaré in the 'Acta Mathematica,' vol. xxii., "L'œuvre mathématique de Weierstrass," p. 5. The "test-case" referred to in the text consisted in the publication by Weierstrass (in the year 1872, 'Trans. Berlin Academy,' reprinted in Weierstrass's 'Math. Werke,' vol. ii. p. 71) of the proof of the existence of a continuous function which nowhere possessed a definite (finite or infinite) differential coefficient. This example cleared up a point brought into prominence by Riemann in his posthumously (1867) published Inaugural Dissertation of 1854 ('Werke,' p. 213). The question had already, following on Riemann's suggestions, been discussed by Hermann Hankel in a

Before Weierstrass, Cauchy and Riemann had attempted to define the vague term "function" or mathematical dependence. Both clung to the graphical representation so common and so helpful in analysis since Descartes invented it. We have, of course, in abstract science, a right to begin with any definition we choose. Only the definition must be such that it

remarkable tract on "Oscillating functions," in which he drew attention to the existence of functions which admit of an integral, but where the existence of a differential coefficient remains doubtful. In fact, it appears that the question as to the latter had never been raised; the only attempt in this direction being that of Ampère in 1806, which failed (Hankel, p. 7). Hankel in his original investigation showed that a continuous curve might be supposed to be generated by the motion of a point which oscillated to and fro, these oscillations at the limit becoming infinitely numerous and infinitely small: a curve thus generated would present what he called "a condensation of singularities" at every point, but would possess no definite direction, hence also no differential coefficient. The arguments and illustrations of Hankel have been criticised and found fault with. He nevertheless deserves the credit of having among the first attempted "to gain a firm footing on a slippery road which had only been rarely trodden" (p. 8). In this tract (which is reprinted in 'Math. Ann.,' vol. xx.), as well as in his valuable article on "Limit" (Ersch und Gruber, 'Encyk.,' vol. xc. p. 185, art. "Grenze"), Hankel did much to establish clearly the essential point on which depends the entire modern revolution in our ideas regarding the foundations of the so-called infinitesimal calculus; reverting to the idea of a "limit," both in the definition of the derived function (limit of a ratio) and of the integral (limit of a sum) as contained in the writings both of Newton and Leibniz, but obscured by the method of "Fluxions" of the former and the method of "Infinitesimals" of the latter. Lagrange and Cauchy had begun this revolution, but it was not consistently and generally carried through till the researches of Riemann, Hankel, Weierstrass, and others made rigorous definitions necessary and generally accepted. It is, however, well to note that in this country A. de Morgan very early expressed clear views on this subject. Prof. Voss, in his excellent chapter on the Differential and Integral Calculus ('Encyk. Math. Wiss.,' vol. ii. i. p. 54, &c.), calls the later period the period of the purely arithmetical examination of infinitesimal conceptions, and says (p. 60), "The purely arithmetical definition of the infinitesimal operations which is characteristic of the present critical period of mathematics has shown that most of the theorems established by older researches, which aimed at a formal extension of method, only possess a validity limited by very definite assumptions." Such assumptions were tacitly made by earlier writers, but not explicitly stated.

corresponds with conditions which we meet with in reality, say in geometry and physics, otherwise our science becomes useless: further, our definitions must be consistent, and follow logically from the fundamental principles of arithmetic, otherwise we run the risk of sooner or later committing mistakes and encountering paradoxes. We have two interests to serve: the extension of our knowledge of functions and the rigorous proof of our theorems. The methods of Riemann and 51. Riemann and Weierstrass compared.
of Weierstrass are complementary. "By the instrument of Riemann we see at a glance the general aspect of things—like a traveller who is examining from the peak of a mountain the topography of the plain which he is going to visit, and is finding his bearings. By the instruments of Weierstrass analysis will, in due course, throw light into every corner, and make absolute clearness shine forth."[1] The complementary character of

[1] Poincaré, *loc. cit.*, p. 7. Similarly Prof. Klein (*loc. cit.*, 'Vienna Report,' p. 60): "The founder of the theory [viz., of functions] is the great French mathematician Cauchy, but only in Germany has it received that modern stamp through which it has, so to speak, been pushed into the centre of our mathematical convictions. This is the result of the simultaneous exertions of two workers—Riemann on the one side and Weierstrass on the other. Although directed to the same end, the methods of these two mathematicians are in detail as different as possible: they almost seem to contradict each other, which contradiction, viewed from a higher aspect, naturally leads to this—that they mutually supplement each other. Weierstrass defines the functions of a complex variable analytically by a common formula—viz., the 'Infinite Power Series'; in the sequel he avoids geometrical means as much as possible, and sees his specific aim in the rigour of proof. Riemann, on the other side, begins with certain differential equations. The subject then immediately acquires a physical aspect. . . . His starting-point lies in the region of mathematical physics." We now know from the biographical notice of Riemann, attached to his collected works (1st ed., p. 520), that he was pressed (in 1856) by his mathematical friends to publish a *résumé* of his Researches on Abelian functions—"be it ever so crude." The reason was that Weierstrass was already at work on the same subject. In consequence of Riemann's

the labours of the two great analysts is nowhere better shown than in the special manner in which Weierstrass succeeded in strengthening the foundations[1] on which much of Riemann's work rests.

The labours of the great analysts—Gauss, Cauchy, Riemann, and Weierstrass—all tended to increase our

publication Weierstrass withdrew from the press an extensive memoir which he had presented in the year 1857 to the Berlin Academy, because, as he himself says (Weierstrass, 'Math. Werke,' vol. iv. p. 10): "Riemann published a memoir on the same problem which rested on entirely different foundations from mine, and did not immediately reveal that in its results it agreed completely with my own. The proof of this required investigations which were not quite easy, and took much time; after this difficulty had been removed a radical remodelling of my dissertation seemed necessary," &c. &c. The mutual influence of Riemann's and Weierstrass's work is also referred to by Weierstrass in a letter to Prof. Schwarz, dated 1875, in which he utters what he calls his confession of faith: "The more I ponder over the principles of the theory of functions—and I do this incessantly—the stronger grows my conviction that it must be built up on the foundation of algebraical truths, and that, therefore, to employ for the proof of simple and fundamental algebraical theorems the 'transcendental,' if I may say so, is not the correct way, however enticing *prima vista* the considerations may be by which Riemann has discovered many of the most important properties of algebraical functions. It is a matter of course that every road must be open to the searcher as long as he seeks; it is only a question of the systematic demonstration" (Weierstrass, 'Werke,' vol. ii. p. 235).

[1] This refers mainly to Weierstrass's investigation of the principle called by Riemann "Dirichlet's principle," but which had been stated already with great generality by Thomson (Lord Kelvin) in the year 1847. The validity of this method depended on a certain minimum theorem. Weierstrass has shown that the existence of such a minimum is not evident, and that the argument used is not conclusive. He laid before the Berlin Academy, in the year 1870, a communication giving a test-case to prove that Dirichlet's method was not generally valid ('Werke,' vol. ii. p. 49). "Through this," Prof. Klein says (*loc. cit.*, p. 67), "a great part of Riemann's developments become invalidated. Nevertheless the far-reaching results which Riemann bases upon the principle are all correct, as was shown later on exhaustively and with all rigour by Carl Neumann and H. A. Schwarz. Indeed we must come to the conclusion that Riemann himself arrived at these theorems by a physical intuition, and only afterwards resorted to the principle referred to in order to have a consistent mathematical line of reasoning" (*loc. cit.*, p. 67). See on this also Poincaré (*loc. cit.*, pp. 10 and 15), who gives other instances where the work of Weierstrass supported that of Riemann.

knowledge of the higher mathematical relations, but also to reveal the uncertainty and absence of rigorous definition of the foundations of arithmetic and of geometry. Accordingly we find these great thinkers continually interrupting their more advanced researches by examinations of the principles. This feeling of uncertainty had led, ever since the end of the eighteenth century, to many isolated attacks and half-philosophical discussions by various writers in this country and abroad. Many of them remained long unrecognised; such were the suggestive writings of Hamilton, De Morgan, Peacock in England, Bolzano[1] in Bohemia,

52. Examination of foundations.

[1] The merits of Bernhard Bolzano (1781-1848) as one of the earliest representatives of the critical period of mathematics were recognised after a long interval of neglect by Hankel in his article on "Limit" mentioned above. This philosophical mathematician published many years before Cauchy a tract on the Binomial Theorem (Prague, 1816), in which he gives, in Hankel's opinion, the first rigid deduction of various algebraical series. "Bolzano's notions as to convergency of series are eminently clear and correct, and no fault can be found with his development of those series for a *real* argument (which he everywhere presupposes); in the preface he gives a pertinent criticism of earlier developments of the Binomial Theorem, and of the unrestricted use of infinite series, which was then common. In fact, he has everything that can place him in this respect on the same level with Cauchy, only not the art peculiar to the French of refining their ideas and communicating them in the most appropriate and taking manner. So it came about that Bolzano remained unknown and was soon forgotten; Cauchy was the happy one who was praised as a reformer of the science, and whose elegant writings were soon widely circulated." (Hankel, *loc. cit.*, p. 210.) Following on this statement of Hankel and a remark of Prof. H. A. Schwarz, who looks upon Bolzano as the inventor of a line of reasoning further developed by Weierstrass ('Journal für Mathematik,' vol. lxxiv. p. 22, 1872), Prof. O. Stolz published in 1881 ('Math. Ann.,' vol. xviii. p. 255) an account of the several writings of Bolzano, beginning in the year 1810, in so far as they referred to the principles of the Calculus. "All these writings are remarkable inasmuch as they start with an unbiassed and acute criticism of the contributions of the older literature" (*loc. cit.*, p. 257). A posthumous tract by Bolzano, 'Paradoxieen des Unendlichen,' was republished in 1889 in 'Wissenschaftliche Classiker,' vol. ii., Berlin (Meyer and Müller). As stated above, Hankel was also one of the first to draw attention to the originality and importance of Hermann Grassmann's work.

Bolyai in Hungary, Lobatchevski in Kasan, Grassmann in Stettin. Most of these were unknown to each other. However, near the beginning of the last third of the century three distinct publications created a great stir in the mathematical world, brought many scattered but cognate lines of reasoning together, and made them mutually fertile and suggestive. These three were—*first*, the publication in 1860 of Gauss's correspondence with Schumacher, in which two letters of the former, dated May and July 1831,[1] became known, where he referred to his extensive but unwritten and unfinished speculations on the foundations of geometry and the theorem which refers to the sum of the angles in a triangle. The *second* was the publication in 1867 of the first and only part of Hermann Hankel's "Lectures on the Complex Numbers and their Functions."[2] The *third* was the posthumous publication in the same year of Riemann's paper, dated 1854,[3] "On the Hypotheses which lie at the Foundation of Geometry." Almost simultaneously there appeared the first of Helmholtz's two important papers[4] on the

[1] See 'Briefwechsel zwischen Gauss und Schumacher,' ed. Peters, 1860, vol. ii. pp. 260, 268.

[2] The small volume contains so much original and historical matter that I have on several occasions referred to it. See above, pp. 645, 653.

[3] Riemann, 'Math. Werke,' 1st ed., p. 254 *sqq.*

[4] The first publication of Helmholtz was a lecture on "the actual foundations of geometry," which he delivered on the 22nd May 1868 to the Medical Society at Heidelberg. This communication, which referred to investigations carried on for many years,—notably in connection with the theory of the colour-manifold,—was occasioned by the publication of Riemann's paper in the 'Transactions' of the Göttingen Society. He had heard of this through Schering, to whom he wrote on the 21st April 1868 before having seen Riemann's paper: "I have myself been occupied with the same subject during the last two years, in connection with my researches in physiological optics. . . . I now see, from the few hints which you give as to the result of the investigation, that Riemann has arrived at exactly the same results. My starting-point was the question, How must a magnitude of several dimensions be constituted, if solid bodies are to move in it everywhere continuously, monodromically, and as freely as bodies move in real space?" On receiving from Schering a reply with a copy of Riemann's paper, Helmholtz wrote (18th May), "I enclose a short exposition of that which in my researches on the same subject is not covered by Riemann's work." A fuller paper, with the title "On the Facts which lie at the foundation of Geometry," appeared in the 'Göttinger Nachrichten,' June 3, 1868. See Helmholtz, 'Wiss. Abhandl.,' vol. ii. pp. 610 and 618, &c.; also 'H. von Helmholtz,' by Leo Koenigsberger (1903), vol. ii. p. 138, &c. In another lecture, "On the origin and meaning of the Axioms of Geometry" (1870, reprinted in abstract in 'The Academy,' vol. i.), as well as in an article in vol. i. of 'Mind' (p. 301), he discussed "the philosophical bearing of recent inquiries concerning geometrical axioms and the possibility of working out analytically other systems of geometry with other axioms than Euclid's" (reprinted in vol. ii. of 'Vorträge und Reden').

same subject, through which it became more widely known and attracted the attention of other than purely mathematical writers. The small but eminently suggestive volume of Hankel showed the necessity of a revision and extension of the fundamental principles and definitions[1] of general arithmetic and algebra as

[1] In this treatise Hankel introduced into German literature the three terms "distributive," "associative," and "commutative" to define the three principles which govern the elementary operations of arithmetic, and introduced further what he calls the principle of the permanence of former rules in the following statement: "If two forms, expressed in the general terms of universal arithmetic, are equal to each other, they are to remain equal if the symbols cease to denote simple quantities; hence also if the operations receive a different meaning." Hankel seems to have been led to his definitions by a study of French and English writers, among whom he mentions Servois ('Gergonne's Ann., v. p. 93, 1814) as having introduced the terms "distributive" and "commutative," and Sir W. R. Hamilton as having introduced the term "associative." He further says (p. 15): "In England, where investigations into the fundamental principles of mathematics have always been treated with favour, and where even the greatest mathematicians have not shunned the treatment of them in learned dissertations, we must name George Peacock of Cambridge as the one who first recognised emphatically the need of formal mathematics. In his interesting report on certain branches of analysis, the principle of permanence is laid down, though too narrowly, and also without the necessary foundation." Other writings, of what he terms Peacock's Cambridge school, such as those of De Morgan, Hankel states that he had not inspected; mention-

an introduction to the advanced theories of Gauss and Riemann; and for this purpose he went back to the unnoticed labours of Grassmann in Germany, to the writings of Peacock and De Morgan in England, and incidentally introduced into Germany the elaborate algebra of quaternions, invented and practised by Hamilton twenty years before that time. The papers of Riemann and Helmholtz similarly showed the necessity of a thorough investigation of the principles and foundations of ordinary or Euclidean geometry, and showed how consistent systems of geometry could be elaborated on other than Euclidean axioms. Only

53. Non-Euclidean geometry.

from that moment, in fact, did it become generally recognised that already, a generation before, two independent treatises on elementary geometry had been published in which the axiom of parallel lines was dispensed with and consistent geometrical systems developed. These were contained — as already stated — in the 'Kasan Messenger,' under date 1829 and

ing only a short paper by Dr F. Gregory on Symbolical Algebra in the Edinburgh 'Transactions.' Whilst Hankel was delivering lectures on these fundamentals, Weierstrass in Berlin was likewise in the habit of introducing his lectures on the Theory of Analytic Functions by a discussion of the theory of Complex Numbers. This introduction was published, with Weierstrass's permission, in the year 1872 by Dr E. Kossak (in a programme of the Friedrichs-Werder Gymnasium), after lectures delivered by Weierstrass in 1865-66. To what extent Hankel may have been influenced by Weierstrass's lectures, which he seems to have attended after leaving Göttingen, is uncertain, for in spite of his very extensive references he does not mention Weierstrass. In Kossak's 'Elemente der Arithmetik' the term "permanence of formal rules" is not used, but the treatment of the extended arithmetic is carried on along the same lines—*i.e.*, not by an attempt to represent the complex quantities, but on the ground of maintaining the rules which govern the arithmetic of ordinary numbers. Great importance is also attached to the principle of inversion as having shown itself of value in the theory of elliptic functions, and being not less valuable in arithmetic. As stated above (p. 640, note), this principle is also insisted on by Peacock.

1830, the author being Lobatchevski; and in the appendix to an Introduction to Geometry, published by Wolfgang Bolyai at Maros Vasarheli, a town of Transylvania, the appendix being by the author's son, Johann Bolyai. The elder Bolyai having been a friend and correspondent of Gauss, and his speculations evidently of the same nature as those indicated by the latter in the above-mentioned correspondence, conjectures have been made as to which of the two originated the whole train of thought.[1] The independent investigations of Riemann and Helmholtz started from a differ-

[1] See above, p. 652, note. What is important from our point of view in the investigations of both Riemann and Helmholtz lies in the following points: First, Neither Riemann nor Helmholtz refers to the non-Euclidean geometry of Lobatchevski or Bolyai. This is not surprising in the case of Helmholtz, whose interest was originally not purely mathematical; in fact, we may incidentally remark how, in spite of his profound mathematical ability, he on various occasions came into close contact with mathematical researches of great originality and importance without recognising them—*e.g.*, the researches of Grassmann and Plücker. As regards Riemann, his paper was read before Gauss, who certainly knew all about Bolyai, and latterly also about Lobatchevski, of whom he thought so highly that he proposed him as a foreign member of the Göttingen Society. Gauss could therefore easily have pointed out to Riemann the relations of his speculations with his own and those of the other mathematicians named. Since the publication of the latest volume of Gauss's works, it has become evident that Gauss corresponded a good deal, and more than one would have supposed from reading Sartorius's obituary memoir, on the subject of non-Euclidean (astral or imaginary) geometry, notably with Gerling; and that several contemporary mathematicians, such as Schweikart, came very near to Gauss's own position. Second, although Riemann, and subsequently also Helmholtz, made use of the term "manifold" (*Mannigfaltigkeit*), it does not appear in the course of their discussion that they considered the space-manifold from any other than a metrical point of view. In fact, the manifold becomes in their treatment a magnitude (*Grösse*). It is true that Riemann does refer to certain geometrical relations not connected with magnitude but only with position, as being of great importance. These two points through which the researches of Riemann and Helmholtz stand in relation to other, and at the time isolated, researches, were dwelt on, the first by Beltrami, and the second by Cayley and Prof. Klein.

ent origin: both made use of the more general conception of an extended magnitude, introduced the notion of the curvature of space by analogy with Gauss's measure of curvature of a surface, and tried to express in algebraical formulæ the general and necessary properties of a magnitude which should form the foundation of a geometry. The relation of these algebraical results to those arrived at by the critical and purely geometrical methods of Lobatchevski and Bolyai were set out by Beltrami, who showed clearly that three geometries of two dimensions are possible—the Euclidean, that of Lobatchevski, where the three angles of a triangle are less than two right angles, and a third where they are more. He showed the analogy of the third with geometry on the sphere, and suggested the pseudo-sphere as a surface on which the second could be similarly represented. At the same time he indicated the generalisation through the algebraical formula of the conception of dimensions, and introduced the symbolical term geometry of four or more dimensions, as Grassmann and Cayley had done before him.[1] Through all these investigations a habit

[1] The geometry of non-Euclidean space, as well as the geometry of four or more dimensions (both usually comprised under the term "non-Euclidean geometry"), can now boast of an enormous literature, the enumeration of which alone would fill many pages. A complete bibliography up to the year 1878 is given in vols. i. and ii. of the American 'Journal of Mathematics' by Prof. Bruce Halsted, who has done much to make known to English readers the original writings of the pioneers in this subject. Later publications are referred to in Dr Victor Schlegel's papers ('Leopoldina,' xxii., 1886, Nos. 9-18): "Ueber Entwickelung und Stand der n-dimensionalen Geometrie," &c., &c. In France Houël published (beginning with the year 1866) translations of memoirs referring to this subject; in fact, he was almost the first to draw attention to this important modern departure. But it is almost exclusively owing to the various writings of Prof. Felix Klein that the different points of origin of this most recent mathematical speculation, which are to be found in the mathematical literature of all the principal nations, have been put in the true light and brought into connection. In fact, here, as in several other subjects, his publications, including his lithographed lectures on non-Euclidean geometry (delivered at Göttingen, 1893-94), serve as the best guide through the labyrinth and controversies of this intricate subject. See especially his article "Ueber die so-genannte nicht-Euclidische Geometrie" in vol. iv., 'Math. Ann.,' 1871. In this paper he connects the independent researches of Cayley (following Laguerre, 'Nouv. Ann. de Math.,' 1853), who in his sixth memoir on Quantics showed how metrical geometry can be included in projective geometry by referring figures to a fundamental fixed figure in space called by him the "Absolute," with the independent researches of Lobatchevski, Bolyai, Riemann, and Beltrami.

has been introduced into mathematical writings which has not a little puzzled outsiders, and even exposed the logically rigorous deductions of mathematicians to the ridicule—not to say the contempt—of eminent philosophical authorities. The complete parallelism or correspondence of geometrical with algebraical notions—the possibility of expressing the former with perfect accuracy by the latter, and of retranslating the latter into the former, and this in more than one way, according to the choice of the space element (point, line, sphere), led to the habit of using purely geometrical presentable ideas as names for algebraical relations which had been generalised by the addition of more than a limited number of variables. Thus the conception of curvature, easily defined for a plane curve, and extended by Gauss to surfaces, was, by adding a third variable in the algebraic formula, applied to space. We are then told that it is necessary to understand what is meant by the curvature of space, this being a purely algebraical relation, not really presentable, but only formed by analogy from the geometrically presentable relations of geometry on a surface. In a similar

54. Curvature of space.

way the idea of the dimensions of space was extended, and four and more dimensions freely spoken of when really only a limited number is geometrically presentable. In the hands of mathematicians these terms are useful, and we may discard the criticism of philosophers and laymen as based on misunderstanding.[1] The introduction, however, into geometrical work of conceptions such as the infinite, the imaginary, and the relations of hyperspace, none of which can be directly imaged, has a psychological significance well worthy of examination.[2] It gives a deep insight into the resources and working of the mind. We arrive at the borderland of mathematics and philosophy.

[1] The most important philosophical criticism of the non-Euclidean geometry is that of Lotze, contained in the second book, chap. ii., of the 'Metaphysik' (1879, p. 249, &c.) It must not be forgotten that Lotze wrote at a time when the novel and startling conceptions put forward by popular writers on the subject had been employed in the interest of a spiritualistic philosophy, to the delusions of which some even of Lotze's friends had fallen a prey. This explains the severity of Lotze's criticisms, which are of the very same nature as those he pronounced many years earlier on similar aberrations (see 'Kleine Schriften,' vol. iii. p. 329). Those who are interested in following up the subject should refer to the writings of Friedr. Zöllner as collected in the four vols. of his 'Wissenschaftliche Abhandlungen' (Leipzig, 1878-81). They belong to the curiosities of the philosophical and scientific literature of that age, but can hardly claim a place in the history of thought.

[2] See the remark of Cayley in his Presidential Address ('Coll. Works,' vol. xi. p. 434): "The notion, which is really the fundamental one (and I cannot too strongly emphasise the assertion), underlying and pervading the whole of modern analysis and geometry, is that of imaginary magnitude in analysis and of imaginary space (or space as a *locus in quo* of imaginary points and figures) in geometry. I use in each case the word imaginary as including real. This has not been, so far as I am aware, a subject of philosophical discussion or inquiry. As regards the older metaphysical writers, this would be quite accounted for by saying that they knew nothing, and were not bound to know anything, about it; but at present, and considering the prominent position which the notion occupies—say even that the conclusion were that the notion belongs to mere technical mathematics or has reference to nonentities, in regard to which no science is possible—still it seems to me that (as a subject of philosophical discussion) the notion ought not to be thus ignored; it should at least be shown that there is a right to ignore it."

There exists, moreover, an analogy between the manner in which these novel and extended ideas have been historically introduced and the mode of reasoning which led Sir W. R. Hamilton to the invention of a new and extended algebra—the algebra of quaternions. This analogy becomes evident if we study the small volume of Hermann Hankel, which appeared about the same time as Riemann's and Beltrami's fundamental geometrical dissertations.

55. Generalised conceptions.

The extension of Hamilton was only possible by dropping one of the fundamental principles of general arithmetic, the commutative principle of multiplication, which is symbolically expressed by saying that $a \times b$ is equal to $b \times a$. By assuming that $a \times b$ is equal to $-b \times a$, Hamilton founded a new general arithmetic on an apparently paradoxical principle. Similarly Lobatchevski and Bolyai constructed new geometries by dropping the axiom of parallel lines. Hankel made clear the significance of the new algebra, Riemann and Beltrami that of the new geometry. The practical performance anticipated and led up to the theoretical or philosophical exposition of the underlying principles. But there was a third instance in which a new science had been created by abandoning the conventional way of looking at things. This was the formation of a consistent body of geometrical teaching by disregarding the metrical properties and studying only the positional or projective properties, following Monge and Poncelet. The two great minds who worked out this geometry independently of the conception of number or measurement, giving a purely geometrical definition of distance and number, were Cayley in Eng-

land and Von Staudt in Germany. It was reserved for
56. Klein's exposition. Prof. Felix Klein of Göttingen to show how the generalised notions of distance introduced into geometry by Cayley and Von Staudt opened out an understanding of the three geometries of Euclid, of Lobatchevski, and of Riemann.[1] We have to go back to the purely projective properties of space to understand these different possibilities. Lobatchevski attacked the problem practically, Riemann analytically, Klein geometrically. Through the labours of Klein the subject has arrived at a certain finality. And what was still wanting after he had written his celebrated memoir (which was approved and

[1] See the note on p. 714, above; also 'Math. Ann.,' vol. iv. p. 573, and vol. vi. p. 112. Prof. Klein —following a usage in mathematical language — distinguishes three different geometries, the hyperbolic, the elliptic, and the parabolic geometry, corresponding to the possession by the straight line at infinity of two real or two imaginary (that is, none) or two coincident points. The whole matter turns upon the fact that, although metrical relations of figures are in general changed by projection, there is one metrical relation—known in geometry as the "anharmonic ratio" (in German *Doppelverhältniss*)—which in all projective transformations remains unchanged. As this anharmonic ratio of points or lines can be geometrically constructed without reference to measurement (Von Staudt, 'Geometrie der Lage,' 1847 and 1857), a method is thus found by which, starting from a purely descriptive property or relation, distance and angles—*i.e.*, metrical quantities—can be defined. Some doubts have been expressed whether, starting from the purely projective properties of space and building up geometry in this way (arriving at the metrical properties by the construction suggested by Von Staudt), the ordinary idea of distance and number is not tacitly introduced from the beginning. This may be of philosophical, but is not of mathematical, importance, as the main object in the mathematical treatment is to gain a starting-point from which the several possible consistent systems of geometry can be deduced and taken into view together. See on this point, *inter alia*, Cayley's remarks in the appendix to vol. ii. of 'Collected Works' (p. 604 *sqq.*), also Sir R. S. Ball's paper (quoted there), and more recently the discussion on the subject in Mr Bertrand Russell's 'Essay on the Foundations of Geometry' (1897, p. 31, &c.; p. 117, &c.) See also the same author's article on non-Euclidean Geometry in the supplement of the 'Ency. Brit.,' vol. xxviii.

commented on by Cayley) was later on supplied in consequence of a suggestion of his. The researches of Riemann, and still more those of Helmholtz, had not merely a mathematical, they had also a logical and a psychological, meaning. Space was conceived to be a threefold - extended manifold. There are other manifolds besides space—such, for instance, as the threefold-extended manifold of colours. Helmholtz came from the study of this manifold to that of space. Now the question arises as to the conditions or data which are necessary and sufficient for the foundations of a science like geometry. We have seen that the axiom of parallel lines is not required; we have also seen that the notion of distance and number can be generalised. What other data remain which cannot be dispensed with? Helmholtz had attempted to answer this question. But neither he nor Riemann had considered the possibility of a purely projective geometry. Now it is the merit of Prof. Klein to have seen that there exists a purely algebraical method by which this problem can be attacked. This is the method of groups referred to above, and 57. Sophus Lie.
applied by Sophus Lie to assemblages of continuously variable quantities. Klein was one of the first to recognise the power of this new instrument. He saw that the space problem was a problem of transformations, the possible motions in space forming a group with definite elements (the different freedoms of motion) which were continuously variable—*i.e.*, in infinitesimal quantities—and which returned into themselves under certain well-defined conditions. They possessed, moreover, in the maintenance of distance the algebraic property of in-

variance. He also expressed some doubt regarding the logical consistency of the assumptions of Helmholtz. Sophus Lie undertook this investigation, and thus brought the logical side of the labours of Riemann and Helmholtz to a final conclusion.[1] This is one of the celebrated instances where the rigorous algebraical methods have detected flaws in the more intuitional or purely geometrical process, and extended our knowledge of hidden possibilities.

But there is yet another branch of the great science of number, form, and interdependence, the principles and foundations of which had been handed down from earlier ages, where the critical and sifting process of the nineteenth century has led to an expansion and revolution of our fundamental ideas. Here also, as in so many other directions, the movement begins with Gauss. Hitherto I have spoken mainly of algebra or general arithmetic, of geometry, of the connections of both in the

[1] "Lie was early made aware by Klein and his "program" that the space problem belonged to the theory of groups. . . . Ever since 1880 he had been pondering over these questions; he published his views first in 1886 on the occasion of the Berlin meeting of natural philosophers. Helmholtz's conception was itself unconsciously (but remarkably so, inasmuch as it dates from 1868) one belonging to the theory of groups, trying, as it did, to characterise the groups of the sixfold infinite motions in space, which led to the three geometries, in comparison with all other groups. He did this by fixing on the free mobility of rigid bodies—*i.e.*, on the existence of an invariant between two points as the only essential invariant. When Lie took up this problem in principle, as one belonging to the theory of groups, he recognised that for our space that part of the axiom of monodromy was unnecessary which added periodicity to the free mobility round a fixed axis. . . . The value of these investigations lies mainly in this, that they permit of our fixing for every kind of geometry the most appropriate system of axioms. . . . And they justly received in the year 1897 the first Lobatchevski prize awarded by the Society of Kasan" (M. Nöther, 'Math. Ann.,' vol. liii. p. 38). A lucid exposition of Lie's work will be found in Mr B. Russell's 'Essay,' &c., p. 47 *sqq.*

theory of forms and functions: there remains the science of numbers—of number in the abstract and also of the named numbers of ordinary arithmetic. Gauss's earliest labours were connected with this branch. Superseding the work of Fermat, Euler, and Legendre, he produced that great book with seven seals, the 'Disquisitiones Arithmeticæ.' The seals were only gradually broken. Lejeune Dirichlet did much in this way: others followed, notably Prof. Dedekind, who published the lectures of Dirichlet and added much of his own. The question may be asked, Have we gained any new ideas about numbers?

58. Theory of numbers.

In this abstract inquiry we can again facilitate our survey by distinguishing between the practical and the purely theoretical interests which stimulated it. Looking at the matter as well as the formal treatment by which it was rendered accessible, we may say Gauss not only taught us some very remarkable new properties of numbers—he also invented a new instrument or calculus for their investigation. Let us consider his work and that of his followers from these different points of view.

First, then, there were certain definite problems connected with the properties of numbers which had been handed down from antiquity. Such were the division of the circle into equal parts by a ready geometrical construction, the duplication of the cube, and the quadrature of the circle or the geometrical construction of the number π.[1] To the latter may be attached the

[1] See above, vol. i. p. 181, note. The student will find much interesting matter referring to these problems in Prof. Klein's little volume entitled 'Famous Problems in Elementary Geometry,' transl. by Beman and Smith, Boston and London, 1879. In it is also given

properties of the number e, the basis of the Napierian or natural logarithms, this number having been shown by Euler to stand in a remarkable arithmetical relation to the number π—a relation which could be very simply expressed if one had the courage to make use of the imaginary unit. As in the instance referred to above, when I dealt with the problem of the solution of the higher order of equations, so also in the case of the three celebrated problems now under review, the reasoning of the mathematicians of the nineteenth century lay largely in proving why these problems were insoluble or in defining those special cases in which they were soluble. Moreover, the labours of Gauss and the class of mathematicians who followed or read him were directed towards the defining and fixing of general conceptions, the study and elaboration of which embraced these single problems as special cases. Prime numbers had always been the object of special attention. Division and par-

an account of several mechanical contrivances for the solution of transcendental problems, or of those where the use of the compass and the ruler do not suffice. Although accurate constructions with a ruler and compass, or with either alone, were known to the ancients only in comparatively small numbers, approximations, and sometimes very close ones, seem to have been known. A very interesting example is Röber's construction of the regular heptagon, of which we read in the correspondence of Sir W. R. Hamilton with De Morgan (Life of Hamilton, by Graves, vol. iii. pp. 141, 534), and which was described by him in the 'Phil. Mag.,' February 1864. The approximation to the correctly calculated figure of the true septisection of the circle was so close that he could not discover, up to the 7th decimal, whether the error was in the direction of more or less. On carrying the calculation further, he found the approximation to be such that a heptagon stepped round a circle equal in size to the equator would reach the starting-point within 50 feet. The inventor or discoverer of this method—Röber, an architect of Dresden—supposed that it was known to the ancient Egyptians, and in some form or other connected with the plans of the temple at Edfu, but on this point I have obtained no information. The question is not referred to in Prof. Cantor's 'History of Mathematics.'

tition of numbers had been studied, and many interesting formulæ had been found by induction, and subsequently proved—or not proved—by a multitude of ingenious devices. As in so many other directions of research so also here, the genius of Gauss gave a great impetus to progress by the invention of a definite calculus and an algorithm. This invention referred to the solution of what used to be known as indeterminate equations: to find two or more numbers—notably integers, which obey a certain algebraical relation. For one large class of these problems (which already occupied the ancient geometers), viz., those of the divisibility of one number by another (called the modulus) with or without residue, Gauss invented the conception and notation of a congruence. Two numbers are congruent if when divided by a certain number they leave the same remainder. "It will be seen," says Henry Smith, "that the definition of a congruence involves only one of the most elementary arithmetical conceptions—that of the divisibility of one number by another. But it expresses that conception in a form so suggestive of analysis, so easily available in calculation and so fertile in new results, that its introduction into arithmetic has proved a most important contribution to the progress of the science."[1] Notably the analogy with ordinary algebraic equations and the possibility of transferring the properties and treatment of these was at once evident. It became a subject of

59. Gauss's theory of congruences.

[1] See Henry J. S. Smith in his most valuable 'Report on the Theory of Numbers' (Brit. Assoc., 1859-65, six parts. Reprinted in 'Collected Math. Papers,' vol. i. pp. 38-364). It gives a very lucid account of the history of this department of mathematical science up to the year 1863.

interest to determine the residues of the powers of numbers. A number is said to be a quadratic, cubic, or biquadratic residue of another (prime) number (the modulus) if it is possible to find a square, cube, or biquadratic number which is congruent with the first number. The theory of congruences was a new calculus: as such it was, like the theory of determinants or of invariants or the general theory of forms, a tactical device for bringing order and simplicity into a vast region of very complicated relations. Gauss himself wrote about it late in life to Schumacher.[1] "In general the position as regards all such new calculi is this—that one cannot attain by them anything that could not be done without them: the advantage, however, is, that if such a calculus corresponds to the innermost nature of frequent wants, every one who assimilates it thoroughly is able—without the unconscious inspiration of genius which no one can command—to solve the respective problems, yes, even to solve them mechanically in complicated cases where genius itself becomes impotent. So it is with the invention of algebra generally, so with the differential calculus, so also—though in more restricted regions—with Lagrange's calculus of variations, with my calculus of congruences, and with Möbius's calculus. Through such conceptions countless problems which otherwise would remain isolated and require every time (larger or smaller) efforts of inventive genius, are, as it were, united into an organic whole." But a new calculus frequently does more than this. In the course of its

[1] See 'Briefwechsel,' &c., vol. iv. p. 147; also Gauss's 'Werke,' vol. viii. p. 298.

application it may lead to a widening of ideas, to an enlargement of views, to a removing of artificial and conventional barriers of thought. As I stated early in this chapter, the attempts of Gauss to prove the fundamental theorem of algebra, that every equation has a root, suggested to him the necessity of introducing complex numbers; the development of the theory of congruences and of residues—notably of the higher residues—confirmed this necessity. In the year 1831, in his memoir on biquadratic residues, he announces it as a matter of fundamental importance. In the earlier memoir he had treated this extension of the field of higher arithmetic as possible, but had reserved the full exposition. And before he redeemed this promise the necessity of doing so had been proved by Abel and Jacobi, who had created the theory of elliptic functions, showing that the conception of a periodic function (such as the circular or harmonic function) could be usefully extended into that theory, if a double period—a real and an imaginary one—were introduced. A simplification similar to that which this bold step led to in the symbolic representation of those higher transcendents, had been discovered by Gauss to exist in the symbolical representation of the theory of biquadratic residues which only by the simultaneous use of the imaginary and the real unit "presented itself in its true simplicity and beauty." In this theory it was necessary to introduce not only a positive and negative, but likewise a lateral system of counting—*i.e.*, to count not only in a line backwards and forwards, but also sideways in two directions, as Gauss showed very plainly in the now familiar manner. At the

60. Generalised conception of number.

same time a metaphysical question presented itself—viz., Can such an extension into more than two dimensions be consistently and profitably carried out? Gauss had satisfied himself that it could not;[1] but the proof of this was only given in more recent times by Weierstrass, who definitely founded the whole discussion of the subject on the logical principle "that the legitimacy of introducing a number into arithmetic depends solely on the definition of such number." And this leads me to another extension in the region of number suggested by Gauss's treatment, which has also become fundamental, and, in the hands of Dirichlet, Kummer, Liouville, Dedekind, and others, has remodelled the entire science of higher arithmetic. It is based on the logical process of the

[1] A concise history of this subject is given by Kossak in the Program referred to above, p. 712, note. Gauss had promised to answer the question, "Why the relations between things which have a manifoldness of more than two dimensions would not admit of other" (than the ordinary complex numbers introduced by him) "fundamental quantities being introduced into general arithmetic?" He never redeemed his promise. In consequence of this, several eminent mathematicians, notably Hankel, Weierstrass, and Prof. Dedekind, have attempted to reply to this question, and to establish the correctness of the implied thesis according to which any system of higher complex numbers becomes superfluous and useless. Prof. Stolz, in the first chapter of the second volume of his 'Allgemeine Arithmetik,' gives an account of these several views, which do not exactly coincide. In general, however, the proof given by Weierstrass, and first published by Kossak, has been adopted. This proof is based upon the condition that the product of several factors cannot disappear except one of its factors is equal to zero. "We must, therefore, exclude from general arithmetic complex numbers consisting of three fundamental elements. This is, however, not necessary if the use of them be limited" by some special conditions (Kossak, *loc. cit.*, p. 27). In the course of the further development of this matter Weierstrass arrives at the fundamental thesis "that the domain of the elementary operations in arithmetic is exhausted by addition and multiplication, including the inverse operations of subtraction and division." "There are," says Weierstrass, "no other fundamental operations—at least it is certain that no example is known in analysis where, if an analytical connection exists at all, this cannot be analysed into and reduced to those elementary operations" (p. 29).

inversion of operations in the most general manner. In the direct process we build up algebraical formulæ—called equations or forms—by a combination of addition and multiplication. We can omit subtraction and division, as through the use of negative quantities and fractions these are reduced to the former. Now, given the most general algebraical equation or form, we can search out and define the simple factors or forms into which it can be split up, and these factors and their products we can take to serve as the definition of numbers. The question then arises, What are the properties of numbers thus inversely defined? and, secondly, Do these numbers exhaust or cover the whole extent of number as it is defined by the uses of practical life? The answer to the former question led to the introduction of complex and subsequently of ideal numbers; the discovery by Liouville that the latter is not the case has led to the conception of transcendental, *i.e.*, non-algebraic, numbers.

61. Process of inversion.

The idea of generalising the conception of number, by arguing backward from the most general forms into which ordinary numbers can be cast by the processes of addition and multiplication, has led to a generalised theory of numbers. Here, again, the principal object is the question of the divisibility of such generalised algebraical numbers and the generalised notion of prime numbers—*i.e.*, of prime factors into which such numbers can be divided. Before the general theory was attempted by Prof. Dedekind, Kronecker, and others, the necessity of some extension in this direction had already been discovered by the late Prof. Kummer of

62. Kummer's ideal numbers.

Berlin when dealing with a special problem. This was no other than the celebrated problem of the division of the circle into equal parts, which had been reduced by Gauss to an arithmetical question. Gauss had shown that the accurate geometrical solution of this problem depended on the solution of certain simple binomial forms or equations. The study of such forms accordingly became of special interest: it necessitated the employment of the extended notion of number called by Gauss that of complex numbers. Now it is one of the fundamental laws in the theory of ordinary numbers that every integer can be divided only in one way into prime numbers. This law was found to break down at a certain point if complex numbers were admitted. Kummer, however, suggested that the anomaly disappeared if we introduced along with the numbers he was dealing with other numbers, which he termed ideal numbers—*i.e.*, if we considered these complex factors to be divisible into other prime factors. The law of divisibility was thus again restored to its supreme position. These abstract researches led to the introduction of a very useful conception—the conception not only of generalised numbers, but also of a system (body, corpus, or region) of numbers;[1] comprising all numbers which, by the

[1] The idea of a closed system or domain of generalised numbers has revolutionised the theory of numbers. Originally the theory of numbers meant only the theory of the common integers, excluding complex numbers. Gauss, in the introduction to the 'Disquisitiones,' limits the doctrine in this way. He excludes also the arithmetical theories which are implied in cyclotomy—*i.e.*, the theory of the division of the circle; stating at the same time that the principles of the latter depend on theories of higher arithmetic. This connection of algebraical problems with the theory of numbers became still more evident in the labours of Gauss's successors—Jacobi and Lejeune Dirichlet, and was surprising to them. "The

ordinary operations of arithmetic, can be formed out of the units or elements we start with. Thus all rational integers form a system; we can compound them, but also resolve them into their elements. Where we introduce new elements or units we only arrive at correct laws if we are careful to cover the whole field or system which is measured by the application of the fundamental operations of arithmetic. Throughout all our abstract reasoning it is the fundamental operations which remain permanent and unaltered,—a rule which,

reason for this connection is now completely cleared up. The theory of algebraical numbers and Galois's 'theory of equations' have their common root in the general theory of algebraical systems; especially the theory of the system of algebraical numbers has become at the same time the most important province of the theory of numbers. The merit of having laid down the first beginnings of this theory belongs again to Gauss. He introduced complex numbers, he formulated and solved the problem of transferring the theorems of the ordinary theory of numbers, above all, the properties of divisibility and the relation of congruence, to these complex numbers. Through the systematic and general development of this idea,—based upon the far-reaching ideas of Kummer,—Dedekind and Kronecker succeeded in establishing the modern theory of the system of algebraical numbers" (Prof. Hilbert in the preface to his "Theorie der Algebraischen Zahlkörper," 'Bericht der Math. Ver.,' vol. iv. p. 3). In the further course of his remarks Prof. Hilbert refers to the intimate connection in which this general or analytical theory of numbers stands with other regions of modern mathematical science, notably the theory of functions. "We thus see," he says, "how arithmetic, the queen of mathematical science, has conquered large domains and has assumed the leadership. That this was not done earlier and more completely, seems to me to depend on the fact that the theory of numbers has only in quite recent times arrived at maturity." He mentions the spasmodic character which even under the hands of Gauss the progress of the science exhibited, and says that this was characteristic of the infancy of the science, which has only in recent times entered on a certain and continuous development through the systematic construction of the theory in question. This systematic treatment was given for the first time in the last supplement to Dedekind's edition of Dirichlet's lectures (1894, 4th ed., p. 134). A very clear account will also be found in Prof. H. Weber's 'Lehrbuch der Algebra' (vol. ii., 1896, p. 487, &c.) He refers (p. 494) to the different treatment which the subject has received at the hands of its two principal representatives—Prof. Dedekind (1871 onwards) and Kronecker (1882)—and tries to show the connection of the two methods.

as we saw above, was vaguely foreshadowed by Peacock, and expressly placed at the head of all mathematical reasoning by Hermann Hankel. In passing it may also be observed how the notion of a system of algebraical numbers, which belong together as generated in certain defined ways, prepares us for the introduction of that general theory of groups which is destined to bring order and unity into a very large section of scattered mathematical reasoning. The great importance of this aspect is clearly and comprehensively brought out in Prof. H. Weber's Algebra. Nothing could better convince us of the great change which has come over mathematical thought in the latter half of the nineteenth century than a comparison of Prof. Weber's Algebra with standard works on this subject published a generation earlier.

63. Modern algebra.

I have shown how the definition of algebraical numbers has led to an extension and generalisation of the conception of number. Another question simultaneously presented itself, Does this extension cover the whole field of numbers as we practically use them in ordinary life? The reply is in the negative. Practice is richer than theory. Nor is it difficult to assign the reason of this. Numbering is a process carried on in practical life for two distinct purposes, which we distinguish by the terms counting and measuring. Numbering must be made subservient to the purpose of measuring. Thus difficulties arising out of this use of numbers for measuring purposes presented themselves early in the development of geometry in what are called the incommensurable quantities: taking the side of a square as ten, what is the number which measures the

64. Algebraical and transcendental numbers.

diagonal? Assume that we prolong the side of the square indefinitely, we have a clear conception of the position of the numbers 15, 20, 30, &c.; but what is the exact number corresponding to the length of the diagonal? This led to the invention of irrational numbers: it became evident that by introducing the square root of the number 2 we could accurately express the desired number by an algebraical operation. But there are other definite measurements in practical geometry which do not present themselves in the form of straight lines, such as the circumference of a circle with a given radius. Can they, like irrational quantities, be expressed by definite algebraical operations? Practice had early invented methods for finding such numbers by enclosing them within narrower and narrower limits; and an arithmetical algorithm, the decimal fraction, was invented which expressed the process in a compact and easily intelligible form. Among these decimal fractions there were those which were infinite—the first instances of infinite series—progressing by a clearly defined rule of succession of terms; others there were which did not show a rule of succession that could be easily grasped. Much time was spent in devising methods for calculating and writing down, *e.g.*, the decimals of the numbers π and e.[1]

It will be seen from this very cursory reference to the practical elements of mathematical thought how the ideas or mental factors which we deal with and

[1] The transcendent nature of the numbers e and π was first proved by Hermite and Prof. Lindemann. The proofs have been gradually simplified. A lucid statement will be found in Klein's 'Famous Problems,' p. 49 *sqq.*

string together in mathematical reasoning are derived from various and heterogeneous sources. We begin with counting, then we introduce measuring; in both cases we have definite elements or units which may serve to express order or quantity or both, and we have definite conventional operations; then we have symbols which may denote order or quantity or operation. With these devices we perform on paper certain changes, and we get accustomed to use indiscriminately these heterogeneous conceptions, arithmetical, geometrical, algebraical—nay, even dynamical, as when Newton introduced the conception of a flow or fluxion. As mathematics is an instrument for the purpose of solving practical problems, skill in alternately and promiscuously using these incongruous methods goes a very long way. Geometrical, mechanical evidence helps frequently where pure logic comes to a standstill, and pure logic must help and correct where apparent evidence might deceive us. Mathematics and science generally have always progressed by this alternate use of heterogeneous devices, and will probably always do so. The straight line of pure logic has but very meagre resources, and resourcefulness is the soul of all progress. But though this may be so in practice, there are two other interests which govern scientific reasoning. There is the love of consistency and accuracy, and of clean and transparent, as distinguished from muddled and scamped, work. The latter leads inevitably into serious errors and paradoxes, as the great mathematicians, Gauss, Cauchy, Abel, pointed out early in the century. Mathematics then frequently

65. Counting and measuring.

exhibited the slovenliness of a man who talks at the same time in more than one language, because he is too negligent to arrange his thoughts clearly. Then there come in the demands of the teacher who has to introduce abstract and difficult subjects in a clear, consistent, and simple manner, taking heed that with the elements he does not introduce the sources of future error. The same interest that led in ancient times to the composition of the Elements of Euclid has led, in the higher education of the nineteenth century, beginning with the École Polytechnique and ending with Weierstrass's famous courses of lectures at Berlin, to a revision and recasting of the whole elementary framework of mathematics. In the mean time the resourcefulness in applied mathematical thought which ever since the age of Newton has characterised the individual research of this country, has opened out new vistas and afforded much material for critical siftings and strict definitions. Both qualities were united in the great mind of Gauss with a regrettable absence of the love of teaching and the communicative faculty. Like Newton's 'Principia,' his greatest works will always remain great storehouses of thought; while his unpublished remains might be compared to the Queries appended to the 'Opticks' and to the 'Portsmouth Papers.'

Several eminent mathematicians in France, Germany, and Italy have been for many years[1] working at the

[1] The literature of this subject has been rapidly increasing since the year 1872, — the approximate date of the following publications, which created an epoch: R. Dedekind, 'Stetigkeit und irrationale Zahlen' (Braunschweig, 1872); E. Heine, "Die

clearer enunciation of the fundamental conceptions of the science, and though the ways in which they approach the subject are different, a general consensus seems to be within view as to the elementary definitions. The main difficulty lies in the introduction into pure arithmetic of the ideas which are forced upon us when

Elemente der Functionenlehre" ('Journal für Mathematik,' vol. lxxiv. p. 172, 1872). This paper refers both to Weierstrass's and Cantor's theories; H. Kossak, in the pamphlet referred to above (p. 712, note). This contains the principles of Weierstrass's theory; C. H. Méray, 'Nouveau Précis d'Analyse infinitésimale' (Paris, 1872). The first comprehensive publication of Georg Cantor belongs to the year 1883, 'Grundlagen einer allgemeinen Mannigfaltigkeitslehre' (Leipzig, Teubner). It was preceded by various articles in the 'Journal für Mathematik,' vol. lxxvii. p. 257, vol. lxxxiv. p. 82, and 'Math. Ann.,' vol. xv. p. 1, in which he had introduced and defined several of the terms and conceptions that have since become generally accepted in writings on this subject. These earlier publications, by—or referring to—the pioneers in this new province of mathematical thought, were followed by a number of further expositions by Cantor, Dedekind, and Weierstrass. The principal writings of Cantor have been republished in the 'Acta Mathematica,' vol. ii. Prof. Dedekind published in the year 1888 an important pamphlet, 'Was sind und was sollen die Zahlen,' and has incorporated many of the results of his researches in his later editions of Dirichlet's 'Lectures'; whilst the lines of reasoning peculiar to Weierstrass have become better known through the writings of his pupils and the collected edition of his mathematical works which is now in progress. A complete bibliography is given in three important articles in vol. i. of the German 'Math. Encyc.' by Profs. Schubert (p. 1, &c.), Pringsheim (p. 48, &c.), and Schönflies (p. 184, &c.) Important works, giving a summary and analysis of these various researches, now exist in the mathematical and philosophical literature of France, Germany, Italy, and England. Like the non-Euclidean geometry, the subject has attracted considerable attention also outside purely mathematical circles. Notably Cantor's writings have been exhaustively dealt with from a philosophical point of view — in Germany by Walter Brix (Wundt's 'Philosophische Studien,' vol. v. p. 632, vol. vi. pp. 104 and 261), and by B. Kerry, 'System einer Theorie der Grenz-begriffe' (Leipzig und Wien, 1890); in France by M. Louis Couturat, 'De l'Infini mathématique' (Paris, 1896); and latterly in this country by Mr Bertrand Russell, 'The Principles of Mathematics,' vol. i. (Cambridge, 1903). Italian mathematicians have also dealt largely with the subject, notably G. Peano, who published an important work, 'Arithmetices principia nova methodo exposita' (Turin, 1889).

we apply the counting process to the needs of geometry and physics. We are here confronted with notions which require to be arithmetically defined — the infinite and the continuous. The same notions at the beginning of the century attracted the attention of eminent analysts like Cauchy. It is now clear, thanks to the labours of Prof. Georg Cantor of Halle, that for mathematical purposes we must distinguish between the indefinitely great and the actually infinite in the sense of the transfinite. To deal with the actually infinite, as distinguished from the immeasurably or indefinitely great, we have to introduce new notions and a new vocabulary. For instance, in dealing with infinite aggregates, the proposition that the part is always less than the whole is not true. Infinities, indeed, differ, but not according to the idea of greater and smaller, of more or less, but according to their order, grade, or power (in German *Mächtigkeit*). Two infinities are equal, or of the same power, if we can bring them into a one-to-one correspondence. Prof. Cantor has shown that the extended range of numbers termed algebraic have the same power as the series of ordinary integers—one, two, three, &c.—because we can establish a one-to-one correspondence between the two series—*i.e.*, we can count them. He has further shown that if we suppose all numbers arranged in a straight line, then in any portion of this line, however small, there is an infinite number of points which do not belong to a countable or enumerable multitude. Thus the continuum of numerical values is not countable—it belongs to a different

66.
Georg Cantor's theory of the transfinite.

grade of infinity; it has a higher, perhaps the second, power.[1]

In all these, and in many similar investigations, a conception has gradually emerged which was foreign to older mathematics, but which plays a great and useful part in modern mathematical thought. Older mathematics, ever since the introduction of general arithmetic or algebra, centred in the conception of equality and in the solution of equations. Everything was reduced to magnitude. But there are other relations besides those of magnitude, of more or less. Often in practical pursuits, if we cannot find a counterpart or write down an exact numerical equation, we can gain information by a correspondence. This conception of correspondence plays a great part in modern mathematics. It is the fundamental notion in the science of order as distinguished from the science of magnitude. If older mathematics were mostly dominated by the needs of mensuration, modern mathematics are dominated by the conception of order and arrangement. It may be that this tendency of thought or direction of reasoning goes hand in hand with the modern discovery in physics, that the changes in nature depend not only or not so much on the quantity of mass and energy as on their distribution or arrangement.

67. Correspondence.

With these reflections we touch the limits of mathe-

[1] A summary of Prof. Cantor's work is given by Prof. Schönflies in the 'Encyklop. Math. Wiss.,' vol. i. p. 184 *sqq.* The importance of accurate definitions and distinctions regarding the infinite and the continuous is dwelt on and the different recent theories set forth in a very lucid address to the London Math. Society by Prof. Hobson, "On the Infinite and Infinitesimal in Mathematical Analysis," November 1902.

matical thought and enter the region of metaphysics. Like other lines of reasoning which have occupied us in former chapters, the exact and rigid definitions and deductions of arithmetic and geometry lead us up to that other large department of our subject—philosophic thought. Many eminent mathematicians of recent years have noticed this tendency, and have urged the mutual help which arithmetic and geometry on this side, logic and psychology on that, may derive from each other. The names of Helmholtz, Georg Cantor, and Dedekind in Germany; of M. Tannery and M. Poincaré in France; of Peano and Veronese in Italy, stand prominently forward abroad; while England can boast of having cultivated, much earlier, by the hands of De Morgan and Boole, a portion at least of this borderland, and of having in recent years taken up the subject again in an original and independent manner.[1] Cayley, in his address to the British Association in 1883, has said: "Mathematics connect themselves on the one side with common life and the physical sciences; on the other

[1] I refer to the important but unfinished works of Mr Whitehead on 'Universal Algebra' (vol. i., 1898), and of Mr Bertrand Russell on 'The Principles of Mathematics' (vol. i., 1903). I must defer a more detailed appreciation of these and other writings of this class, such as those of the late Prof. Ernst Schröder ('Algebra der Logik,' 3 vols., 1890-95) and of Prof. Gottlob Frege (see an account of his writings in the appendix to Mr Russell's 'Principles'). They belong largely to a department of philosophical thought which may be termed "the Philosophy of the Exact Sciences." This deals with two great questions—the logical foundations of scientific reasoning, and the general outcome and importance of scientific thought, not for technical purposes, but in the great edifice of human thought which we may term Philosophy. It deals with what has been called "the Creed of Science" and its value. Stanley Jevons and Prof. Karl Pearson in this country, Prof. Mach in Germany, and M. Poincaré in France, have treated the philosophy of science in one or both of these aspects.

side with philosophy in regard to our notions of space and time, and in the questions which have arisen as to the universality and necessity of the truths of mathematics and the foundation of our knowledge of them"; and he subsequently refers specially to the "notion which is really the fundamental one underlying and pervading the whole of modern analysis and geometry," meaning the complex magnitude, as deserving to be specially discussed by philosophers. Beginnings of the philosophical treatment of this and other questions indeed exist. The questions are still *sub judice*, and the historian can merely refer to their existence and importance.

There is, however, one controversy which has arisen out of these and similar speculations, and out of the desire to bring unity and consistency into the fundamental notions of elementary as well as higher mathematics, which deserves to be specially mentioned, because it occupies a prominent place in foreign literature, having given rise to a special term, and thus commanding more general attention. Prof. Klein of Göttingen, under whose master-hand many abstract and obscure subjects have become plain and transparent, has prominently brought the subject before the scientific public in a recent address.[1] I refer to the tendency represented in its extreme form by the late Prof. Kronecker of Berlin, to reduce all mathematical conceptions to the fundamental arithmetical operations with integral numbers, banishing not only all geometrical and dynamical conceptions, such as those of continuity and flow, but

68. Arithmetising tendency in mathematics.

[1] 'Ueber Arithmetisirung der Mathematik' (Göttingen, 1895).

also such apparently algebraical notions as those of irrational and complex quantities. This attempt is an outcome of the school of Weierstrass, which has done so much to banish vagueness and introduce precision into modern text-books.

Opposed to this so-called arithmetising[1] tendency is the equally emphatic view, strongly urged by the late Prof. Paul Du Bois-Reymond in his general theory of Functions, that the separation of the operations of counting and measuring is impossible, and, if it were possible (as, since the publication of his work, the fuller expositions of Kronecker and his followers have tried to show that it is), would degrade mathematics to a mere play with symbols.[2] He tries to show that such is philosophically impossible, and finds a support for his view in the historical genesis of the idea of irrational numbers in the incommensurable magnitudes of Euclid and ancient geometry. Prof. Klein in his address favours the arithmetical tendency as destined to introduce logical

[1] The term seems to have been coined by Kronecker. See Prof. Pringsheim in the 'Encyklop. Math. Wiss.,' vol. i. p. 58, note 40. Kronecker's position is set forth in Journal für Math., vol. ci. pp. 337-355, 1887.

[2] "The separation of the conception of number and of the analytical symbols from the conception of magnitude would reduce analysis to a mere formal and literal skeleton. It would degrade this science, which in truth is a natural science, although it only admits the most general properties of what we perceive into the domain of its researches ultimately to the rank of a mere play with symbols, wherein arbitrary meanings would be attached to the signs as if they were the figures on the chessboard or on playing-cards. However amusing such a play might be, nay, however useful for analytical purposes the solution would be of the problem,—to follow up the rules of the signs which emanated from the conception of magnitude into their last formal consequences,—such a literal mathematics would soon exhaust itself in fruitless efforts; whereas the science which Gauss called with so much truth the science of magnitude possesses an inexhaustible source of new material in the ever-increasing field of actual perceptions," &c., &c. ('Allgemeine Functionen-Theorie,' 1882, p. 54).

precision and consistency into the foundations of mathematics, and everywhere to further the very necessary process of critical sifting; but he denies that pure logic can do all, and points to the valuable assistance and suggestive power of geometrical construction and representation.[1] Most of my readers will no doubt agree with this view. Indeed the perusal of the foregoing chapters must have produced on their minds the conviction that, so far as the advance of science and also of mathematics is concerned, it largely depends upon the introduction of different aspects leading to different courses of reasoning. The unification of all of these into one consistent and uncontradictory scheme, though it remains a pious hope and far-off ideal, has not been the prominent work of the nineteenth century. Rather, wherever it has been attempted it has had a narrowing effect, and has resulted in a distinct curtailment of the great and increasing resources of Scientific Thought.

[1] Prof. Klein summarises the opinion which he holds as to the present task of mathematical science as follows: "Whilst I everywhere demand the fullest logical elaboration, I at the same time emphasise that *pari passu* with it the intuitive representation of the subject should be furthered in every possible manner. Mathematical developments which have their origin in intuition cannot count as a firm possession of science unless they have been reduced to a strict logical form. On the other side, the abstract statement of logical relations cannot satisfy us until their importance for every form of representation has been clearly demonstrated, so that we recognise the manifold connections in which the logical scheme stands to other departments of knowledge according to the field of application which we select. I compare mathematical science to a tree which stretches its roots ever deeper into the soil, and at the same time expands its branches freely upwards. Are we to consider the root or the branches as the more important part? The botanist will tell us that the question is wrongly put, and that the life of an organism consists in the interaction of its various parts" (*loc. cit.*, p. 91).

RETROSPECT AND PROSPECT.

In the foregoing chapters I have attempted to set forth the chief conceptions which are contained in the scientific literature of the nineteenth century. Upon these the scientific work of that period has been founded or they are the results to which its scientific reasoning has led. The most important outcome of the scientific work of the century does not lie in the region of thought, but rather in that of practical application; and this I have only incidentally referred to. Only in so far as it has reacted upon scientific thought, suggesting or modifying scientific ideas, has it been necessary to allude to it.

My readers who have so far accompanied me may be struck by one feature which, indeed, is characteristic of scientific thought. Our survey has presented such thought as broken up into a series of different aspects; and although certain connections between these aspects have been occasionally pointed out, no attempt has been made to combine them into one comprehensive or united view. The reason for this is to be found in the nature of scientific thought itself, which, proceeding by a definite method, starts from the great variety of phenomena which surround us in time and space; the only assumption

which science is obliged to make being the inevitable one that Nature is intelligible to the human mind, which is the same as saying that we must assume the existence of some kind of Order.

1. Order and Unity.

There exists, indeed, in the human mind a further demand, which may be defined by saying that the conception of order in Nature or of its intelligibility should not be held merely as a formal iteration, but should be expressed as a highest Unity by some term which conveys to our minds something more than the idea of an empty form. From this demand there have further arisen at all times various attempts to give expression to the ideas of unity, of simplicity, and of the significance of the whole scheme of existence which we call Nature. Such attempts do not form part of purely scientific thought. They are speculations for which those principles of science that are capable of exact enunciation do not suffice. They have, indeed, frequently appeared in the literature of the nineteenth century. But although there are isolated cases where scientific authorities of the first order have indulged in them, such authorities have, as a rule, shown an increasing reluctance to deal with fundamental questions or with principles which extend beyond the limits of scientific thought. We have no examples in the nineteenth century of such intellects as those of Leibniz or Newton. However different these two great thinkers of an earlier age may have been, they had this in common, that for them the scientific and the religious aspects were not only equally important, but equally occupied their attention. The characteristic difference was that Leibniz apparently strove after a

unification of scientific and religious reasoning, frequently to the disadvantage of both, whereas Newton kept them so distinctly apart that his immortal scientific works can be studied without any reference whatever to his theological writings.

The two positions represented by these two great men — namely, the attempt on the one side to unify or combine the scientific and the religious aspects, and on the other to keep them apart or contrast them — have, indeed, been adopted by many thinkers in the course of our period; but an attempt to do justice to such problems has been more usually considered the duty of philosophy *par excellence*. In the rare instances in which scientific authorities of the first order have ventured upon a solution of these problems, they have stepped outside of the limits of scientific reasoning; having, as it were, attempted to occupy the more impartial if not more elevated position of judges who assign to scientific reasoning its position and its value in the connected whole of human thought and interests.[1]

Consistently with the division of thought which underlies the present history, and which has been explained in the third part of the Introduction, I relegate the exposition of such theories to the second part of this work, which deals with philosophical thought. The fact that in the course of the nineteenth century there have still appeared scientific thinkers who have not only attacked special scientific problems, but also the great universal world-problem, may well be

2. Philosophical problems.

[1] Examples of this will be found in the writings of André Marie Ampère, of Emil Du Bois-Reymond, and of Gustav Theodor Fechner.

noted as a connection, a bond of union, between those two great realms of systematic thought which, for the sake of convenience, I have kept apart in this historical survey.

There are other features in the scientific thought of the period, as it has become known to us, which naturally lead up to a different treatment from that which is peculiar to science. In almost every instance, in following up the various aspects of scientific thought, I have had to show how they have brought us to problems which cannot be solved by the means which we call scientific or exact; and in many instances I have shown how the foremost scientific thinkers themselves have been led up to inquiries which they have variously termed philosophical, metaphysical, logical, or psychological. Such has notably been the case with the ultimate conceptions of the atomic theory, of the doctrine of energy, and, still more, with the conceptions which underlie the scientific treatment of the phenomena of life and consciousness. The further we have advanced from the simple mechanical conceptions of motion and inertia or mass, into the phenomena of the actual world of natural objects which exhibit order, development, purpose, and consciousness, the more we have been obliged to make use of terms not capable of being defined by the simple categories of exact or mathematical thought; and with whatever zeal some of the foremost thinkers have in the course of the century attempted to express these more indefinite conceptions in terms of mechanical science, they have only partially succeeded, and have certainly failed in

banishing them from the scientific vocabulary. Such conceptions have always crept in again, proving that they are indispensable even to the purely scientific comprehension or description of natural objects, or of nature as a whole.

It is not surprising, therefore, that an independent examination of the ultimate conceptions which science makes use of, or which it evolves, should have been a task which has occupied some of the greatest intellects of our period, and that the problem arising from this should form a fitting transition from the purely scientific to the philosophical portion of this history.

Now, if we try to characterise in the briefest possible manner the general problems which scientific thought as a whole has definitely formulated and placed before the philosophical thinker, there are two words which stand out prominently as indicating the two grand and complementary conceptions which either underlie all scientific inquiry or result from it. The first of these has already been stated. We saw that exact or scientific thought assumes that there exists in Nature an intelligible ORDER. The closer definition of this order in the so-called laws of the cosmos has to be ascertained by experience, and has been the subject of the foregoing narrative. The subject which remains for philosophical discussion is not any special form of order, but the fact that any kind of order exists at all, and that it is accessible to the human intellect. Clearly this is a question which affects Nature, the object, as much as the human Intellect, the subject.

But if the idea of Order underlies all scientific thought,

standing as it were at the entrance of scientific reasoning, there is another idea which stands at the end of all scientific thought. This is the idea of UNITY in its most impressive form as Individuality. It remains over as an ultimate empirical fact to which scientific reasoning advances, of equal importance with order.

3. Individuality.

These two conceptions of Order and Individuality likewise govern the two great divisions under which scientific thought has been studied by us—Physics and Biology. After reviewing in the first three chapters the characteristic attitudes taken up by the three leading nations in scientific thought, I entered upon the four abstract conceptions—namely, Attraction, Atomism, Kinetics, and Energy—which are capable of strict mathematical definition, and which form the skeleton or framework around or in which the sciences of Astronomy, Dynamics, Physics, and Chemistry have arranged their various doctrines. They serve together to define more precisely the conception of the general order of things, appropriately termed the Cosmos. In the four chapters following upon these I dealt with the different conceptions under which a comprehension, not so much of the general order as of the special events and things of our world, has been gained. These conceptions, referring to the actual forms, the history, the life and soul of things natural, have been likewise dealt with in four chapters. On them the physics of the universe and of our earth, the sciences dealing with the organised and animated creations, have been built up. Beginning with a special kind of order—namely, that indicated by external figure—these sciences

have advanced through the study of the changes of figure to an increasing appreciation of an underlying unity. In many of the organs of living creatures the unity seems to lie outside the organs themselves, as the unity of a machine which exists in the design of the maker adapting it to a certain purpose; whereas in the animated world it seems to be inside the objects of Nature. The sciences of life have accordingly forced upon us more and more the conception not only of orderly arrangement, but also of a unifying principle — that is, Individuality.

These two conceptions of Order and Individuality are as little new as are the various conceptions of purely scientific thought, most of which, as has been shown, have been handed down to us from earlier times. They have accordingly been defined and studied by philosophers from antiquity. The various positions which thinkers have taken up with regard to them during the nineteenth century have, however, been characteristic of the age, and have been very largely influenced by the conceptions of Order and Unity which science itself has elaborated. In this connection it is of importance to note that the idea of Order or arrangement has only within the nineteenth century met with a comprehensive mathematical treatment; and, so far as that of Unity is concerned, it can also be said that the mathematical sciences have in the course of the nineteenth century for the first time approached the analysis of the allied idea of Continuity, which indeed plays an increasingly important part in many scientific theories. It may even be held that the

scientific mind advances from the idea of Order or arrangement to that of Unity through the idea of Continuity.

If, however, these highest conceptions had been introduced to us by scientific thought in the form only of limiting ideas or highest abstractions, it is doubtful whether the special discussion of them would have attracted so much attention or occupied so many minds as has actually been the case. In many instances we found it to be quite sufficient for the purposes of science that fundamental principles should be dogmatically asserted, and that their usefulness should be the only proof of their correctness. If no other interest attached to the conceptions of order and unity than attaches, for instance, to the ultimate principles of dynamics, to atomism, or to the axioms of geometry, the number of persons who take up these refined studies would probably be exceedingly small. The reason why the conceptions of order, unity, and individuality have received so much attention lies in this, that they have not only a logical meaning as instruments of thought, but also, as the words themselves indicate, a practical meaning, being bound up with the highest ethical and æsthetical, as well as with our social and religious, interests. The word order means something more than arrangement when we speak of the social or moral order; the word unity is more than an arithmetical conception when we speak of the unity of action or of purpose, or the unity of design in art; the word individuality acquires a higher meaning in the term personality. Those thinkers who in the nine-

4. Practical interests attaching to Order and Unity.

teenth century, as well as in former ages, have dealt exhaustively with these the most abstract and highest conceptions of which human thought is capable, have not been, or have only very rarely been, led to their inquiries from the side of purely scientific interests; they have approached them with a full appreciation of the great moral and religious interests which lie hidden in the deeper significance which we attach to the words. In starting, therefore, on the survey of philosophical thought, it would be quite inadequate to take scientific ideas as a suitable introduction. Whatever future ages may bring, the philosophy of the nineteenth century has certainly not been exclusively, or even pre-eminently, scientific or exact. If philosophy has assumed the name of a science, it has done so in that larger sense of the word which, as we have seen, is peculiar to the German language. In this connection scientific treatment means simply methodical treatment, whereas there is an increasing tendency in many circles to identify the word science with exact mathematical or positive treatment. The exact treatment of philosophical problems, such as has been attempted but only very partially carried out in the systems of Auguste Comte in France and of Herbert Spencer in England, belongs almost entirely to a later part of that century, and forms, even then, only one side of its large philosophical literature. Philosophical thought had a brilliant history in the earlier part of the century before the ideas of Positivism or of modern Evolution were much thought of. It will therefore be necessary in any account of philosophical thought to ascertain and clearly define the positions

occupied by the great thinkers who governed and revolutionised the thought of earlier generations before the great generalisations of science, notably those connected with the ideas of energy and the theory of descent, could have had any influence whatever. Though the latter have acquired in recent times a great, perhaps an undue, importance, it will only be after becoming acquainted with an earlier and different phase of philosophic thought that we shall have once more to return to those conceptions and trains of reasoning which must be uppermost in the mind of the writer as well as of the reader of the foregoing chapters.

5. The geographical centre of philosophic thought.

But in starting on the historical account of an entirely different realm of thought, I shall not only have to ask my readers to enter into a new circle of ideas, which for a long time during the course of the nineteenth century lay entirely outside of that circle of ideas with which we have become acquainted so far; we shall be assisted also by finding an entirely different geographical centre from which these ideas emanated. It has been repeatedly pointed out that the great volume of scientific thought with which we have hitherto been occupied, emanated in the latter part of the eighteenth century from the French capital; and in the course of narration I have had to go back almost in every single instance to the foundations laid in French scientific literature. I shall now have to invite my readers to give their attention to the peculiar features which were characteristic not of French but of German literature at the end of the eighteenth and the beginning of the nineteenth century.

The centre of philosophical thought during the first half of the nineteenth century lay as much in Germany as the centre of scientific thought lay, somewhat earlier, in France. It is true that in both cases, if we trace the movement a little further back, we come upon the powerful influences of English thought. Newton can be considered as marking the beginning of the modern era of scientific thought; Locke can be looked upon as having infused into philosophic thought much of its modern spirit. But though this must be conceded to a large extent, it must also be admitted that the scientific thought of the nineteenth century for a long time received its special colouring through the influence of the French mathematicians and naturalists, with Laplace and Cuvier as their most illustrious representatives; while philosophical thought for a long time received its specific colouring from the idealistic movement which began with Kant and culminated in Hegel. And although it was again the specific influence of English thought which in the latter part of the nineteenth century diverted alike scientific and philosophical thought from the channels in which they ran during the first half of the century, we have only very partially emancipated ourselves from the overwhelming influence which the conceptions of the idealistic school of German philosophy have had upon the deeper philosophical thought of all three nations alike. The features peculiar to that period are still strongly marked on the philosophical countenance of the age: neither the lights nor the shadows thrown by the great lumin-

aries which appeared on the philosophical horizon of Germany a century ago have as yet died away.

It will be the object of the second part of this work to trace in more detail this powerful influence, to define more clearly wherein it consisted, and to discover to what extent it still survives or is mingled with other influences, among which that which we have studied exclusively in the first part of this history will prove to have been one of the most important.

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THE END.

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